A House Built on Sand: Exposing Postmodernist Myths About Science

Noretta Koertge

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A Plea for Science Studies Philip Kitcher

Philip Kitcher (Contributor Webpage)

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Abstract and Keywords

This essay contrasts two views of science that are popular in Science Studies: the realist-rationalist approach, and the socio-historical perspective, and calls for an integration of the best features of each. It discusses appeals to the theory-ladenness of observation and the undertermination of theories by evidence. It analyzes the historiographic program of relying on actor's categories.

Keywords: science studies, sociology of science, underdetermination, theory-ladenness, actor's categories, historiography

Something has gone badly wrong in contemporary science studies. [1] Some of us have spent large portions of our academic careers arguing for the importance of the critical study of science. Yet practicing scientists have not always responded favorably to those arguments. Richard Feynman's famous (perhaps apocryphal) judgment that philosophy of science is about as useful to scientists as ornithology is to birds has been quoted and echoed by Steven Weinberg, who entitles an entire chapter of *Dreams of a Final Theory* "Against Philosophy." [2] More recently, humanists and social scientists studying science have been viewed less as irrelevant dilettantes than as subversives dedicated to undermining scientific authority. The recent books by Paul Gross and Norman Levitt (*Higher Superstition*) and by Lewis Wolpert (*The Unnatural Nature of Science*) make it plain that distinguished scientists find large portions of the work done in the name of science studies ignorant, confused, and damaging. Alan Sokal's celebrated hoax reveals that the editors of one journal leaped at the chance to publish pretentious nonsense because it resonated so well with what they wanted to claim about science. When this episode is juxtaposed with the other scientific criticisms, there's an obvious temptation to generalize and dismiss the entire field as a mess. So, from my perspective, something has gone badly wrong. My aim is to try to work out just what the trouble is and how science studies might do better.

Philosophers, historians, and sociologists might turn to areas of science because they hope to illuminate issues that arise within their home disciplines. This is especially obvious in the case of philosophy, where traditional problems may be recast by drawing on concepts and results from contemporary science: a knowledge of physics may provide insights into determinism, and findings from neuroscience may shed light on topics in the philosophy of mind. [3] Scientists should hardly view this kind of research as threatening (or even misconceived), although some may entertain the fantasy that they could do it better if only they had a spare Sunday afternoon or two. More likely is the charge of "vulgar scientism" from historians, philosophers, and sociologists who resent the notion that the intellectual purity of their disciplines should be sullied by borrowing from the natural sciences. Much more controversial among **(p. 33)** scientists is the thought that the arrow of illumination can run from history, philosophy, or sociology to science, that studies of science by outsiders might identify questions and answers that the protagonists miss. I want to begin by suggesting that this contribution from science studies is not just a theoretical possibility but something that has been achieved in a significant number of recent books and articles.

Historical, philosophical, and sociological perspectives can offer (1) valuable analyses of how contemporary scientific understanding has emerged, (2) conceptual and methodological clarification, especially in areas of theoretical dispute, (3) increased awareness of the social pressures that affect certain kinds of scientific research, and (4) investigations of the impact of scientific findings on individuals and on society, which can serve as the foundations of a more rational science policy. Among the examples of contributions in all these areas, I would cite (1) historical accounts of the development of Darwinism, eugenics, molecular biology, and the character of experimental research in high-energy physics; [4] (2) philosophical work on the sociobiology debate, the IQ controversy, the units of selection controversy, the implications of Bell's theorem, and causal methodology in the social sciences; [5] (3) socio-historical research on the ways in which excluding certain kinds of people from scientific research has affected the character of the science that is done; [6] and (4) studies of the social implications of contemporary molecular genetics. [7] The work done in these and similar areas seems to me to be an important part of scientific activity and often continuous with science itself. This continuity is expressed in the fact that historians, philosophers, and sociologists frequently collaborate with scientific specialists in the pertinent fields and sometimes publish in the most respected scientific journals.

Thus, the charge that science studies is populated by people who are ignorant in the areas of science about which they pronounce—a charge frequently voiced in the wake of Sokal's hoax—is absurd. [8] To set it firmly to rest, it may help to discuss in a little more detail one exemplary study. In 1986, a historian of earth science, Martin Rudwick, wrote a long and important book about a dispute that raged in geology in the 1830s, taking his title from the name given to the dispute by those involved in it—*The Great Devonian Controversy*. [9] Using an extraordinary wealth of sources, particularly journals, letters, and field notebooks, Rudwick was able to trace in great detail the ways in which a scientific debate was resolved. His theme was that not only the particular encounters with pieces of rock in a variety of places but also the social structures of British and European science affected the process of resolution. This was a work that could have been written only by someone with a rare combination of talents, for Rudwick is not simply a historian steeped in the culture of Victorian England and nineteenth-century Europe; he used to be, in addition, a paleontologist, whose purely scientific works are still used and cited. [10] Any scientists who believe that the field is populated by ignoramuses should read Rudwick, for he is an expert on the questions whose history he discusses, an expert by any measure that critics care to propose.

The obvious response to the citation of an individual case like this is the suggestion that it is exceptional, but, in fact, although cases of double Ph.D.s are relatively rare, many people who practice the history of science, the philosophy of science, or the sociology of science have substantial training in one or more area of science. Their scientific educations may differ from those of research scientists, may be less narrowly **(p. 34)** focused and not have the depth of knowledge in any area that researchers typically have. Indeed, historians, philosophers, and sociologists of science often have a peculiar mix of scientific knowledge, comparable to that of undergraduates in some respects, to graduate students in other respects, and akin in some ways to that of research professionals in still more respects. Thus a philosopher working on the measurement problem in quantum mechanics may know as much about recent mathematical results in this area as any professional physicist but be ignorant of the details of experimental procedures that any talented undergraduate physics major can carry out with ease. Some historians of contemporary biology have a broader knowledge of this science than most professional biologists do, although they might fail undergraduate exams designed to test students' ability to recognize organismal structures or cell types. Plainly, what is important is that scholars in science studies have the information that is pertinent to their projects, and it would be folly to chide them for being unable to perform tasks that are irrelevant to the questions they are attempting to answer. [11]

At this point, critics of science studies might concede that the field they are attacking is a mixture, containing some lines of research that are genuinely informed and valuable, but that these are largely outweighed by work that is more prominent because it makes flamboyant claims on the basis of ignorant and casual reflections on some aspect of the sciences. I believe that if this were publicly acknowledged, it would be an important step in the right direction, lessening the current intensity of the "science wars," for it would start to introduce distinctions in a literature that often seems to suppose a united band of humanist thugs. My aim in the next sections is to go further along this line by tracing some of the reasons that the shape of contemporary science studies is a matter for debate and to identify some of the flaws that concerned scientists have found in the parts of science studies they most dislike.

Some Points That Ought to Be Uncontroversial

Science studies ought to respond to two clusters of phenomena. Its systematic danger is to emphasize the themes in one cluster and to slight those in the other—even though both should be uncontroversial. A helpful first step in trying to understand disagreements about the role and status of science studies is to remind ourselves of these themes.

The Realist–Rationalist Cluster

1. In the most prominent areas of science, the research is progressive, and this progressive character is manifested in increased powers of prediction and intervention.

2. Those increased powers of prediction and intervention give us the right to claim that the kinds of entities described in scientific research exist independently of our theorizing about them and that many of our descriptions are approximately correct.
3. Nonetheless, our claims are vulnerable to future refutation. We have the right to claim that our representations of nature are roughly correct while acknowledging that we may have to revise them tomorrow.
4. Typically our views in the most prominent areas of science rest upon evidence, and disputes are settled by appeal to canons of reason and evidence.

- **(p. 35)**

5. Those canons of reason and evidence also progress with time as we discover not only more about the world but also more about how to learn about the world.

In declaring that these five theses ought to be uncontroversial, I am, of course, waving a red flag at those who think some of them are false. [12] Nonetheless, items 1 through 5 are, at least superficially, accurate descriptions of aspects of science that would strike those who reflect on most areas of science and their histories, so that scholars who wish to reject them have to take on the burden of explaining why appearances are deceptive.

We don't have to probe very deeply to find out why the realist–rationalist cluster is advanced. There are striking differences between the historical development of the arts and literature and the historical development of the sciences: older scientific claims live on in textbooks; the education of scientists frequently recapitulates, to some extent, the history of the disciplines in which they are trained; and older tools and techniques, both conceptual and physical, are still used to solve research problems, often with an explicit understanding of their limitations. In some areas of science, the visual representations produced show an impressive accumulation of detail—think of models of chemical molecules, genetic maps, and delineations of the sequence of geological strata and the fossils characteristic of them. Similarly, doubting the existence of the kinds of entities discussed by scientists—remote from sensory observation though they may be—often seems as strained as querying the existence of medium-size dry goods. We think, for example, that our current abilities to manipulate organisms and to produce yeast, flies, and mice (to name three much-transformed kinds of living things) with peculiar combinations of characteristics depend on the detailed genetic maps that molecular biologists have assembled and that the pattern of successful interventions would be impossible unless there were genes and, indeed, unless our genetic maps were approximately correct. [13] (Just as we believe that it would be miraculous for millions of tourists to navigate their ways around metropolitan subway systems unless the maps posted for their instruction were approximately accurate.) [14]

Nevertheless, even though we naturally take ourselves to have the right to believe the central claims most implicated in our successful interventions in nature, it is only proper to acknowledge our own fallibility. Our predecessors often thought, quite justifiably, that there were things in heaven or earth that turned out to be beyond the credence of later natural philosophy, and so our own judgments about what there is and how it is may prove faulty in some respects. Just as we see earlier inquirers as having parts of a correct picture of the phenomena they explored, so we can anticipate that our successors will make finer discriminations than we do, that we will take our place in the historical progression of scientific views, with our own insights and our own mistakes. We cannot tell, of course, which bits of what we believe they will throw away as misguided—for if we could, we would presumably make the changes ourselves—but we should suspect that there will be such bits. For the time being, we can only express rational confidence in the whole, perhaps committing ourselves most to those parts of our current science that seem most bound up with our predictive and manipulative successes. [15]

The sorting out of what is correct, worthy of being taught and built on, and what is not appears to depend on the advancement of evidence. Scientists do experiments, **(p. 36)** make observations, and review collections of specimens; they report what they find in ways that seem governed by agreed-upon rules; and they perform mathematical analyses and develop lines of reasoning that their colleagues scrutinize. At least according to the scientific self-image, the acceptance or rejection of scientific claims—including claims about the validity of instruments, experimental techniques, and competent performance, is the result of a process subject to canons of reason and evidence. (Historically, a central task for the philosophy of science has been to identify these canons.) With the growth of science, decisions about how to assess parts of science can be improved. We have a much greater awareness of statistical inference and statistical methodology than was available a few decades ago (let alone in the nineteenth century); part of Darwin's achievement consisted in his recognizing more clearly than had his predecessors that a theory might be supported by being able to systematize a wide body of observations, even if it did not issue in concrete predictions about the future; and experimental practices in biomedical science have benefited from greater understanding of the benefits of double-blind trials and the problems of placebo effects.

Much more could be said about each of the theses in the realist–rationalist cluster, but I hope that these brief remarks will indicate some features that those who would challenge them must explain away. Let us turn to another collection of themes, equally well supported by the historical and contemporary practice of science.

The Socio-Historical Cluster

> 1. Science is done by human beings, that is, by cognitively limited beings who live in social groups with complicated structures and long histories.
> 2. No scientist ever comes to the laboratory or the field without categories and preconceptions that have been shaped by the prior history of the group to which he or she belongs.
> 3. The social structures present within science affect the ways in which research is transmitted and received, and this can have an impact on intratheoretical debates.
> 4. The social structures in which science is embedded affect the kinds of questions that are taken to be most significant and, sometimes, the answers that are proposed and accepted. [16]

Again, I shall be relatively brief in defending these themes.

Although some idealized treatments of science proceed as if inquiry were carried out by subjects who were disembodied, logically omniscient, and alone, everybody knows better. Actual investigators live significant portions of their lives outside laboratories, having social relations not only with fellow scientists but also with those who support their research or are affected by it; they have positions in a wider society; and finally, their abilities to perform logical inferences and mathematical calculations are limited and fallible. Those who want to slight the first thesis in the socio-historical cluster surely do not contest these points but, rather, deny that they have any impact on the practice of science. Each of the three following theses identifies a way in which individual and group histories and/or social roles make a difference to scientific work.

Every time a scientist makes an observation, does an experiment, or proposes a line of reasoning, he or she draws on the categories and appeals to the standards **(p. 37)** current in a particular group, usually a relatively small group of specialists interested in a technical problem. Much of what the scientist takes for granted has not been independently checked but was absorbed in a period of training, so that the work may go forward rather than recapitulating, slowly and tediously, what has been done in the past. The dependence on concepts that were introduced long ago or on established standards that scientists have not questioned for generations is most obvious when there is broad revision, old concepts are discarded, or standards are modified. [17] So thesis 2 in the socio-historical cluster ought to be accepted.

Behind thesis 3 lies the obvious thought that scientists stand in complex relations of affiliation and opposition; they cooperate with some of their fellows and compete with others. There's little doubt that alliances have played important roles in the historical development of various sciences: the debates of the late seventeenth and early eighteenth centuries between Cartesians and Newtonians showed clearly how antecedent loyalties can incline the mind to respond to some considerations and ignore others; and Darwin's own cultivation of leading figures in the British scientific establishment was surely important to his securing an audience for evolutionary ideas. Perhaps it may be thought that this type of social impact is an unfortunate distortion of science, and when scientists are behaving “properly,” they are indifferent to an argument put forward by a friend, a rival, or a detractor. Yet for reasons that ultimately stem from John Stuart Mill, we might believe that the possibility of debate among contending factions, each bound together by ties of solidarity, might contribute to the eventual articulation of superior positions, that a social system for science can take advantage of the facts of human competition and cooperation to work efficiently for the uncovery of truth. [18]

Finally, and perhaps most obviously, the kinds of problems singled out as important depend in part on the history of the field and on the wider interests of members of society. Contemporary studies of heredity suppose that some problems are especially significant—mapping and sequencing various genomes, identifying the structures and roles of particular molecules —partly because of the history of research on the large question “How are traits inherited?” which has defined the field from the beginning, partly because of what it is now possible to do, and partly because of the practical consequences of certain forms of inquiry when applied to the problems of certain kinds of societies (specifically the hope that the maps and sequences will help us address medical problems). [19] Less obviously, the practical demands and the history of research standards also help determine what will count as acceptable solutions, specifying, for example, the precision that an answer must achieve if it is to be applicable. The perennial worry voiced by some scientists about the distortion of a research agenda by practical concerns reinforces this thesis about the effects of society on science.

The challenge for science studies is to do justice to both clusters. The history of science studies and Science Studies (the capitals refer to the current and controversial work in the field) shows an initial period (up to the 1960s), during which the first cluster dominated—scientists were conceived as asocial, logically omniscient beings whose work was shaped only by what occurred in the lab. Since the 1970s, Science Studies has sometimes ignored the first cluster entirely—scientists have been conceived as brain-dead from the moment they enter the laboratory to the moment at which they **(p. 38)** leave. Curious stories are then told about the ways in which class or gender, toilet training or religious education, political disputes in the wider society, and large cultural styles determine the character of a researcher's work. Often these treatments are described with so broad a brush, connecting with the details of the scientific work at so high a level of generality—or even misunderstanding—that the research professional is easily moved to righteous indignation and, hence, some of the legitimate complaints about scientific ignorance raised by Gross, Levitt, Sokal, Wolpert, and others. [20]

Yet the realist–rationalist cluster is not always dismissed, even in works devoted to showing the subtle ways in which the themes in the socio-historical cluster play out. A study that fails to slight either cluster is Rudwick's *Great Devonian Controversy*, which I have already praised. Hence the task I have identified as central to science studies is sometimes undertaken, although I should concede that such ventures are much rarer than they ought to be.

It's precisely the overemphasis on the second cluster that provokes the critics. There's no denying that there are loony ventures styling themselves as contributions to Science Studies, that introduce fanciful pieces of terminology, play verbal games, and show an astonishing degree of incomprehension about aspects of science that high school students usually understand (the blunders are often accompanied by fervent denunciations of the evils of science). [21] In response to this is a tendency to link sophisticated scholars with interesting things to say, scholars like Helen Longino and

Steven Shapin, to much less intelligent and informed authors. At this stage of my argument, however, it's important to note that the critics are broadly right to recognize a persistent danger of overemphasizing the second cluster and ignoring the first. My next aim is to understand the reason for this lack of balance.

The Source of the Trouble

The root of the problem is some bad philosophy that has been strikingly influential in contemporary history and sociology of science (and occasionally in some contemporary philosophy of science). Several ideas have been dramatically overinterpreted, to such an extent that they give rise (as we shall see later) to the Four Dogmas of Science Studies.

The Theory Ladenness of Observation

It's been a philosophical commonplace since the early 1950s that our observations of the world presuppose concepts and categories in terms of which we make sense of the flux of experience. [22] The temptation is to claim that we thus find in nature only what we bring to it, that the world—or, at least, the only world we can meaningfully talk about—is "shaped" or "constructed" by us so that it will conform to our prior categories.

Stripping down the argument in this way makes its absurdity evident. As Thomas Kuhn (one of the early defenders of the theory ladenness of observation) clearly saw, the fact that concepts and categories are involved in observation doesn't mean **(p. 39)** that the content of experience is determined by them or that we cannot be led by experience to reconceptualize the phenomena. [23] Nor does it imply that we are somehow "cut off" from the world or that the only world we can talk about must be "constructed."

It is easy to be seduced into accepting a false picture: we imagine ourselves sitting in a cave, or behind a screen, onto which images are projected and suppose that some of the features of the images are dependent on properties of the surface. How, then, can we ever discover what the "real objects" that are the sources of these images are like? For significant periods in the history of philosophy, thinkers have been tempted by this picture, but as many critics have pointed out, it has a serious flaw. [24] In perception, we are in causal contact with physical objects, and although this contact is mediated by our *having* certain kinds of psychological states ("perceptions," “representations”), we do not perceive by *perceiving* those states. There are interesting questions for perceptual psychology about the extent to which our prior beliefs, concepts, and training influence the character of our perceptual states, and we can look to physics, physiology, and psychology to illuminate them. [25]

So it would be more accurate to say not that the world is shaped by our categories but that our representations of the world are so shaped and that the shaping is open to empirical investigation. But at this point, the champions of social constructivism will surely object that science is being “privileged,” that the defense is circular, that questions are being begged, and so forth. They are quite right to recognize that the approach I have outlined could not possibly succeed in answering a certain kind of skeptical question. If the invitation is to throw away all our beliefs, start from scratch, and justify the claim that the objects about which we form perceptual beliefs are as we represent them, then we could not offer our contemporary blend of physics, physiology, and psychology to advance the kind of picture of perception I have sketched. But neither can champions of Science Studies offer any rival picture, even one that uses screens, veils, or cave walls. Descartes launched philosophy on a quest for fundamental justification, and despite the many insights uncovered by him and his brilliant successors, we now know that the problem he posed is insoluble—just as we know that the problem of trisecting an angle with ruler and compass is insoluble and that the task of proving the consistency of arithmetic within arithmetic cannot be completed. [26] If the constructivist reminds us that we haven't shown on the basis of a set of principles that precede the deliverance of empirical science that our scientific opinions are reliable, the right response is to confess that we haven't. There is no such set of principles that will do that job, but by the same token, no set of principles will establish a constructivist picture. The only way to separate out the contributions of our histories of learning to our observations is to call on some parts of science in the way I have proposed.

Once this point is recognized, it's easy to see that the overinterpretation of the theory ladenness of observation leads to a kind of global skepticism that makes it impossible to say anything at all. If it's offered as a prelude to one of the usual claims about the role of society or social interests in the shaping of science, the enterprise will be vulnerable to the same kind of relentless request to justify categories. “You want to talk about air pumps, societies of gentlemen, vats of ferment, Renaissance courts, inscriptions. With what right do you employ these notions? Why do you tell commonsense psychological stories about the ways' in which human motivation leads to action or **(p.**

40) think that any of the macroscopic objects—including people—are as our commonsense contemporary views take them to be? No privileging!" Consistency requires constructivists to take such criticism seriously, leading them to a point at which they can say nothing.

There are interesting problems about global skepticism and more refined debates about scientific realism, and philosophical inquiry can go much further with this dialectic. [27] Yet to appreciate the muddles of one prominent line in contemporary science studies, we need go no further than this. Convinced by the idea that they can never talk about things "as they are," some practitioners effectively demand a response to the global skeptical challenge for entities they don't like (the ontologies of the sciences) and then proceed to talk quite casually and commonsensically about things they do like (people, societies, human motives). There is a name for this kind of inconsistency; it is *privileging*.

The Underdetermination of Theory by Evidence

Every scientist knows that individual experiments can be ambiguous and that, if something goes wrong, it's possible to identify alternative hypotheses as blameworthy. Pierre Duhem formulated the point at the turn of the century, insisting that hypotheses are tested in bundles, and Quine, in a much more abstract idiom, proposed that "total science" is underdetermined by all possible experience. [28] Duhem thought that the scientist's "good sense" (*bon sens*) enabled him or her to sort things out. Although Quine typically makes vague references to "an ideal organon of scientific method," his principal point seems to be a logical one: For any inconsistent set of sentences containing a self-consistent statement *S*, there is a consistent subset of the original set containing *S*, and typically there are many alternative consistent subsets of the original set. [29]

This idea has been dramatically overblown by some historians and sociologists who have contended that it shows that the world can have no bearing on what scientists accept. To see how bizarre this is, we should note that the point also seems to show that society can have no bearing on what scientists accept. But once we take seriously the notion that there's more to methodology than being consistent, it's easy to recognize that the gyrations that social constructivists envisage as available responses to experience involve epistemic costs. By analyzing major protracted scientific debates, we can see that the impact of experience is complex and subtle and that rational scientists are eventually forced out of untenable positions.

Duhem started a line of thought that enabled us to see that there is no instant rationality in science, but it's wrong to conclude from this that there are not context-independent standards of good reasoning that, when applied to increasingly comprehensive experiences, resolve scientific debates. In the early phases of the chemical revolution, phlogistonians could offer alternative analyses of the chemical reactions that Lavoisier viewed as showing the absorption or release of oxygen. As the number of findings increased, it became more and more difficult—and ultimately impossible—to find any consistent and unified way of treating all the reactions. Hypotheses to the effect that one substance was a complex compound containing phlogiston, designed to work for one reaction, broke down for others, whereas Lavoisier's proposals about **(p. 41)** constitution were largely successful. [30] There is an obvious sense in which defenders of the phlogiston theory could have gone on: they could have proposed that the composition of substances varied with the presence of some external factor or that little green people came down and added or subtracted amounts of phlogiston to make the equations balance. Nobody should doubt the logical possibility of holding on to a pet hypothesis, come what may, but what Duhem saw—with his *bon sens*—was that this circumstance does not show that these possibilities are rational.

Some workers in science studies maintain, however, that it's legitimate, even correct, to approach an episode in the history of science (or in current science) without probing the details of the experiments and the reasoning from them, precisely because we know in advance that the world can make no impact on a scientist's beliefs. [31]

The appeal to underdetermination is, once again, the reformulation of a form of skepticism—tantamount to the freshman reader of Descartes who demands to be shown that it is inconsistent to suppose that one is alone in the universe with the sensations and thoughts of the moment. Just as Dr. Johnson replied to Berkeley by kicking a stone, so the critics respond to the overinterpretation of underdetermination by citing the successes of contemporary science. This is part of a correct answer, but it needs to be supplemented with a diagnosis of the philosophical errors that have induced serious scholars to forget all about scientists' research, experiments, and reasoning and to glory in the richness of their personal and social lives—in short, to lose sight of one cluster of themes in their fascination with the other.

The Variety of Belief

There is another line of argument that sometimes leads practitioners of Science Studies to the same point. Suppose we begin from the evident fact that people, including scientists, sometimes differ in their beliefs. How can we account for this fact? Not, it is suggested, by appealing to the world, for the nature that the believers confront is the same in both instances. So the explanation of variety in belief must lie elsewhere, in the different societies that the believers inhabit.

This argument needs only a clear statement to self-destruct (or should it be "self-deconstruct"?). People with different beliefs may confront the same nature, but their relations to nature can be strikingly different. Travel, it is supposed, broadens the mind, and in the history of science, those who travel often encounter things at odds with their own beliefs and with the beliefs of those they left at home. It's not hard to explain the differences in belief between those who have ranged widely and those who have stayed at home by recognizing the variation in experience of nature. In many instances of scientific controversy, [32] something like this is occurring: one group of scientists has a wider range of experiences of nature than the other, and sometimes the ranges just are different. So the argument goes astray near the beginning in supposing that explanations that appeal to nature have to take a particular form: Scientist *X* believes that thus-and-so because thus-and-so. Once we abandon this unpromising way of explaining belief, the leap to social explanation is revealed as the extraordinary leap that it is.

We can identify a genuine insight in the posing of a problem about the variety of belief, however, if we recall the philosophical practice against which early advocates **(p. 42)** of the Strong Programme in the sociology of knowledge were reacting. From the 1930s through the 1960s, philosophers of science were fascinated with the (perfectly legitimate) problem of understanding the justification of scientific beliefs, and they focused on true beliefs. Prevailing pictures of justification tended to identify relatively simple forms of inference that made it puzzling how any rational person could ever have opposed the great achievements in the history of science. The salutary point made by the rebellious sociologists emphasized the natural rationality of members of our species and made it particularly hard to conceive of the intelligent participants in protracted scientific debates as bigoted, prejudiced, or irrational. What was missing in this entire opposition was a clear conception of how intricate and difficult reasoning in complex scientific contexts often is. In the debate between Lavoisier and his opponents, there are no simple rules of instant rationality, and a careful philosophical reconstruction can explain how reasonable people can disagree for a very long time and yet, ultimately find themselves compelled by the evidence to reach consensus. [33]

Once this is recognized, we can identify the motivation for and the overdevelopment of one of the great shibboleths of much work in Science Studies, the principle of symmetry. In the early 1970s, David Bloor famously proposed that explanations of true and false belief should be “symmetrical” that is, they should appeal to the same kinds of causes. There is an important insight here: human beings have broadly similar capacities, live in broadly similar ways, and the large-scale physiological, physical, and psychological determinants of their beliefs are the same. We don't usually explain a scientist's belief by attributing to him or her some special faculty that that person alone possesses—although we should note that on some occasions we do appeal to the fact that someone has an ordinary capacity developed to a high degree in some particular direction. [34] Such an appeal is quite compatible with the recognition that there are serious and important differences in the processes by which people form their beliefs: Terrie the traveler differs from stay-at-home Sam because Terrie has seen things that Sam hasn't. Even though their different beliefs have much in common (perception plays an important role for both), the details are different (they've had different opportunities for perception). Sometimes, we're rightly prepared to make judgments about the quality of the processes through which beliefs have been formed. If three students are supposed to use the data to compute the chance that a patient has a particular disease, we commend the first for an impeccable Bayesian analysis, correct the errors of a second who neglects the base rate, and are aghast at the performance of a third who simply mixes guesswork with an appeal to the gambler's fallacy. Of course, all three students' beliefs are generated by “the same types” of causes (all engage in computational processes), but the three are importantly different and differ in their degrees of reliability.

Neglect of these simple points leads to some curiosities of Science Studies discussions. Rudwick's study of the resolution of the “Great Devonian Controversy” was criticized for treating some of the actors “asymmetrically.” [35] According to Rudwick's narrative, the community of geologists eventually came to agree on a view of the ordering of strata, although two figures continued to hold out for different conclusions. These two figures were an interesting mixture of the cases considered in the previous paragraph: both had far more limited experience than the large majority who achieved

(p. 43) consensus, and both defended their beliefs by processes that were far less reliable. To chide Rudwick for failing to treat the “outliers” symmetrically is the same plea for phony equality that one might make in querying the judgment about a race. Down by the track are the official judges, with eyesight regularly tested, the best auxiliary equipment, and ample experience; all of them agree about the result. Up in the stands are two spectators. One of them has a partially blocked view, and the other has mislaid his spectacles; neither has ever judged a race before. Each issues a verdict at odds with the judges' consensus—and with the other, but we must be symmetrical, we must not privilege. In the delightful epigram of a fine logician, we must be so open-minded that our brains fall out. [36]

“Actors' Categories” and the Writing of History

One last muddle dominates much of the work in contemporary Science Studies. Just as sociologists of science abase themselves before the shibboleth of symmetry, so historians insist that narratives must be constructed in terms of “actor's categories”: in telling the story of a scientific development, we must not employ concepts that were not available to the people involved.

The emphasis on actors' categories has a serious point. If a historian is able to make vivid the ways in which a group of past scientists represented the world around them, then it is possible to appreciate the course of their inquiries as they experienced them, and this serves an important explanatory purpose. Rudwick's account of the “Great Devonian Controversy” provides us with the participants' perspectives so that as we follow their investigations, we feel their surprises and see the lure of approaches that, some pages later, turn out to be fruitless. Yet it would be wrong to think that this is the only explanatory role that history should serve or that appeals to what we now accept are always out of place. Historians of mathematics have often found it illuminating to cite Frobenius's proof that there exist exactly three associative division algebras over the reals in explaining why Hamilton's inquiries into higher-dimensional analogues of the complex numbers broke down where they did. [37] Any such account will not help us see the inquiry as Hamilton saw it, but it will enable us to understand just why he faced the problems he did at various stages. Later knowledge can be employed in history to fulfill an explanatory function, different from that of immersing us in the world of the protagonists. [38]

Purists may worry about using any findings from modern science in understanding the past. As in other instances, purism leads quickly to absurdity. Should the military historian studying trench warfare between 1914 and 1918 abstain from drawing on a technical understanding of the effects of shell impacts on the landscape, of the spread of infectious disease, of the psychological consequences of life in the trenches—an understanding that may have been produced by reflecting on the events chronicled? No matter how resolute we may be in seeking actors' categories, any account of past people will involve assumptions about motivation and action, the character of the public world and human responses to it, and we rightly make those assumptions using the best information we have. [39] Once we recognize that trying to suspend some current beliefs can be valuable in giving us insight into the situations **(p. 44)** as they appeared to the participants and that not suspending those beliefs can be important in leading us to recognize ("from the outside") their problems and successes, we can give the historians' totem its precise due.

To hammer home the point, let me offer one last example. We know very well that Europe suffered from outbreaks of bubonic plague during the late Middle Ages, and modern science gives us an account of how the bubonic plague was spread. It's easy to recognize the legitimacy of two quite different styles of history of the plague years. One offers us the perspectives of the actors, uses their categories, and presents us with the options and difficulties as they saw them. The other draws on contemporary epidemiology to explain why the plague broke out where it did, why various strategies against it were ineffective, how some people who survived were enabled to do so, and so forth. Histories of both types can be genuinely illuminating, and the second should not be ruled out of court by a priori prejudices.

Many critics of Science Studies recognize the relativism that often runs rampant. In identifying four routes that begin from sensible starting points, I hope to have shown that the road to relativism is paved with the best of intentions and the worst of arguments. So practitioners come to inscribe on their hearts the Four Dogmas: (1) There is no truth save social acceptance; (2) no system of belief is constrained by reason or reality, and no system of beliefs is privileged; (3) there shall be no asymmetries in explanation of truth or falsehood, society or nature; and (4) honor must always be given to the "actors' categories."

It would be wrong, however, to leave the diagnosis of the malaise of contemporary Science Studies at just this point. When the Four Dogmas have been thoroughly absorbed, so that younger scholars start from their conclusions as if they were gospel, then enterprises of real peculiarity can be launched. [40] How can Science Studies be liberated from the asymmetrical treatment of society and nature achieved in the early phase of the sociology of scientific knowledge? [41] How can the lessons of Science Studies be applied to Science Studies itself? Wait! There are new fashions announced in Gallic haute couture. [42] Let us mix in some Lacan, some Lyotard, a dash of Deleuze. Let us play with Derrida. [43] Let us have actor networks, mangles of practice, emergent dialectical surfaces, multivocalized polygendered postphallogocentric transcategorially sensitive discourses. … Let us have solutions to problems that nobody has ever thought of posing about science before; indeed, let us forget about science entirely in our de-privileging of canonical texts and our elevations of context. Like Lear on the heath, "We shall do such things—What they are yet I know not—but they shall be / The terror of the earth." [44]

I exaggerate, of course, but only a bit. The thoughtful reader, taking up a book such as Latour's *We Have Never Been Modern* or Pickering's *The Mangle of Practice*, can only wonder at the height to which the seas of Science Studies have risen. Science seems no longer to be the principal subject (pride of place now being given to Science Studies itself); instead, we have entered a discourse as closed off from the phenomena that were once central to the field as some philosophical investigations of the 1950s with their exclusive obsessions with the blackness of ravens. Dimly, one sees that rival perspectives are being pitted against one another, but the exact character of the positions and the standards to which they are to be accountable are completely obscure. In the end, one can only ask, "If these are the answers, what, please, are the questions?" [45]

(p. 45) This is, of course, the point at which critics of Science Studies, both inside and outside science studies, should cry "Enough!" Just as the protagonists think that there is a seamless line of reasoning that leads them to their conclusions, their opponents buy into the same assumption and suppose that the entire enterprise was rotten from the beginning. [46] I share their impatience with the later stages of the project—the automatic assumption that the Four Dogmas are sound and that one must therefore undertake the projects I have parodied—but by trying to expose the exact points at which insight gives way to overinterpretation, I hope to prepare the way for a more sympathetic view of science studies, one that will not only offer a different picture of the sciences but also show how some pieces of scholarship that are icons in contemporary Science Studies might be put to better use.

The Real Challenges

The most general challenge today is to do justice to both clusters of themes. This task is not impossible, and in recent years, several books have appeared that, in different and occasionally incompatible ways, attempt to mix historical, philosophical, and sociological insights about science. Like Rudwick's study of the history of geology, Peter Galison's *How Experiments End* is a thorough investigation of historical episodes (this time in the context of twentieth-century physics), revealing the multiple constraints that operate in everyday experimental practice. Ronald Giere's *Explaining Science* links philosophical accounts of scientific reasoning to models of human cognition and takes some steps toward embedding human knowledge in a social matrix. The details of social exchange within a scientific group (the systematists who embrace “pattern cladism”) are probed in David Hull's *Science as a Process*, and Hull shows clearly how a relentless concern for prestige can give rise to progressive conceptual evolution. From a more abstract perspective, in *Science as Social Knowledge*, Helen Longino explores the conditions for a well-ordered scientific community and argues that societal values play important roles in scientific decisions. John Dupre's *The Disorder of Things* sounds a similar theme about the relation between science and broader values as well as arguing for important differences and disconnections among the various sciences. Finally, in *The Advancement of Science*, I try to show the intricacy of the reasoning processes that figure in major scientific debates and to construct a formal framework for understanding how various kinds of social institutions, social relationships, and personal aspirations can play a positive role in the genesis of new knowledge. [47] Perhaps immodestly, I would like to see these works as grabbing hold of different pieces of the same (important!) elephant.

The books I have mentioned address two major groups of issues, in incomplete and inadequate ways. The first concerns the relation between the practice of science and the values of the broader society; the second focuses on the ways in which social relations and structures of various types figure in the doing of science. What kinds of value judgments enter into scientific decision making, and exactly where do they enter? Just in the funding agency? Just at the stage when research is being designed? At the point when conclusions are being reached? When those conclusions are disseminated? Or at all these points and more? Is there a tension between epistemic and **(p. 46)** other values, and if so, how should we think about this tension and its resolution? [48] How do such phenomena as reputation, lines of affiliation, competition for resources, and need for cooperation on large-scale projects affect the ways in which scientific questions are pursued and the answers that are accepted? What are the contemporary social institutions that shape scientific research, and are they well designed for the advancement of knowledge? Plainly, the two clusters of questions are intertwined, and it is hard to conceive of answering them independently of each other.

It should also be plain that these questions are important. Reflective scientists want to understand the ways in which existing arrangements foreclose certain kinds of opportunities. (Why should social systems that have evolved from the seventeenth century be expected to be particularly good at fostering contemporary scientific research?) Reflective people (whether scientists or not) want to know whether research in various areas is skewed by the values of particular groups and, at the broadest level, how science bears on human flourishing. A large part of the motivation for many scholars who enter science studies is to try to articulate ways in which science can be used for human good. Virtually all traditional philosophy of science ignores that motivation. A sad irony of contemporary Science Studies is that even though it may seem more responsive to broader concerns, its espousal of the Four Dogmas undercuts them.

Suppose that you are worried about the impact of scientific discoveries on human well-being. An immediate corollary is that no general picture that endorses a global skepticism about scientific achievement can be satisfactory. [49] For if we are led into blanket constructivism, rejection of notions of reason, evidence, and truth, then there is a terrible irony. The last thing that political liberals want to say about the excesses of pop sociobiology or *The Bell Curve* is that these ventures are just like the social constructions of Darwin and Einstein [50] or that because talk of reason is passé, there's no less reason to believe claims about the genetic determination of criminal behavior than to endorse the double-helical model of DNA. We need the categories of reason, truth, and progress if we are to sort out valuable science from insidious imitations.

It has been obvious for about half a century that research yielding epistemic benefits may have damaging consequences for either individuals or even the entire species. Philosophical stories about science have been narrowly focused on the epistemic. Faced with lines of research that have the capacity to alter the environment in radical ways, to transform our self-understanding, and to interact with a variety of social institutions and social prejudices to affect human lives, there is a much larger problem of understanding just how the sciences bear on human flourishing. There seems to be a strand in contemporary Science Studies that responds to this problem by trotting out every argument (however bad) that can be interpreted as debunking the sciences—as if its proponents were frightened of a monster and had resolved to cure their terror by insisting on its unreality. [51] Any such strategy is not only inaccurate but also politically jejune. Only by careful analysis of science and its relations to a wide range of human concerns—indeed, only by analysis that comes to terms with the themes in the two clusters—can we hope to start a public dialogue that can be expected to produce a "science for human use." [52]

If Gross and Levitt are correct to think that one of the motivations behind Science Studies is to make the world safe for humane concerns, then the Four Dogmas are a **(p. 47)** terrible bar to insight into serious issues. Not surprisingly, the contribution of Science Studies to exploring one set of questions that philosophers have neglected—the questions about values—has thus been limited. Yet given the pronounced emphasis on the social, one might think that recent work in Science Studies would have at least supplied tools for addressing the second, the issues concerning the ways in which social structures shape research. Any such hope is doomed to disappointment.

It is time for confession. In constructing a general approach [53] to the question "Do the structures of science interact with individual motivations to promote the reliability of collective learning?" I had to make up (guess) a lot of my own sociology. This was not negligence. There was no theoretical source to which I could turn for guidance about the character of the causal processes that affect research. To be sure, there are "case studies," investigations that deploy "folk" categories, like the ones I employ, but there is no systematic body of theory that would identify major causal factors —such as one might obtain from a sociologist of criminal behavior if one were interested in the social causes of crime. I suspect that David Hull, investigating his warring systematists, also had to go it alone, doing his illuminating "natural history of a scientific community" without benefit of guidance from theoretical sociology.

Sociologists of science sometimes offer interesting studies of historical or contemporary groups that deploy commonsense ideas about social interactions and individual interests: Shapin and Schaffer's study of the Hobbes–Boyle controversy is a case in point. I shall not reiterate the criticisms offered by others (or by myself on other occasions) but recognize —as indeed Gross and Levitt seem to do—the fine detail about the political disputes in which Boyle and Hobbes were embroiled. Yet like Hull and me, Shapin and Schaffer do not draw on any antecedent general view of social causation. Their views about what is important to people involved in political debates are entirely sensible—and entirely atheoretical.

An earlier generation of sociologists of science conceived their subject differently. Robert Merton and his successors (now typically—and unfairly —scorned by Science Studies) wanted to try to understand the causal processes in scientific communities; they hoped to do for science what other sociologists (then and since) have done for other areas of human life. Contemporary sociology has well-developed subdisciplines that study religious affiliation and organization, crime and socially deviant behavior, and so forth. Moreover, we expect that a sociologist in one of these areas will be able to advance our understanding of important phenomena, shedding light, for example, on how crime rates may be expected to increase or decrease with age distributions or economic trends. We anticipate that the sociologist will offer a causal model, identifying some factors as relevant and taking account of their interactions, and because the phenomena are complex, we may be prepared to tolerate only limited accuracy. Mertonian sociology envisaged advancing this kind of analysis for social phenomena in general and also for the parallel problems that arise with respect to science.

So, I suggest, contemporary science studies faces two large and important problems. Because of its adherence to the Four Dogmas (and its repudiation of connections to other parts of sociology), Science Studies fails to answer those problems. Nevertheless, I now want to suggest that certain contributions to Science Studies, including some that have been vigorously criticized, could prove genuinely useful in responding to the real challenges that confront us.

(p. 48) Beyond the “Science Wars”

So far, my discussion has been largely critical. Although I have defended science studies, my main aim has been to identify where Science Studies has gone wrong and how it leaves the most important issues unaddressed.

But as I have noted in passing, I believe that there are valuable insights in works that have become icons of the field, even though those insights are compromised by argumentative overextensions. It is time to justify these remarks.

Plainly, I believe that critics of Science Studies such as Gross, Levitt, Sokal, and Wolpert have identified shortcomings in some contemporary discussions of science. Reactions to their criticisms, especially to *Higher Superstition*, have been intense: many workers in Science Studies (including those whose works have been attacked and those whose works have not) find the book ignorant and opinionated (to put their responses in relatively mild language). When pressed to elaborate, they typically complain that Gross and Levitt and other critics do not draw distinctions, that they treat peripheral people and central workers in the field as if they were minor variants of one another, tarring the latter with the sins of the former. A closer look at the criticisms reveals that this complaint is not, strictly speaking, accurate. *Higher Superstition* takes some pains to recognize the differences in the quality of individual work: Shapin and Schaffer, for example, are praised for producing a book that is "exhaustively and meticulously researched" (Gross and Levitt, 1994, p. 68) and for raising "serious and genuine" questions (65); Helen Longino and Evelyn Fox Keller are described as "anything but inept" (136) and are explicitly contrasted with cruder feminist writers (notably Sandra Harding). Yet I think that, at a deeper level, the defenders of Science Studies are correct in believing that the critics press good points too far. Just as Science Studies has overextended genuine insights to fashion the Four Dogmas, so do critics like Gross and Levitt want to tell a simple story, one that will attribute the same deep motivation to all those whose works they address.

The critics approach Science Studies as if it were driven by a common ideology that aims to "demystify science, to undermine its epistemic authority, and to valorize" ways of knowing "incompatible with it" (Gross and Levitt, 1994, p. 11). There is no doubt that some of the targets of the criticism do subscribe to this ideology and that even some of those who are (rightly) viewed as central to Science Studies do so: Sandra Harding announces that we need something different from the sciences as traditionally practiced, that we need instead "sciences and technologies that are *for* women and that are for women in *every class, race, and culture*." [54] Harding is a perfectly good "type specimen" of the views that Gross and Levitt want to oppose. The trouble with their treatment of other workers in Science Studies—other feminists like Longino and Keller and non-feminists like Shapin and Schaffer—is that they are seen as variants of this same general type. Longino and Keller, we are told, also want to "defend ideology in the academy" (Gross and Levitt, 1994, p. 136); Shapin and Schaffer stick up for the "voiceless and excluded masses" against "snobbish, purse-proud, rank-conscious plutocrats" (69). All discussions in Science Studies are thus heard against the accompaniment of the most strident voices, and it is thus hardly surprising that critics can find no value in them.

This strikes me as terribly wrong. Thinking of Keller, [55] Longino, and Shapin and Schaffer as belonging to the same intellectual species as Harding is a bit like thinking **(p. 49)** of gibbons, chimpanzees, seals, and dolphins as being conspecific with opossums (they all are mammals, of course, but there the similarities end). Shapin and Schaffer want to understand an important episode in the birth of modern science, and even though they may have gone awry because of their fascination with Dogma 2, it would be uncharitable not to see that their work corrects some of the overrationalistic tendencies of earlier accounts and that it may point to a future story that does justice to both clusters of ideas. Longino's *Science as Social Knowledge* is notable for two main themes: first, that objectivity is a social notion and that to claim that a belief is objective is to maintain that it has emerged from a process of critical discussion in a society with particular features (especially a tradition of scrutiny from alternative perspectives, to which all members of the society have access); and second, that the values of particular subgroups in society have affected scientific research at a number of different levels, including the choice of what Longino calls "global assumptions." [56] Longino's two themes combine in her call for detailed scrutiny of the ways in which the scientific research actually carried out is partial, reflecting only the values and concerns of certain groups within the broader society, and in her vision of a relationship between science and society that is more democratic and open. This is hardly an attempt to enthrone ideology in the academy.

A principal problem with the assumption that all contributors to Science Studies are really variations on Sandra Harding is that it forecloses the possibility that some of them might offer valuable insights into pursuing the projects outlined in the previous section. If we are to understand the complexities of the relationship between science and social institutions, we will need rich descriptions of particular instances, and some parts of the sociology of science (as currently pursued) as well as the style of history that Shapin and Schaffer exemplify may aid our attempts to paint a more general picture. Similarly, Longino's thoughtful discussions of the ways in which values may surface in scientific research can help us formulate questions that have been too long neglected in studies of science. Once we see the importance of accommodating two sets of themes, the realist–rationalist cluster and the socio-historical cluster, it's clear that even works trumpeting the hegemony of the social can serve as parts of an eventual synthesis. Here, perhaps, is one place where there's a good argument for symmetry: just as philosophers of science would not want to dismiss traditional studies as devoid of insight (even though they were oblivious to the themes of the socio-historical cluster), so too we can hope to free the more penetrating achievements of Science Studies from the unfortunate influence of the Four Dogmas. Impassioned critiques of Science Studies, viewed as a monolithic ideology, endanger this important possibility. The critics seem to yearn to turn the clock back, to revive a world in which only the "friendly" themes of the rationalist–realist cluster are bruited and in which outsiders sing only happy songs around the scientists' campfires.

A View from the Marginalized Middle

I doubt that this essay will please anyone, for it attempts to occupy middle ground, and the heat of many of the exchanges of recent years make it plain that the middle is an uncomfortable place to be. Some of my scientist friends echo Cato, convinced that **(p. 50)** the destruction of science studies is the only remedy and inviting me to join them in a dance on the charred remains. Colleagues in Science Studies view as an act of betrayal any suggestion that the discussions in the field have identifiable shortcomings. So the middle is thoroughly marginalized, and those of us who occupy it have been moved, again and again, to repeat Mercutio's most famous expostulation. [57]

I have written this essay in the hope that, within science studies at least, we can transcend the culture wars and use the debate to fashion more productive approaches to important issues. It is hard to be optimistic. The trenches have been dug deeply, and the fire shows little signs of stopping. Even the title of this book reveals an important lack of mutual understanding. Whatever its faults, Science Studies is not "a house built on sand." It is better conceived as a colony strung out on a difficult, but strategically important, seashore. Some of the buildings—gross and gaudy in self-advertisement—stand on pathetically slender foundations; they hardly need a tsunami to wash them away, the merest ripple will do. Others are a curious mixture of craftsmanlike work and jerry-building, often with a folly or a vast, unscoured stable attached. A few buildings, more modest, sneered at or ignored by the most ambitious architects, are constructed to last. Perhaps if this image is accepted, we can begin to see that we should neither announce utopia nor call for the bulldozers. What is needed is slum clearance and urban renewal, a project in which historians, philosophers, sociologists, and scientists all should all be invited to join.

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(1.) Here and throughout, I use “science studies” to refer to the field of the study of science by nonscientists, paradigmatically historians, philosophers, and sociologists; and “Science Studies” to refer to particular views about that field, specifically the grab bag of doctrines that have drawn the wrath of scientific critics. Capitalization of the title sentence is left as an exercise for the reader.

(2.) For a penetrating response to Weinberg, see Wesley Salmon's lucid essay “Dreams of a Famous Physicist,” forthcoming in a collection of his essays to be published by Oxford University Press. In fairness to Weinberg, I should note that he does acknowledge that his chapter title is an overstatement.

(3.) These are only two among many obvious examples. For paradigms of this kind of philosophical work, see John Earman, *A Primer on Determinism* (Dordrecht: Kluwer, 1986); and Patricia Smith Churchland, *Neurophilosophy* (Cambridge, Mass.: MIT Press, 1985).

(4.) See, for example, Ernst Mayr and William Provine, eds., *The Evolutionary Synthesis* (Cambridge, Mass.: Harvard University Press, 1980), which is typical of much work on evolution after Darwin in its collaboration between historians of science and leading evolutionary biologists. For eugenics, see Daniel Kevles, *In the Name of Eugenics* (London: Penguin, 1987); for the first decades of molecular biology, see Horace Freeland Judson's magisterial *The Eighth Day of Creation* (New York: Simon & Schuster, 1979); and for experiments in twentieth-century physics, see Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987).

(5.) For the IQ controversy, see Ned Block and Gerald Dworkin's anthology *The IQ Controversy* (New York: Pantheon, 1974), especially the long essay by the editors. Block also wrote, in *Cognition* (1995), the single best diagnosis of the flaws of Richard Herrnstein and Charles Murray's *The Bell*

Curve. Pioneering work on the units of selection controversy has been done by David Hull, William Wimsatt, and Elliott Sober; see, in particular, Sober's *The Nature of Selection* (Chicago: University of Chicago Press, 1992). The implications of Bell's theorem have been explored by numerous contemporary philosophers of science, including Bas van Fraassen, Abner Shimony, Arthur Fine, Jon Jarrett, and Geoffrey Hellman. Many of the most important essays on quantum mechanics by philosophers have appeared in *Physics Review Letters*; and for two recent studies that add new dimensions to the discussion of issues in the foundations of quantum mechanics, see David Albert's *Quantum Mechanics and Experience* (Cambridge, Mass.: Harvard University Press, 1992); and Tim Maudlin's *Quantum Non-Locality and Relativity* (Oxford: Blackwell, 1994). Groundbreaking work on causal modeling in the social sciences has been done by Clark Glymour, Peter Spirtes, Richard Scheines, and Kevin Kelly. See Glymour et al.'s *Discovering Causal Structure*. On the sociobiology debate, see Michael Ruse, *Sociobiology: Sense or Nonsense* (Dordrecht: Reidel, 1979); and my own *Vaulting Ambition: Sociobiology and the Quest for Human Nature* (Cambridge, Mass.: MIT Press, 1985).

(6.) See Londa Schiebinger, *The Mind Has No Sex?* (Cambridge, Mass.: Harvard University Press, 1989); and Kenneth Manning, *Black Apollo of Science* (New York: Oxford University Press, 1983). Paul R. Gross and Norman Levitt offer a somewhat condescending evaluation of Manning's work in their *Higher Superstition* (Baltimore: Johns Hopkins University Press, 1994), 285, n. 62), without offering any detailed criticism.

(7.) For example, see Troy Duster, *Backdoor to Eugenics* (New York: Routledge, 1990); Dorothy Nelkin and Laurence Tancredi, *Dangerous Diagnostics*, 2d ed. (Chicago: University of Chicago Press, 1994); Daniel Kevles and Leroy Hood, eds., *The Code of Codes* (Cambridge, Mass.: Harvard University Press, 1992); and my own *The Lives to Come: The Genetic Revolution and Human Possibilities* (New York: Simon & Schuster, 1996).

(8.) Although Gross and Levitt are also often read as making this charge, this seems to be more a matter of the tone of *Higher Superstition* than of what they say by way of criticizing prominent figures in Science Studies. True, they rightly take Bruno Latour and Sandra Harding to task for their ignorance on various technical matters (matters that are directly relevant to the topics they discuss), but Gross and Levitt explicitly note technical competence in other cases. For example, they praise Harmke Kamminga's exposition of chaos theory (95), single out Scott Gilbert's “distinguished” exposition of developmental biology (117, 121), and remark on Evelyn Fox Keller's extensive training in science (140).

Here and throughout this chapter, parenthetical page references to Gross and Levitt are to *Higher Superstition*. I also doubt that Sokal would accuse all practitioners of Science Studies of scientific illiteracy. Yet the myth is now widespread among scientists, and it needs to be debunked.

(9.) Published by the University of Chicago Press in 1986. When four societies in science studies (the History of Science Society, the Society for the Social Study of Science, the Society for the History of Technology, and the Philosophy of Science Association) met together that year (the only occasion on which all four have ever met together), Rudwick's book was the focus of a unique multidisciplinary symposium in which scholars from all four societies commented on it.

(10.) Stephen Jay Gould pays tribute to Rudwick's paleontological work in his review of *The Great Devonian Controversy* (see *An Urchin in the Storm* [New York: Norton, 1987], 78), but perhaps the real compliment is the fact that Rudwick's work on brachiopods is cited as an important illustration in one of the most widely read essays in recent evolutionary theory, Gould and Lewontin's “The Spandrels of San Marco and the Panglossian Paradigm: ACritique of the Adaptationist Programme,” *Proceedings of the Royal Society of London* B 205 (1979): 581–98.

(11.) Of course, scholars in science studies sometimes fail to recognize that particular pieces of scientific information would be useful to them, and professional researchers can play a valuable critical role in pointing this out. This is quite a different objection, however, from the charge that science studies is filled with ignorant dilettantes.

(12.) Here I should acknowledge that some of these theses are matters of sophisticated philosophical debate and that some philosophers have offered serious challenges to straightforward readings of them. In particular, Hilary Putnam, Arthur Fine, Nelson Goodman, and Richard Rorty all would object to the most obvious interpretation of 2. It seems to me important to separate philosophical worries about the understanding of the relation of thought and language to reality from the much cruder suggestions put forward in most of contemporary Science Studies, and also to recognize that for Putnam and Fine at least, there is a sense in which 2 can be construed to be true. (This may also hold for Goodman and Rorty, although here I am less confident.) The major objections to 2 that condemn strong versions of realism is that philosophers have added metaphysical encumbrances to the ordinary practices of describing the accomplishments of the sciences, not that the practice of identifying these accomplishments ought to be radically revised. This is clearest in Fine's commendation of “the natural ontological attitude” see *The Shaky Game* (Chicago: University of Chicago Press, 1986).

(13.) Of course, some philosophers would demur. See Bas van Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980). Van Fraassen's views have been subject to extensive discussion and criticism. See, for example, P. Churchland and C. Hooker, eds., *Images of Science* (Chicago: University of Chicago Press, 1984).

(14.) Here it's important to recognize that in both instances, the appropriate notion of accuracy depends on conventions of map reading. One should not infer from standard genetic maps that chromosomes are beautifully straight, any more than from the familiar map of the London Underground that the directional relationships among various stations are precise.

(15.) *Seem* is the right word here, for our knowledge of what parts of our beliefs genuinely do work for us is itself partial.

(16.) It's worth noting that Gross and Levitt seem to acknowledge this point (139), although they seem mostly unwilling to accept the idea that historical, philosophical, or sociological investigations might reveal in detail how it applies to particular scientific fields (one exception is their praise for some of Stephen Jay Gould's historical studies).

(17.) The *locus classicus* for this theme is obviously Thomas Kuhn's *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962; expanded 2d ed., 1970). The theme essentially develops Kuhn's account of “normal science,” although as I have suggested, the characteristics of normal science may best be appreciated by looking at those convulsive changes that Kuhn calls “revolutions.” It is important to note, however, that everything I have said can be accepted without endorsing Kuhn's account of scientific revolutions. There is no need to suppose that the large changes involve “conversion experiences.” For my own attempt to develop the sociohistorical themes without the nonrationalist elements that sometimes surface in Kuhn's writings, see my *The Advancement of Science* (New York: Oxford University Press, 1993).

(18.) See J. S. Mill, *On Liberty*, chap. 2. Paul Feyerabend has urged that science should always find a place for the voicing of heretical views (see his

Against Method [London: Verso, 1975]), and Elisabeth Lloyd argues that his defenses of heterodoxies are designed to exemplify the strategy of playing the devil's advocate, whose participation Mill saw as so important. From a quite different direction, I have argued that there is no reason to think that social institutions that take advantage of our rivalries and loyalties are necessarily opposed to theadvancement of knowledge. See my “The Division of Cognitive Labor,” *Journal of Philosophy* 87 (1990): 5–22; and chap. 8 of *The Advancement of Science*.

(19.) Of course, critics claim that the optimism is ill based. See Richard Lewontin, “The Dream of the Human Genome,” in his *Biology as Ideology* (New York: Harper, 1992). I try to arrive at a realistic assessment in chaps. 4 and 5 of *The Lives to Come*.

(20.) I should note that many of the most egregious examples cited by Gross and Levitt are by people whom practitioners of Science Studies would not view as central to the field. As one who has attended numerous fora in Science Studies during the past decade, I have never heard of Steven Best, Katherine Hayles, Maryanne Campbell, or Morris Berman. I don't think that I have heard presentations that cite Stanley Aronowitz or Jeremy Rifkin and only a couple that allude to Carolyn Merchant. By contrast, the following figures are omnipresent in the presentations and discussions: Bruno Latour, Donna Haraway, Steven Shapin, Simon Schaffer, Helen Longino, Evelyn Fox Keller, and Sandra Harding (all of whom are discussed by Gross and Levitt) as well as Harry Collins, Peter Galison, Lorraine Daston, Paul Forman, Norton Wise, Trevor Pinch, Michael Lynch, Andrew Pickering, and Ian Hacking (none of whom receives a mention).

(21.) Many, though not all, of the most bizarre ventures are by people whom central practitioners in Science Studies would see as both confused and peripheral to the enterprise.

(22.) A seminal work is Wilfrid Sellars's essay “Empiricism and the Philosophy of Mind,” originally published in 1956 and reprinted in Sellars, *Science, Perception, and Reality* (London: Routledge & Kegan Paul, 1963). Philosophers of science often encountered arguments akin to Sellars's through the presentations of Norwood Russell Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958); and Thomas Kuhn, *The Structure of Scientific Revolutions*.

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(30.) For my reconstruction of parts of this example, see *The Advancement of Science*, 272–90. The intricate details of many of Lavoisier's experiments and his reasoning from them are provided by F. L. Holmes in *Lavoisier and the Chemistry of Life* (Madison: University of Wisconsin Press, 1985).

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(39.) "But in using contemporary science, we might be wrong!" Indeed. But all this shows is that the history we write today is fallible and that future developments in science may provide cause for rewriting. This should not be surprising. After all, it would be very odd for historians to plead that the knowledge they amass is especially invulnerable to revision, that it can survive changes in belief and context!

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(44.) Russell originally used this quotation to characterize Nietzsche's philosophy (*History of Western Philosophy* [London: Allen and Unwin, 1946], 734). That seems to me unfair, but to apply much more aptly to contemporary work in Science Studies. Perhaps some future scholar will view my judgment as unjust. I think it more likely that both the writings I criticize and my criticisms will vanish into dust.

(45.) To be fair, Pickering's book sometimes emerges from its preoccupation with finding a proper idiom for Science Studies to offer some descriptions of parts of scientific practice. In my judgment, these descriptions would be far more illuminating if they were stripped of the web of metaphors that seems to be the book's primary purpose to promulgate.

(46.) As David Hull pointed out to me, my tracing of the route to the Four Dogmas and beyond offers an intellectual history of Science Studies, but it would be interesting to accompany this with a social history. He indicates the outlines along which the history might go (using the kind of analysis deployed in his study of the wars among competing cladists (*Science as a Social Process* [Chicago: University of Chicago Press, 1988]). In the beginning, a group of young Turkssay some provocative things, flaunting orthodox views about the sciences, hinting that scientific knowledge isn't what it's cracked up to be. They become influential and, in a decade or so, point out that their statements have been misinterpreted, that their critiques are more nuanced than has been supposed. Younger Turks view this as a cop-out and decide to make a niche and a name for themselves by going beyond their insufficiently radical predecessors. And so it goes. Writing a history of this kind—revealing the social interests at work in the development of Science Studies—would be an interesting project, whether or not Hull's intriguing sketch is accurate.

(47.) Other recent studies that point toward more adequate interdisciplinary work in science studies include important essays by Arthur Fine, “Science Made Up,” in *The Disunity of Science*, ed. Peter Galison and David Stump (Stanford, Calif.: Stanford University Press, 1996), 231–54; and Nancy Cartwright, “Fundamentalism and the Patchwork of Laws,” *Proceedings of the Aristotelian Society*, 1994.

(48.) This question was raised forcefully by Isaac Levi in a penetrating discussion of *The Advancement of Science*. See his contribution to the symposium on that book in *Philosophy and Phenomenological Research*, September 1995, 619–27; and my response, 671–73. In *The Lives to Come*, I address some of Levi's concerns in a concrete case (the ethical and social issues around the Human Genome Project). I offer a more general discussion in my essay “An Argument about Free Inquiry” (*Nous* 31, 1997, 279–306), but this is only a first start at addressing a complex of neglected issues.

(49.) Gross and Levitt appreciate this point (45), as does Evelyn Fox Keller; see her *Secrets of Life, Secrets of Death* (New York: Routledge, 1992), 3–5. Keller once remarked to me that many of the most serious concerns about the ethical and social implications of the Human Genome Project result from recognizing that people do have genotypes (really) and that their genotypes can be discovered.

(50.) Although I independently made this point on several occasions, I owe this elegant formulation of it to some witty remarks by Richard Boyd.

(51.) I suspect that some of the authors who provoked Gross and Levitt's critique were moved by this strategy.

(52.) I owe this phrase to Jonas Salk. For decades, Salk was interested in promoting the understanding of the interaction between the sciences (particularly the biological sciences) and human concerns. Shortly before his death, Patricia Churchland and I had several conversations with him about the ways in which such understanding might be advanced, and in one of these Salk used the phrase to characterize our projected joint enterprise.

(53.) In chap. 8 of *The Advancement of Science*.

(54.) *Whose Science? Whose Knowledge?* (Ithaca, N.Y.: Cornell University Press, 1991), 5. Italics in original. Harding thinks that such sciences will also benefit “female men.” Both this book and her previous book are admirably clear about what she is claiming, although the clarity of the claims does tend to show the poverty of the argument.

(55.) There are important differences between Keller and Longino and also among positions that Keller has defended at different stages of her career. For reasons of space, I do not deal with Keller's complex views here, but I urge those who know her only through Gross and Levitt's critique to read her most recent book, *Secrets of Life, Secrets of Death*.

(56.) See Hull, *Science as a Social Process*, 66–81, 86–98. I note, for the record, that I do not agree with Longino's account of objectivity. The differences between our positions can be gleaned from our respective contributions to F. Schmitt, ed., *Social Epistemology* (Lanham, Md.: Rowan and Allanheld, 1994).

(57.) “A plague o' both your houses!” *Romeo and Juliet*, 3.1.89. Note that despite his position in the middle, Mercutio is closer to the Montagues than to the Capulets.

Notes:

(1.) Here and throughout, I use “science studies” to refer to the field of the study of science by nonscientists, paradigmatically historians, philosophers, and sociologists; and “Science Studies” to refer to particular views about that field, specifically the grab bag of doctrines that have drawn the wrath of scientific critics. Capitalization of the title sentence is left as an exercise for the reader.

(2.) For a penetrating response to Weinberg, see Wesley Salmon's lucid essay “Dreams of a Famous Physicist,” forthcoming in a collection of his essays to be published by Oxford University Press. In fairness to Weinberg, I should note that he does acknowledge that his chapter title is an overstatement.

(3.) These are only two among many obvious examples. For paradigms of this kind of philosophical work, see John Earman, *A Primer on Determinism* (Dordrecht: Kluwer, 1986); and Patricia Smith Churchland, *Neurophilosophy* (Cambridge, Mass.: MIT Press, 1985).

(4.) See, for example, Ernst Mayr and William Provine, eds., *The Evolutionary Synthesis* (Cambridge, Mass.: Harvard University Press, 1980), which is typical of much work on evolution after Darwin in its collaboration between historians of science and leading evolutionary biologists. For eugenics, see Daniel Kevles, *In the Name of Eugenics* (London: Penguin, 1987); for the first decades of molecular biology, see Horace Freeland Judson's magisterial *The Eighth Day of Creation* (New York: Simon & Schuster, 1979); and for experiments in twentieth-century physics, see Peter Galison, *How Experiments End* (Chicago: University of Chicago Press, 1987).

(5.) For the IQ controversy, see Ned Block and Gerald Dworkin's anthology *The IQ Controversy* (New York: Pantheon, 1974), especially the long essay by the editors. Block also wrote, in *Cognition* (1995), the single best diagnosis of the flaws of Richard Herrnstein and Charles Murray's *The Bell Curve*. Pioneering work on the units of selection controversy has been done by David Hull, William Wimsatt, and Elliott Sober; see, in particular, Sober's *The Nature of Selection* (Chicago: University of Chicago Press, 1992). The implications of Bell's theorem have been explored by numerous contemporary philosophers of science, including Bas van Fraassen, Abner Shimony, Arthur Fine, Jon Jarrett, and Geoffrey Hellman. Many of the most important essays on quantum mechanics by philosophers have appeared in *Physics Review Letters*; and for two recent studies that add new dimensions to the discussion of issues in the foundations of quantum mechanics, see David Albert's *Quantum Mechanics and Experience* (Cambridge, Mass.: Harvard University Press, 1992); and Tim Maudlin's *Quantum Non-Locality and Relativity* (Oxford: Blackwell, 1994). Groundbreaking work on causal modeling in the social sciences has been done by Clark Glymour, Peter Spirtes, Richard Scheines, and Kevin Kelly. See Glymour et al.'s *Discovering Causal Structure*. On the sociobiology debate, see Michael Ruse, *Sociobiology: Sense or Nonsense* (Dordrecht: Reidel, 1979); and my own *Vaulting Ambition: Sociobiology and the Quest for Human Nature* (Cambridge, Mass.: MIT Press, 1985).

(6.) See Londa Schiebinger, *The Mind Has No Sex?* (Cambridge, Mass.: Harvard University Press, 1989); and Kenneth Manning, *Black Apollo of Science* (New York: Oxford University Press, 1983). Paul R. Gross and Norman Levitt offer a somewhat condescending evaluation of Manning's work in their *Higher Superstition* (Baltimore: Johns Hopkins University Press, 1994), 285, n. 62), without offering any detailed criticism.

(7.) For example, see Troy Duster, *Backdoor to Eugenics* (New York: Routledge, 1990); Dorothy Nelkin and Laurence Tancredi, *Dangerous Diagnostics*, 2d ed. (Chicago: University of Chicago Press, 1994); Daniel Kevles and Leroy Hood, eds., *The Code of Codes* (Cambridge, Mass.: Harvard University Press, 1992); and my own *The Lives to Come: The Genetic Revolution and Human Possibilities* (New York: Simon & Schuster, 1996).

(8.) Although Gross and Levitt are also often read as making this charge, this seems to be more a matter of the tone of *Higher Superstition* than of what they say by way of criticizing prominent figures in Science Studies. True, they rightly take Bruno Latour and Sandra Harding to task for their ignorance on various technical matters (matters that are directly relevant to the topics they discuss), but Gross and Levitt explicitly note technical competence in other cases. For example, they praise Harmke Kamminga's exposition of chaos theory (95), single out Scott Gilbert's “distinguished” exposition of developmental biology (117, 121), and remark on Evelyn Fox Keller's extensive training in science (140).

Here and throughout this chapter, parenthetical page references to Gross and Levitt are to *Higher Superstition*. I also doubt that Sokal would accuse all practitioners of Science Studies of scientific illiteracy. Yet the myth is now widespread among scientists, and it needs to be debunked.

(9.) Published by the University of Chicago Press in 1986. When four societies in science studies (the History of Science Society, the Society for the Social Study of Science, the Society for the History of Technology, and the Philosophy of Science Association) met together that year (the only occasion on which all four have ever met together), Rudwick's book was the focus of a unique multidisciplinary symposium in which scholars from all four societies commented on it.

(10.) Stephen Jay Gould pays tribute to Rudwick's paleontological work in his review of *The Great Devonian Controversy* (see *An Urchin in the Storm* [New York: Norton, 1987], 78), but perhaps the real compliment is the fact that Rudwick's work on brachiopods is cited as an important illustration in one of the most widely read essays in recent evolutionary theory, Gould and Lewontin's “The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme,” *Proceedings of the Royal Society of London* B 205 (1979): 581–98.

(11.) Of course, scholars in science studies sometimes fail to recognize that particular pieces of scientific information would be useful to them, and professional researchers can play a valuable critical role in pointing this out. This is quite a different objection, however, from the charge that science studies is filled with ignorant dilettantes.

(12.) Here I should acknowledge that some of these theses are matters of sophisticated philosophical debate and that some philosophers have offered serious challenges to straightforward readings of them. In particular, Hilary Putnam, Arthur Fine, Nelson Goodman, and Richard Rorty all would object to the most obvious interpretation of 2. It seems to me important to separate philosophical worries about the understanding of the relation of thought and language to reality from the much cruder suggestions put forward in most of contemporary Science Studies, and also to recognize that for Putnam and Fine at least, there is a sense in which 2 can be construed to be true. (This may also hold for Goodman and Rorty, although here I am less confident.) The major objections to 2 that condemn strong versions of realism is that philosophers have added metaphysical encumbrances to the ordinary practices of describing the accomplishments of the sciences, not that the practice of identifying these accomplishments ought to be radically revised. This is clearest in Fine's commendation of “the natural ontological attitude” see *The Shaky Game* (Chicago: University of Chicago Press, 1986).

(13.) Of course, some philosophers would demur. See Bas van Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980). Van Fraassen's views have been subject to extensive discussion and criticism. See, for example, P. Churchland and C. Hooker, eds., *Images of Science* (Chicago: University of Chicago Press, 1984).

(14.) Here it's important to recognize that in both instances, the appropriate notion of accuracy depends on conventions of map reading. One should not infer from standard genetic maps that chromosomes are beautifully straight, any more than from the familiar map of the London Underground that the directional relationships among various stations are precise.

(15.) *Seem* is the right word here, for our knowledge of what parts of our beliefs genuinely do work for us is itself partial.

(16.) It's worth noting that Gross and Levitt seem to acknowledge this point (139), although they seem mostly unwilling to accept the idea that historical, philosophical, or sociological investigations might reveal in detail how it applies to particular scientific fields (one exception is their praise for some of Stephen Jay Gould's historical studies).

(17.) The *locus classicus* for this theme is obviously Thomas Kuhn's *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962; expanded 2d ed., 1970). The theme essentially develops Kuhn's account of "normal science," although as I have suggested, the characteristics of normal science may best be appreciated by looking at those convulsive changes that Kuhn calls "revolutions." It is important to note, however, that everything I have said can be accepted without endorsing Kuhn's account of scientific revolutions. There is no need to suppose that the large changes involve "conversion experiences." For my own attempt to develop the sociohistorical themes without the nonrationalist elements that sometimes surface in Kuhn's writings, see my *The Advancement of Science* (New York: Oxford University Press, 1993).

(18.) See J. S. Mill, *On Liberty*, chap. 2. Paul Feyerabend has urged that science should always find a place for the voicing of heretical views (see his *Against Method* [London: Verso, 1975]), and Elisabeth Lloyd argues that his defenses of heterodoxies are designed to exemplify the strategy of playing the devil's advocate, whose participation Mill saw as so important. From a quite different direction, I have argued that there is no reason to think that social institutions that take advantage of our rivalries and loyalties are necessarily opposed to the advancement of knowledge. See my "The Division of Cognitive Labor," *Journal of Philosophy* 87 (1990): 5–22; and chap. 8 of *The Advancement of Science*.

(19.) Of course, critics claim that the optimism is ill based. See Richard Lewontin, "The Dream of the Human Genome," in his *Biology as Ideology* (New York: Harper, 1992). I try to arrive at a realistic assessment in chaps. 4 and 5 of *The Lives to Come*.

(20.) I should note that many of the most egregious examples cited by Gross and Levitt are by people whom practitioners of Science Studies would not view as central to the field. As one who has attended numerous fora in Science Studies during the past decade, I have never heard of Steven Best, Katherine Hayles, Maryanne Campbell, or Morris Berman. I don't think that I have heard presentations that cite Stanley Aronowitz or Jeremy Rifkin and only a couple that allude to Carolyn Merchant. By contrast, the following figures are omnipresent in the presentations and discussions: Bruno Latour, Donna Haraway, Steven Shapin, Simon Schaffer, Helen Longino, Evelyn Fox Keller, and Sandra Harding (all of whom are discussed by Gross and Levitt) as well as Harry Collins, Peter Galison, Lorraine Daston, Paul Forman, Norton Wise, Trevor Pinch, Michael Lynch, Andrew Pickering, and Ian Hacking (none of whom receives a mention).

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(42.) It is worth repeating an insightful footnote of Larry Laudan: "Foucault has benefited from that curious Anglo-American view that if a Frenchman talks nonsense it must rest on a profundity which is too deep for a speaker of English to comprehend." *Progress and Its Problems* (Berkeley and Los Angeles: University of California Press, 1977), 241, n. 12. I think that Laudan overstates Foucault (whose treatments of madness, the clinic, and punishment seem to me insightful) but that his diagnosis of a peculiar tendency among Anglo-American academics is correct.

(43.) Alan Sokal was surely moved to perpetrate his hoax by the grandiose silliness of much postmodern discourse. That hoax may have made dialogue in Science Studies and between students of science and scientists far more difficult (as Arthur Fine pointed out, Sokal's action made it far harder for a philosopher of physics to interact with already suspicious physicists—although Sokal has gone on record praising the work of several philosophers of physics), but it has probably served a valuable purpose in departments of literature. Reaction to the hoax has made it clear that some emperors are naked, so Sokal may have given scholars who focus on Dante or Jane Austen the courage to deny that the study of urban graffiti has quite the same depth or interest.

(44.) Russell originally used this quotation to characterize Nietzsche's philosophy (*History of Western Philosophy* [London: Allen and Unwin, 1946], 734). That seems to me unfair, but to apply much more aptly to contemporary work in Science Studies. Perhaps some future scholar will view my judgment as unjust. I think it more likely that both the writings I criticize and my criticisms will vanish into dust.

(45.) To be fair, Pickering's book sometimes emerges from its preoccupation with finding a proper idiom for Science Studies to offer some descriptions of parts of scientific practice. In my judgment, these descriptions would be far more illuminating if they were stripped of the web of metaphors that seems to be the book's primary purpose to promulgate.

(46.) As David Hull pointed out to me, my tracing of the route to the Four Dogmas and beyond offers an intellectual history of Science Studies, but it would be interesting to accompany this with a social history. He indicates the outlines along which the history might go (using the kind of analysis deployed in his study of the wars among competing cladists (*Science as a Social Process* [Chicago: University of Chicago Press, 1988]). In the beginning, a group of young Turks
say some provocative things, flaunting orthodox views about the sciences, hinting that scientific knowledge isn't what it's cracked up to be. They become influential and, in a decade or so, point out that their statements have been misinterpreted, that their critiques are more nuanced than has been supposed. Younger Turks view this as a cop-out and decide to make a niche and a name for themselves by going beyond their insufficiently radical predecessors. And so it goes. Writing a history of this kind—revealing the

social interests at work in the development of Science Studies—would be an interesting project, whether or not Hull's intriguing sketch is accurate.

(47.) Other recent studies that point toward more adequate interdisciplinary work in science studies include important essays by Arthur Fine, "Science Made Up," in *The Disunity of Science*, ed. Peter Galison and David Stump (Stanford, Calif.: Stanford University Press, 1996), 231–54; and Nancy Cartwright, "Fundamentalism and the Patchwork of Laws," *Proceedings of the Aristotelian Society*, 1994.

(48.) This question was raised forcefully by Isaac Levi in a penetrating discussion of *The Advancement of Science*. See his contribution to the symposium on that book in *Philosophy and Phenomenological Research*, September 1995, 619–27; and my response, 671–73. In *The Lives to Come*, I address some of Levi's concerns in a concrete case (the ethical and social issues around the Human Genome Project). I offer a more general discussion in my essay "An Argument about Free Inquiry" (*Nous* 31, 1997, 279–306), but this is only a first start at addressing a complex of neglected issues.

(49.) Gross and Levitt appreciate this point (45), as does Evelyn Fox Keller; see her *Secrets of Life, Secrets of Death* (New York: Routledge, 1992), 3–5. Keller once remarked to me that many of the most serious concerns about the ethical and social implications of the Human Genome Project result from recognizing that people do have genotypes (really) and that their genotypes can be discovered.

(50.) Although I independently made this point on several occasions, I owe this elegant formulation of it to some witty remarks by Richard Boyd.

(51.) I suspect that some of the authors who provoked Gross and Levitt's critique were moved by this strategy.

(52.) I owe this phrase to Jonas Salk. For decades, Salk was interested in promoting the understanding of the interaction between the sciences (particularly the biological sciences) and human concerns. Shortly before his death, Patricia Churchland and I had several conversations with him about the ways in which such understanding might be advanced, and in one of these Salk used the phrase to characterize our projected joint enterprise.

(53.) In chap. 8 of *The Advancement of Science*.

(54.) *Whose Science? Whose Knowledge?* (Ithaca, N.Y.: Cornell University Press, 1991), 5. Italics in original. Harding thinks that such sciences will also benefit “female men.” Both this book and her previous book are admirably clear about what she is claiming, although the clarity of the claims does tend to show the poverty of the argument.

(55.) There are important differences between Keller and Longino and also among positions that Keller has defended at different stages of her career. For reasons of space, I do not deal with Keller's complex views here, but I urge those who know her only through Gross and Levitt's critique to read her most recent book, *Secrets of Life, Secrets of Death*.

(56.) See Hull, *Science as a Social Process*, 66–81, 86–98. I note, for the record, that I do not agree with Longino's account of objectivity. The differences between our positions can be gleaned from our respective contributions to F. Schmitt, ed., *Social Epistemology* (Lanham, Md.: Rowan and Allanheld, 1994).

(57.) “A plague o' both your houses!” *Romeo and Juliet*, 3.1.89. Note that despite his position in the middle, Mercutio is closer to the Montagues than to the Capulets.