One

INTRODUCTION

A GROWING NUMBER OF HISTORIANS, sociologists, and philosophers of science start their work from a recognition that science is in important ways a social and political activity. It is social in that scientists are always members of communities, trained into those communities and necessarily working within them. Communities, among other things, set standards for inquiry and evaluate knowledge claims. The politics comes in at many levels: science is an arena for rhetoric and alliances, ideology and values of many different types are important components of ongoing research, and scientists are engaged in struggles to gain resources and to promote their views. Importantly, scientists have investments in skills, prestige, knowledge, and specific theories and practices. And conflicts in a wider society may be mirrored by and connected to conflicts within science; splits along gender, race, class, and national lines occur both within science and in the relations between scientists and nonscientists. In short, scientific knowledge is always connected to power.

This book is a philosophical discussion of recent social and political studies of science. It is thus a discussion of the *social construction* of scientific knowledge, of that knowledge as a product of communities and societies, and marked by the circumstances of its production. It shows that there are a number of ways and a number of senses in which scientific knowledges are social constructions; this book is therefore an essay on mediation, on the mediatory roles of scientists between nature and knowledge.

Social constructivism about science and technology is often seen not as a thesis about mediation but as the thesis that scientists and engineers construct the world with their ideas or representations: what they study is not independent of them. I argue that as a universal metaphysical thesis *that* constructivist thesis is untenable, though it is right about some types of cases. It is also not particularly valuable or representative within the constructivist program of science and technology studies. Almost never does actual work called "constructivist," or discussion of constructive processes in science, rest on the thesis that representations literally and simply construct reality; that is rarely at issue in today's science and technology studies. What

is at issue is more complicated: the flexibility of scientific data, the indeterminacy of debates, the methods that are used to end or avoid debates and create knowledge, science as a set of practices, and so forth.

In addition to the obvious myths they dispute, social constructivist studies are often taken as indicating that scientific theories and beliefs cannot represent reality, that scientific *realism* is wrong. The claim is that realism cannot be sustained because the epistemological picture on which it rests is flawed; that is, talk of truth in the sciences is taken to be attached to a questionable picture of science's methods and rationalities. One of my goals here is to argue that, while we should reject most versions of the realist position we should not reject its commonsense core: there are grounds for believing that some scientific knowledge represents a pre-existing material world. What is right about realism can be disconnected from realists' idealized views of science, and the issue of truth can be disconnected from the discredited mythology. Thus I will be arguing for realism as well as for constructivism. The book shows how scientists can often produce accurate knowledge of nature, yet it insists that that knowledge is a human and social product.

My overarching aim, then, is to reconcile the possibility of scientific representation with an understanding of science as a social and political institution. Such an understanding does not just mean being able to point to this or that piece of science as poor, infected by ideology, and so on: traditional pictures of science have enabled us to do this handily, showing ways in which some particular piece of science has diverged from the truth. We have learned from work in science and technology studies (S&TS) and feminist critiques that good science, science as usual, has important political components. This is what appears to cause epistemological problems for realism.

A small aside on terminology is needed: I use the terms *science and technology studies* or *S&TS* to refer to the emerging interdisciplinary field studying science and technology as institutions, or as social enterprises—thus I treat it as a singular term. Because it is an *emerging* field, its boundaries are not entirely clear, which makes for some ambiguity in the term. In addition, although I keep the "and technology" part of the label, my focus in this book is almost exclusively on science.

The difference between finding ideological inputs in this or that piece of science and finding politics in all of science is part of, for example, the difference between feminist empiricism and feminist standpoint epistemologies. A central premise in feminist empiricism is that scientific methodology, when properly applied, is sufficient to correct for gender biases. But feminist standpoint theorists, impressed by the wealth of examples of gender bias in science, say that scientific methodology is clearly *not* in general capable of

producing nonsexist science and that inputs from feminist thinking—"thinking from women's lives," in Sandra Harding's (1991) phrase—will produce preferable science. So science as usual is seen as routinely incorporating sexist assumptions prevalent in our society. Thus this book can be seen to respond to the problem Helen Longino and Evelynn Hammonds pose when they say that "above all, we have yet to demonstrate how the scientific method can provide successful representations of the physical world while at the same time inscribing social structures of domination and control in its institutional conceptual, and methodological core" (Longino and Hammonds 1990, 181). The flip side of that task is to understand the constructed nature of scientific knowledge.

A strong constructivist theme—the claim that representations routinely shape the material world—appears over and over again in S&TS (e.g., Haraway 1989; Latour and Woolgar 1986; Collins 1992; Foucault 1980). For the political critic of science—for example for the feminist historian, sociologist, or philosopher of science—one problem with the strongest versions of constructivism, and antirealism in general, is obvious. While a thoroughgoing skepticism about scientific knowledge can potentially provide tools for delegitimating all of science, it does not allow for certain specific, more local, criticisms; it does not allow, or at least makes difficult, the task of showing that a particular piece of scientific knowledge is poor. But this is an important part of—in fact a strong motivation for—feminist science and technology studies. Feminist critiques of science are often aimed at showing that specific accounts of women, their psychologies, their sexualities, and so forth, are simply wrong as well as ideologically motivated. *Truth* and *falsity*, and hence the qualities of the material and social world, are valuable resources for feminism. Attempts to undermine truth cut against falsity, removing a key foundation of feminist critiques of science and, more generally, political critiques of science. This provides another motivation for the investigation of social constructivism and realism with respect to these critiques, a motivation present in the background throughout the book.

MULTIPLE MEANINGS AND INSIGHTS: AN EXAMPLE

Ruth Bleier's short article "Lab Coat: Robe of Innocence or Klansman's Sheet" (Bleier 1986a) is one of many programmatic articles for feminist S&TS. It lists some accomplishments—the demonstration of sexism in all parts of science, the demonstration that science is a part of culture, one set of social practices among others—and presents some goals—to force scientists to be reflective about the values that influence their work and to articulate a feminist epistemology that avoids some flaws of traditional science. Feminist work on the natural and social sciences has far to go to achieve

these goals, Bleier says, because it has not begun to make much of a dent on the sexism and sexist ideology found in so much of scientific discourse.

To show us some sexism in high places she quotes Nobel Prize winner James Watson complaining, in 1985, about U.S. Government science policy: "The person in charge of biology is either a woman or unimportant. They had to put a woman some place. They only had three or four opportunities, so they got someone in here. It's lunacy" (Watson, quoted in Bleier 1986a, 55–56). To show us how sexist ideology makes its way into some everyday good science, she introduces us to one of her areas of expertise, brain lateralization research, and summarily dismantles an article purporting to link brain lateralization with abilities that supposedly differ with sex.

Bleier's brain lateralization target is a study by Norman Geschwind, the results of which were published in the *Proceedings of the National Academy of Sciences* and reported in the journal *Science*. She argues that the sources drawn upon by Geschwind were used selectively, that he put forward implausible hypotheses on brain development, that he assumed without evidence that there is "greater spatial orientation" in male rats than in female rats, and so on. In short, Geschwind's argument fails on a number of counts, and his claim to be able to link sex and certain skills is unjustified.

Brain lateralization research, part of intelligence research generally, is a clear site of controversy, a site where feminists and nonfeminists—not to mention racists and antiracists, conservatives and radicals—might argue about the naturalness of current social stratification or apparent stratification. The cultural meaning of the results were easily picked up:

> In news reports of his work in *Science*, Geschwind suggested that testosterone effects on the fetal brain can produce "superior right hemisphere talents, such as artistic, musical, or mathematical talents." The *Science* news article was titled "Math Genius May Have Hormonal Basis." (Bleier, 61)

But the translation from lateralization to 'superior talents' still has to be made in order for the cultural meanings of such a study to become understood. That translation is made, Bleier argues, through the subtle substitution of the word *specialized* for *lateralized. Specialized*, in turn, can easily be translated into *superiority.* Feminist S&TS has to pay attention not only to discrimination, overt sexism, and to the ideological basis of scientific claims but also to the language in which claims are made. Feminist critics must become literary-textual critics. They have to correct some blindnesses of scientists, who do not acknowledge the "multiplicity of meanings of their texts" and "do not recognize or acknowledge the degree to which their scientific writing itself participates in *producing* the reality they wish to present" (61).

Like the scientific texts about which Bleier is talking, her own has multiple meanings. In particular, this last claim that scientific writing produces reality can be interpreted along different lines. In important ways the claim sits alone in the text, unsupported by and relatively unconnected to anything else in the article, which increases its ambiguity. The "reality they [scientists] wish to present" might be brain lateralization, or the biological justification for stratification of society along gender lines, or it might be gender differences themselves. The distinctions are important. If Bleier means that scientists produce lateralization, her text is part of a constructivism that assumes that the characteristics of the natural world are invented, rather than discovered, by science; the characteristics of brains do not have definition until scientists describe them. If Bleier means that science produces justifications, she is pointing to the connections between science and social issues and the weight of scientists' words in the political arena; the *Science* article implies that many skills are linked to sex and, therefore, that a sex-stratified society is a natural one. If she means that science produces gender differences, then Bleier is probably pointing to the effects of oppression, that presuppositions of inferiority of skills (in music, art, and mathematics) can lead to real inferiority; gender is the result of training, and science contributes to decisions about that training.

I do not want to defend any one of these options over the others with respect to this particular text. All three, and others of the "multiplicity of meanings" of such a phrase, are themes that run through feminist criticism of the sciences, and S&TS more generally. All three are interesting and worth exploring; all three produce insights on science as a social process.

VARIETIES OF CONSTRUCTIVISM AND REALISM

For the purposes of sketching the disagreement between realists and constructivists, we can imagine scientific knowledge as a map, corresponding in its contours to features of the material world. Of course relatively few realists and constructivists believe that much knowledge is like maps, and they should not believe that maps are much like my caricatures below: language, even cartographic language, doesn't have such neat correspondences with the world.

Realists tend to emphasize the impact of the material world on the resulting representation, and minimize the effects of human agency; for realists, scientists and their work are essentially transparent. While mapping may not be easy, once one develops the right tools the presence of the explorers (experimenters) and cartographers (theoreticians) is essentially irrelevant to the resulting picture.

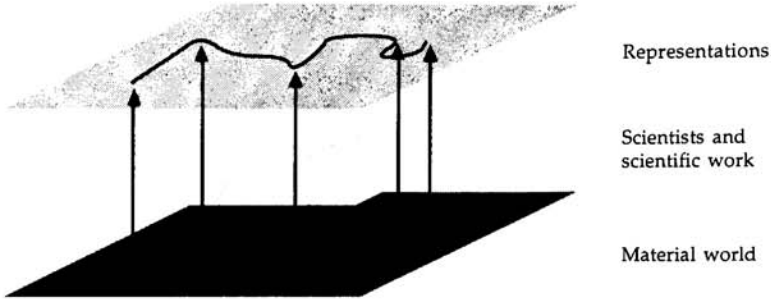

Figure 1.1 Realist mapping

Constructivists, on the other hand, emphasize the middle plane—the scientists and their work—and minimize the importance of the material world; for them the material world is essentially featureless, at least before there is some consensus on those features. Constructivists look to empirical studies of scientific practice, and draw our attention to efforts to produce stability and agreement.

There is a very different position, which has some affinities to constructivism but which I will put in a separate camp. "Empiricism" emphasizes the central plane, but not to the point of saying that theories or representations affect the material world. Instead, the material world is simply an unknown, though one which affects scientists' depictions of it. Empiricists see theories as systematically accounting for data, telling a good story but not one about the real makeup of the world. Empiricists take seriously the idea that science is not mapmaking.

Of course these pictures are too simple because, as with most philosophical doctrines, there is no real consensus on what "scientific realism,"

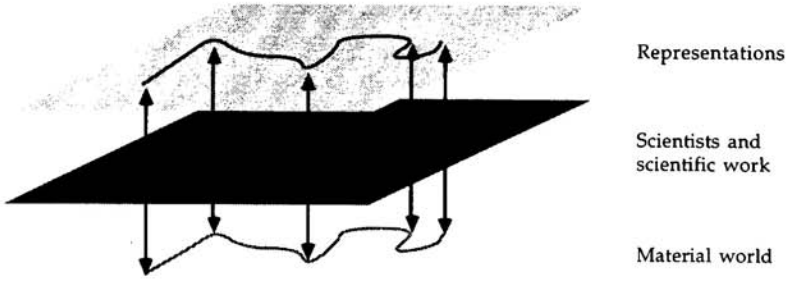

Figure 1.2 Constructivist mapping

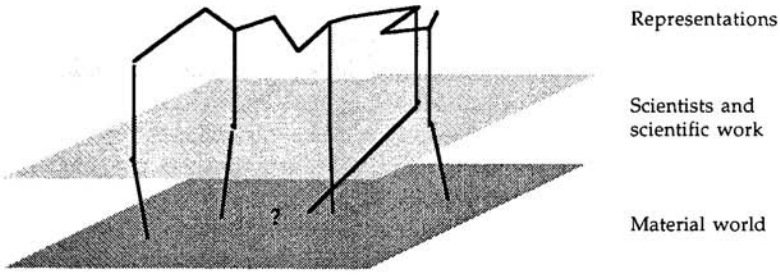

Figure 1.3 Empiricist science

"social constructivism," and "empiricism" mean and how each should be defended. One result of these multiple meanings is miscommunication in the discussions that take place—people routinely attack straw positions without being able to see how their targets differ from real positions taken. Later chapters sort out some of the multiple meanings.

My goal with respect to realism is to create and justify a position compatible with insights from science and technology studies. To do this I will be taking realism in its most deflationary form, a minimal and piecemeal realism. My minimal realism claims that the entities experimenters appear to fruitfully manipulate usually exist, and that researchers have knowledge about some of the properties of these entities.[1] It also claims that obtaining true theories is one of scientists' aims, but not that this need be an overriding aim.[2] The "piecemeal" part of my realism comes from the fact that I argue that cases can be made for the approximate truth of certain specific scientific statements about unobservables; that is, there is good justification for being realists about those theories, though not all theories. Such a position, in its refusal to adopt a picture of science as a unitary activity, makes no commitment about science's general aims or results.[3]

Any realism must also maintain that the objects scientists study and know about usually exist independently of those scientists. That is, features of the material world are independent of human inquiry and the processes involved in that inquiry. This "mind-independence" tenet rules out the constructivist claim that the characteristics science attributes to the world are more accurately described as parts of conceptual schemes. I will not argue for the mind-independence tenet exactly as it stands, but will instead try to elaborate on it so that it is right, taking account of insights from the different forms of constructivism. I will limit the independence of objects of knowledge by spelling out some plausible forms of constructedness.

Realists, good rhetoricians that they are, tend to slide from something like the deflationary position to much stronger ones (Fuller 1988). For that

reason, realism is sometimes thought to entail the strong metaphysical position that "the world consists of some fixed totality of mind-independent objects. There is exactly one true and complete description of 'the way the world is'" (Putnam 1981, 49). Although it is a tendency toward which realists move, few philosophers would *assent* to one-true-theory realism; it is almost always a characterization of realism provided by its opponents rather than its defenders.

Similarly, realism is sometimes thought to entail the position that I will call "rationalism," which claims that to understand science is to understand a rational method used by individual scientists. Rationalists see a unitary method at the core of scientific work, and that method is exemplified in scientists' decisions about the collection of data and the arguments that they make on the basis of that data. From the rationalist position, the study of the core of science is the domain of logic and traditional epistemology, and any type of social analysis is irrelevant. Although few realists would endorse this position, it is nonetheless a significant part of work in the philosophy of science today.[4]

The minimal and deflationary realism above is what I will mean when I use the term in a general way, when I claim that something is compatible or incompatible with realism. The deflationist stance captures what I see as right in scientific realism. I do not want to include any version of either the "one-true-theory" thesis or the rationalist thesis as part of a deflationary realism; those positions, or attitudes, are the part of the baggage of realism I most want to jettison.

My goals with respect to constructivism are more complicated. For most readers the idea that scientists in some sense make or remake the world as they go along is immediately implausible. By articulating a number of separate constructivist claims, I will try to make some versions of the position more plausible at the same time that I make others even less so. For example, I use "neo-Kantian" as a label for the family of positions asserting that there is some special causal or semantic connection from what scientists say (or do) to the structure of the material world. Neo-Kantian constructivists assert that scientific consensus has some direct material import, that representations create their own objects.

In contrast, "heterogeneous" contructivism pays particular attention to the ways in which scientists attempt to construct stable structures and networks of power (Taylor 1995a, 1995b; Latour 1987). It attempts to draw together into one account the variety of resources available to scientists and shows how these are used. And the position can be made more social: theories and other scientific products may be *socially* constructed in the sense that they are the products of many people interacting, possibly with different motivations and different background beliefs. They are genuinely social

products, rather than the direct consequence of people's interactions with the material world.

There are many other constructivist positions, but I will add only one more to the ring for the moment: something may be socially constructed in the sense that its reality depends on social interactions. When a large number of people act as though something is the case, they may be making it the case (Berger and Luckmann 1966; Barnes 1988; Harré 1986). Self-validating statements and social norms are often examples of this type of interaction—a presumption by a sufficient number of people that it is improper for middle- and upper-class women to work is all that is needed for it to *be* improper for these women to work. This is at least a large part of the constructivism of constructivism/essentialism debates about the causes of such things as sexual orientation and gender and racial characteristics.

In this book I argue against strong versions of neo-Kantian constructivism. Other varieties represent important insights on science and culture that need to be *more* fully recognized in science and technology studies. Like the realism I argue for, the constructivisms I argue for are deflationary ones, not the overarching neo-Kantian metaphysical position; that latter position tends to obscure the more mundane, though often more interesting, constructivisms that concretely display effects of the social world on scientific knowledge.

STRATEGIES

There has been relatively little realist/constructivist discussion; this is true even though both realism and constructivism are dominant in different parts of the science and technology studies community. There has been contact, of course—some scholars on both sides have taken the other side seriously enough to argue about it (e.g., Brown 1989; Giere 1988; Bloor 1991) and even to get tired of arguing (Shapin 1982). But the contact has not matured into a discussion that affects both sides the way that empiricism and realism have influenced each other.

There are a number of reasons why realist/constructivist debates have not been as common or close as realist/empiricist ones. One of these has to do with the history of Anglo-American philosophy of science, a history that shows roughly the middle third of this century dominated by logical positivist conceptions of science, which were for the most part in the empiricist camp.[5] Although logical positivism as a force within philosophy of science is pretty much dead, current work occurs in the context of a strong tradition of empiricist arguments and presuppositions. Even after the death of positivism, within philosophy empiricism remains to some extent the dragon to be slain.

A second reason for the lack of realist/constructivist debate lies in the fact that each side usually views the other position as obviously untenable. Philosophers of science, even antirealist philosophers of science (e.g., Laudan 1991), often dismiss the philosophical arguments for strong forms of constructivism as badly flawed. On their part, constructivist historians and sociologists often believe that realism is necessarily attached to a too-rational, progressive, and monistic picture of science, a picture that is implausible given recent empirical studies. Related to this is a divide in style: because constructivists are often working from historical and sociological studies, their and philosophical realists' arguments do not often meet each other head on. The constructivist historian or sociologist and the realist philosopher disagree as to whether constructivism or realism provides the better interpretative framework, but the considerations to be interpreted are different: the nature of language and of causality on the one hand and scientific practice on the other.

This book attempts to bridge some of these divides. With the realist philosopher I maintain that philosophical considerations weigh against neo-Kantian constructivism. And with the constructivist historian or sociologist I maintain that the practice of science does not cohere with the rationalist picture. But I don't want to leave matters there, for that would be merely to repeat the existing sides in the nondiscussion. Other strategies make up the bulk of the book, and together they form what I think of as a gently therapeutic approach. I reconcile realism and constructivism by showing that realism need not be attached to an overly rationalist picture of science or to the idea that there can be unmediated knowledge of nature. At the same time I remove, piece by piece, the reasons for adopting the neo-Kantian position, arguing that that position does little or no intellectual work and, thus, is not worth its price. My argument has the following components:

1. There is a strong case to be made for the premise that science sometimes represents pieces of the material world. The best explanation of scientists' successes in providing empirically adequate accounts of phenomena is that some of their assumptions are approximately right. Yet, while this argument shows that some of scientists' work adequately represents nature, it does not show that scientists are always so successful or even that representation is an overarching goal.

2. S&TS introduces a number of different senses in which scientific knowledge is constructed, and we should take these senses on their own terms. In particular, historical and sociological studies considered constructivist by their authors do not typically employ any assumptions that are constructivist in strong, neo-Kantian senses. This removes the ground for saying

either that S&TS depends on neo-Kantian social constructivism for its foundations or that the successes of S&TS in providing explanations or accounts of scientific activity indirectly support its supposed neo-Kantian foundations. Rather than showing the impossibility of representation and truth in the sciences, S&TS is engaged in the empirical study of scientists' use of available tools and resources to achieve their goals, goals which include accurate representation.

3. There are strong philosophical arguments against neo-Kantian constructivism. Possibly more importantly for the sociologist and political critic, there are also some methodological considerations against this neo-Kantianism. I argue that nonrealist forms of constructivism don't cohere well with our notions of causation, since they seem forced to posit nonmaterial causal links from representations to that which they represent. These constructivisms also don't cohere with our understanding of the distinction between epistemology and ontology, since they deny that the order of being is distinct from the order of knowing. In positing a nonmaterial link from representation to represented, neo-Kantian constructivisms hide *material* links that I argue can often be found. And finally, as I have already mentioned, constructivisms and other antirealisms disarm the strongest arguments and rhetoric of the political critic by denying the validity of the discourse of truth.

4. Some paradigmatic S&TS texts—Thomas Kuhn's *The Structure of Scientific Revolutions* (1970a) and Bruno Latour's *Science in Action* (1987)—are read for their insights on the effects of scientific work. My readings make these texts arguments for distinctive and innovative positions within a framework of deflationary realism, positions that challenge versions of mind independence—thus showing senses in which scientists *do* construct worlds—and rationalist assumptions.

5. Yet the ways in which scientific knowledge is dependent on actions and the social world are relatively mundane. For example, the observation that metaphors are ubiquitous in science shows a form of social shaping of knowledge, but it does not speak against realism, given the representative power of metaphors. And the insight that scientific knowledge is social knowledge—a community product—can contribute to an evolutionary picture of science, whereby the creation of knowledge is possible through variation and selection.

6. Collecting some of the other strategies together, it is possible to articulate a version of scientific realism that incorporates and celebrates (rather than is merely consistent with) important constructivist insights; conversely, it is possible to articulate a version of constructivism that is fully consistent with scientific realism.

There are important affinities between the realism/constructivism discussion in S&TS and the modernism/postmodernism discussion in the other humanities and social sciences. When real positions are examined, as opposed to the caricatures so often trotted out, both are debates about the right ways in which to respond to the apparent failure of foundationalist positions—attempts to ground knowledge absolutely. Postmodernism and sophisticated modernist positions agree in their rejection of foundationalism. What they disagree about is what we can learn from foundationalism's failure; they disagree about the possibility of knowledge, given this failure. This situation is mirrored in the debates between scientific realists and their constructivist counterparts; they agree that science is not rational in a foundational way but disagree about the resultant status of knowledge. This book, especially in its attempt to find a via media, is thus an attempt to contribute to the ongoing discussion on the status of modernity.