Introduction: Challenges to the Classical View of Science

> [W]hen we tell our Whiggish stories about how our ancestors gradually crawled up the mountain on whose (possibly false) summit we stand, we need to keep some things constant throughout the story. The forces of nature and the small bits of matter, as conceived by current physical theory, are good choices for this role.
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> Richard Rorty, *Philosophy and the Mirror of Nature* (1979: 344–345)

> But, irresistibly, I cannot help thinking that this idea is the equivalent of those ancient diagrams we laugh at today, which place the Earth at the center of everything, or our galaxy at the middle of the universe, to satisfy our narcissism. Just as in space we situate ourselves at the center, at the navel of things in the universe, so for time, through progress, we never cease to be at the summit, on the cutting edge, at the state-of-the-art of development. It follows that we are always right, for the simple, banal, and naive reason that we are living in the present moment. The curve traced by the idea of progress thus seems to me to sketch or project into time the vanity and fatuousness expressed spatially by that central position. Instead of inhabiting the heart or the middle of the world, we are sojourning at the summit, the height, the best of truth.
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> Michel Serres with Bruno Latour, *Conversations on Science, Culture, and Time* (1995: 48–49)

People who have not come across it before frequently express surprise that there is a subject called "history of science." "Is that a kind of history or a kind of science?" they sometimes ask. "Do historians of science work in a library or a laboratory?" And, "Why would anyone want to study out-of-date science, anyway?"

To those with some knowledge of the subject, these questions no doubt sound naive. But, as I have encountered them repeatedly, I have come to feel that they reflect serious conceptual difficulties surrounding the yoking together of the words "history" and "science." It is not just that the two subjects are usually well separated in educational institutions, but that they seem to be rooted in fundamentally opposed points of view. History is oriented to the past, while science seems oriented to the future; history is connected with humanity, science (largely) with the

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nonhuman world; history is associated with culture, science with nature; history is thought of as subjective, science as objective; history uses common language, while science uses technical vocabulary; and so on. Because of these common assumptions, the conjunction of "history" and "science" can seem bizarre and confusing.

Even those of us who are now familiar with the history of science should perhaps remind ourselves, from time to time, what a strange hybrid the discipline is. It is worth asking periodically what history of science *is*, what it *can be* as a discipline. This book is about how ideas of the subject have been changing in the last few decades. It argues that the hybridization of history and science has been a remarkably fertile union, giving birth to exciting new ideas about what science is, its role in our culture and society, and the kind of history that is appropriate for understanding it.

In part, these new ideas and approaches have come about through a fundamental reconsideration of the ways of dealing with the past that are embedded in the practices of science itself. Although we sometimes think of science as aimed only toward the future, scientists do have to engage in interpretation and assimilation of the past as part of their work. Practising scientists are continually appropriating the work of their predecessors and orienting themselves in relation to it. They periodically celebrate the work of founders and pioneers of the various scientific disciplines (Abir-Am 1992). It is because their interests in the past do not coincide with those of historians, however, that problems arise. The history of science has had a long struggle to free itself from science's own view of its past.

This is particularly so because of the context in which the subject of history of science originated. When it began, during the eighteenth-century Enlightenment, it was practised by scientists (or "natural philosophers") with an interest in validating and defending their enterprise. They wrote histories in which the discoveries of their own day were presented as the culmination of a long process of advancing knowledge and civilization. This kind of account tied the epistemological credentials of science to a particular vision of history: one that saw it as steady upward progress. The science of the day was exhibited as the outcome of the progressive accumulation of human knowledge, which was an integral part of moral and cultural development. The origins of the history of science lie in this Enlightenment project to advance the standing of natural knowledge by claiming for it a particular kind of history. It is worth considering this legacy briefly, before we discuss the current prospects for the discipline.

For its pioneers, like the eighteenth-century English preacher and chemist Joseph Priestley, the history of science was part of an allencompassing vision of progress. Human knowledge could be seen to have advanced in a single positive direction, even though its forward motion might sometimes have been delayed (McEvoy 1979). Knowledge of the natural world increased in step with the enhancement of human life in all of its material and cultural aspects – the process that Enlightenment intellectuals called "refinement" or "improvement." Priestley wrote, of his *History of Electricity*, that the "idea of a continual rise and improvement is conspicuous in the whole study"; so that the history of science, thus narrated, "cannot but animate us in our attempts to advance still further" (1767: ii–v). A "philosophical" history of this kind would be more instructive than human or "civil" history, with its disorder and immoral behavior. The history of scientific progress offered readers a "sublime" experience, one provoked by ideas that, "relate to great objects, suppose extensive views of things, require a great effort of mind to conceive them, and produce great effects" (Priestley 1777: 154).

This uplifting vision of the progress of science was integrated with a particular model of epistemology. Priestley, like many of his contemporaries, was an empiricist; he believed that knowledge comprised an association of ideas that derived from the impact of external reality upon the senses. Knowledge was stored up in the mind like marks on the proverbial blank slate. Because ideas, which represented impressions of the external world, could be translated in turn into speech and writing, the stock of human knowledge was constantly augmented.

Even when strict empiricism was brought into question, at the end of the eighteenth century, the epistemological model of the mind as a "mirror of nature" was largely retained, and the history of science continued to be narrated as a story of progress. In the 1830s the man who invented the word "scientist," William Whewell, again argued that the historical development of the sciences followed the path by which the human mind gradually gained representational mastery of external reality. Although Whewell complicated Priestley's version of empiricism, by ascribing an essential role to mental activity in anticipating and structuring experience, the basic notion of knowledge as a representation of the object in the mind of the subject was retained (Brooke 1987, Cantor 1991a). This informed Whewell's view of the historian's task and the kind of narrative he should produce. He wrote:

> [T]he existence of clear Ideas applied to distinct Facts will be discernible in the History of Science, whenever any marked advance takes place. And, in tracing the progress of the various provinces of knowledge which come under our survey, it will be important for us to see that, at all such epochs, such a combination has occurred. . . .
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> In our history, it is the *progress* of knowledge only which we have to

> attend to. This is the main action of our drama; and all the events which do not bear upon this, though they may relate to the cultivation and the cultivators of philosophy, are not a necessary part of our theme. (Whewell 1837/1984: 7–9)

In the second half of the twentieth century, both Whewell's story of progress and its undergirding philosophical assumptions have been subjected to damaging criticism. Historical narratives in which science appears to advance steadily in the direction of greater accumulations of factual knowledge are now widely scorned as "whig history." Priestley's and Whewell's chronicles of the steady progress of discoveries have been revealed as nostalgic retrospectives, like the stories the Whig political historians used to tell about the steady growth of English liberty. Today's historians are more likely to set themselves the goal of understanding the past "in its own terms" (whatever that might mean) rather than in the light of subsequent developments. This has yielded histories in which knowledge, rather than continuously increasing, has undergone radical discontinuities and transformations, and in which what subsequently come to be seen as forward movements are deeply rooted in contexts that are quite foreign from a modern perspective.

Recent criticism has also removed a central philosophical support from Whewell's vision of history – the idea of a universal scientific method. Whewell wrote explicitly to demonstrate the pervasive importance of the method of induction, whereby scientific knowledge was supposedly built up, by generalization from collections of particular observations and experiments, to universal laws. The narrative of progress was designed to display the working through of the inductive method and to recommend its continuing use in science. "It will be universally expected," Whewell wrote, "that a History of Inductive Science should . . . afford us some indication of the most promising mode of directing our future efforts to add to its extent and completeness" (1837/1984: 4). Since Whewell's day, however, many alternative accounts of scientific method have come to be entertained in place of his inductivism. More fundamentally, persuasive arguments have been proposed against the belief that scientists consistently adhere to any single, specifiable method in their research. All the methods proposed have been subjected to stringent critiques, while some philosophers have undermined the whole project of methodology by arguing that human action cannot be understood as a process of following general rules. Meanwhile, sociologists of contemporary science have shown that practising scientists do not appear to be bound by any of the rules of method that have been suggested. No single method that has been articulated seems able to capture more than a part of what they actually do (Mulkay 1979: 49–59). To expect such a method to provide a key to historical development has therefore come to seem quite naive. One account has gone so far as to conclude that the only methodological rule that can be consistently applied to the great scientific innovators of the past is that "anything goes" (Feyerabend 1975).

The effect of these challenges has been to undermine the historical and philosophical assumptions upon which the history of science was originally established. Stories of the long-term incremental progress of accumulating knowledge, under the aegis of the scientific method, no longer command general acceptance. Uprooted from its original philosophical foundations, the subject has nonetheless flourished, aided by a multitude of new intellectual resources and alliances. As the link between whiggish history and classical empiricist epistemology has been broken, new links with other versions of philosophy and history, and with the humanities and social sciences, have been forged. In the process, the history of science has ceased to seem fundamentally different from other fields of human history, although it continues to benefit from a wealth of interdisciplinary connections that other kinds of history sometimes lack. Practitioners of the subject have been able to draw upon the contributions of sociology, anthropology, social history, philosophy, literary criticism, cultural studies, and other disciplines.

It would be impossible to survey all of these contributions here. Instead, in Chapter 1, I shall trace a particular lineage that connects recent historical work back to crucial arguments in the philosophy and sociology of science that surfaced in the 1960s and 1970s, though their roots go back somewhat earlier. I begin with Thomas S. Kuhn, whose *Structure of Scientific Revolutions* (1962/1970) launched a fundamental reexamination of the nature of science. Kuhn's book, as we shall see, was given a forceful if contentious interpretation by David Bloor and Barry Barnes, at the University of Edinburgh, who articulated what they called the "Strong Programme" in the sociology of science in the 1970s. This program, with its founding proposition that science should be studied like other aspects of human culture, without regard to its supposed truth or falsity, was controversial among philosophers and many historians. It nonetheless provided an important inspiration for the field that became known as the sociology of scientific knowledge (or "SSK"), which accrued some impressive empirical case studies and began to influence the work of several leading historians by the mid-1980s. As we shall also see in Chapter 1, SSK faced a significant challenge in the late 1980s from an alternative sociological approach, advocated by the "actor-network" school of Bruno Latour and Michel Callon. The arguments over these different approaches fragmented the community of sociologists of science but, paradoxically, confirmed their influence among historians, and increasingly also among philosophers. By the late 1980s, the constellation of "science studies" disciplines was heterogeneous and riven with arguments, but it was no longer possible to evade the conclusion that the traditional understanding of science had been radically undermined.

History of science took up its position as a participant in this lively and burgeoning interdisciplinary field.

The aim of this book is to map these changes, to survey the areas of research in the history of science that have been transformed by them, and to point to areas that might profitably be developed in future. I use the label "constructivism" to sum up the outlook shared by the sociologists of science whom I discuss and the historians who have been influenced by them. The term draws attention to the central notion that scientific knowledge is a human creation, made with available material and cultural resources, rather than simply the revelation of a natural order that is pre-given and independent of human action. It should *not* be taken to imply the claim that science can be entirely reduced to the social or linguistic level, still less that it is a kind of collective delusion with no relation to material reality. "Constructivism," as I shall characterize it, is more like a methodological orientation than a set of philosophical principles; it directs attention systematically to the role of human beings, as social actors, in the making of scientific knowledge. My argument will be that it has already proven to be a productive orientation for historians, one that opens up many intriguing new issues for historical investigation. The label may be a problematic one – it does have quite different connotations in fields like mathematics and architecture, for example – but it serves my purposes better than alternatives like "the Strong Programme," "social construction," or "the sociology of scientific knowledge," in part because it is not the shibboleth of any particular school.

In tracing the development of the constructivist outlook from the work of sociologists of science, I am aware that I am giving only one of a number of possible lineages. Quite different accounts could very plausibly be proposed. One might, for example, consider how various strands in twentieth-century European philosophy have called into question the model, assumed by Whewell, of the mind as mirror of nature (Rorty 1979). Modern philosophical movements, such as phenomenology, hermeneutics, and poststructuralism, have complicated the subject–object relationship that lay at the heart of classical epistemology. For example, phenomenologists have drawn attention to the ways in which human knowledge is a product of our use of our bodies. Human subjects should not be regarded as detached minds passively contemplating the material world; they learn about it through embodied interaction. Martin Heidegger's philosophy similarly presents all knowledge as the outcome of use – as tools or instruments – of the objects we find around us. It is only by encountering things in the world and using them for our own purposes that we come to know them, in Heidegger's view (Rouse 1987). Meanwhile, hermeneutics and poststructuralism have directed attention toward language, which, they suggest, should not be seen simply as a transparent vehicle for communicating thought. Language is to be grasped in its rhetorical and semiotic dimensions, which go well beyond its function of conveying a message. We use words as much to persuade as to communicate, and, as we do so, much of the meaning of our words escapes from our control. Finally, the social collectivity, ignored in the classical model of subject and object, has come to be regarded as critical for the production of knowledge. One source of this is the later philosophy of Ludwig Wittgenstein, with its claim that language finds meaning by virtue of its use in specific "forms of life" (Bloor 1983). The language we use to describe the world seems to be continuous with our practical activities, an integral part of the actions by which we collectively accomplish our goals.

These philosophical perspectives have yielded important resources for constructivism, and I shall refer to them at various points in what follows. I choose, however, to emphasize the foundations of the movement in studies that were as much sociological as philosophical, and in arguments couched in an idiom that was quite different from that of Continental philosophy. This is because it seems to me that the important move was not so much a change in philosophical outlook as a break of the link that had tethered empirical studies of science to the concerns of classical epistemology. It was primarily by setting aside the attempt to evaluate the credentials of scientific knowledge as rational, methodologically sound, or true to reality that a new space was opened up for research into its creation.

Constructivism was inaugurated by a determination to explain the formation of natural knowledge without engaging in assessment of its truth or validity. This is the position that Bloor called "naturalism": It accepts as "science" for the purpose of study what has passed for such in the context under discussion (1976/1991: 5). It is best understood as a pragmatic or methodological deployment of "relativism" in the service of a comprehensive study of human knowledge. It is a way of screening out the issues of epistemic validity that hinder the understanding of knowledge in its social dimension. Those who have followed this approach have noted how frequently questions of what methods characterize proper scientific research, or what is "good" and what is "bad" science, are intensely disputed in the settings they have studied. It therefore seems inappropriate for the analyst to take sides in these disputes. The ways in which boundaries are drawn between "science" and "pseudoscience," for example, or the way in which hierarchies are created to place "hard" sciences (such as physics) above "soft" (human or social) sciences, are important topics for research; but, to understand the creation of these relationships, the researcher should maintain a neutral stance toward all the contending claims. This methodological principle is also frequently called the *symmetry postulate*.

The postulate is a crucial one because any enterprise that seeks to understand science as a cultural formation has to embrace a wide range of different kinds of knowledge. If we are to understand how astronomy disentangled itself from astrology in seventeenth-century Europe, for example, or how modern Chinese medicine has assimilated elements of traditional and Western therapeutics, it is necessary to put aside attempts to demarcate what is scientific from what is not. We cannot assume that the historical changes we are concerned with were dictated by our own notions about what defines a "science." An open mind on that issue is an important precaution for the historian.

Since the symmetry postulate is primarily motivated by a desire to set aside issues of epistemology, it is unfortunate that it has regularly been attacked as a species of philosophical relativism. One *can* of course assert relativistic claims in a metaphysical or ontological way, saying, for example, that there is no such thing as "truth," or that all beliefs about nature are equally valid, or that there is no "reality" to the material world. But such statements encounter severe difficulties if defended as absolute claims; they lead, as can readily be shown, to self-contradictions. More pertinently, the constructivist outlook does not depend upon them. A pragmatic espousal of "methodological relativism" does not rest upon commitment to all of the absolutes that critics have identified with philosophical relativism (Bloor 1976/1991: 158). Thus, to say that judgments of epistemic validity should not provide the basis of explanations of why certain beliefs are held is not to say that no beliefs can ever be judged valid. To say that nature or reality should not be invoked as determinative of scientific belief is not to deny the existence of the real world or that it has some role in the production of knowledge (cf. Barnes and Bloor 1982; Barnes 1994; Fine 1996).

Understood as a methodological precaution, the symmetry postulate does not imply that questions cannot be asked about the differences between sciences, or between science and other forms of culture. Historians certainly want to explore what distinguishes the practices and social profiles of different scientific disciplines at different times in their development. But these problems can best be approached as topics for open-minded and nonevaluative inquiry. One should not assume that there is only one way for a subject to be "scientific" and only one path of development it can follow. The naturalistic perspective is a way to remind ourselves not to take the development of science for granted.

More can be said about the philosophical implications of the symmetry postulate, but I do not need to discuss it any further now. I simply want to note that, in terms of the development of constructivism, its importance lay in opening the way to a much wider range of empirical studies of natural knowledge in its many different contexts. Cutting the link to the preoccupations of traditional epistemology seems to have liberated naturalistic studies of the sciences, enabling them to explore topics and settings, and to develop approaches, that would not have been favored before. From the historian's perspective, the result has been a thoroughgoing *historicization* of science and all of its associated categories: discovery, evidence, argument, experiment, expert, laboratory, instrument, image, replication, law. These topics have increasingly been approached through research that treats them as historically problematic constructs, in need of contextual explanation, rather than through a priori philosophical analysis (Pestre 1995). As a result, philosophical discussion itself has been obliged to confront the findings of empirical research by sociologists and historians, and has been significantly enriched thereby. It is in this sense we can say, using the formulation that Bloor lifted from Wittgenstein, that interdisciplinary science studies are among "the heirs of the subject that used to be called philosophy" (Bloor 1983: chap. 9).

The transformed relationship between empirical studies of science and philosophical analysis has also been manifested in an increased emphasis on science as *practice*. The pioneers of the Strong Programme tended to take over from previous historical and philosophical studies the assumption that science was best regarded as a body of ideas. Philosophers such as W. V. Quine and Mary Hesse had proposed models of scientific knowledge as a network of interlinked concepts and beliefs. This seemed to explain how it could change in a way that would respond to new experimental findings, but would allow some freedom of choice in how they were accommodated. As constructivist inquiry has unfolded, however, analysts have increasingly tended toward an alternative view of science as a cluster of practices. Rather than a purely intellectual accomplishment, science is viewed as a set of activities in which people engage (Pickering, ed. 1992: 1–26). This shift in perspective owes something to the influence of phenomenology and hermeneutics, but it is more directly connected with the kind of approaches to the social sciences that those philosophies have inspired. Interpretive sociology and anthropology can more readily study what people can be observed to do, rather than what they think. Studies of science that have followed these approaches have tended to abandon the attempt to reconstruct conceptual structures, focusing instead on the practical activities that offer themselves to observation.

Of course, practitioners of the sciences do think, and some constructivist studies have taken a significant degree of interest in cognitive processes (e.g., Gooding 1990). But thinking itself can be regarded as a practical activity, intimately bound up with other kinds of doing; hence, the studies of topics such as the manipulation of materials and apparatus, the production and circulation of images, modeling and analogical reasoning, and all the many ways scientists communicate with one another – conversing, making presentations of results, writing grant applications, composing papers for specialist journals, and so on. Following the drift of naturalism, those exploring the construction of scientific knowledge have generally avoided presuppositions about which of these practices are really crucial and which are of secondary importance. Language use, for example, is not seen as merely communicating what is already known, but as one of the practices by which knowledge is constructed. Thus, the language used at the laboratory bench, in the lecture theater, in the journal article, and in the television documentary are all worthy of study; each serves as the means by which knowledge is produced in that location.

Much more contested than the naturalistic outlook or the tendency to focus on practice has been the issue of how to define the social dimension of scientific knowledge. Although the adjective "social" is frequently used to qualify constructivism, there is in fact no unanimity as to how the social element in the making of scientific knowledge should be specified or what explanatory role should be ascribed to it. The Strong Programme used the symmetry postulate as a wedge to make space for social explanations of the adoption of scientific beliefs. Bloor claimed that these explanations should show how beliefs could be traced to social causes, such as the structure of a scientific community or the interests of those involved. The historical case studies published by members of the Edinburgh school generally made claims that were both causal and *macrosocial*, tying individuals' beliefs to factors such as social class or location in a disciplinary community (Barnes and Shapin 1979).

On the other hand, the sociological studies of controversies, among which those of Harry Collins (1985) were preeminent, made no reference to such large-scale social forces. Collins reiterated the Edinburgh line that neither empirical evidence nor the canons of scientific method could make adoption of particular beliefs logically compelling, so that social causes must be decisive. But the social causes he invoked were on a much smaller scale than the Strong Programme had favored. Collins directed attention to the contingent judgments and negotiations made among small groups of scientific specialists. Controversies were said to be decided by the "core set," a small group of specialists who were closely concerned with the issue in question. Larger-scale social structures and interests (those of class, for example) were not shown to operate in these cases.

As the sociology of scientific knowledge built up further case studies, the trend away from macrosocial explanations was confirmed. Bruno Latour and Steve Woolgar's *Laboratory Life: The Social Construction of Scientific Facts* (1979/1986) was a pioneer in the field of ethnographical studies of specific research laboratories. Karin Knorr-Cetina (1981) and Michael Lynch (1984) were also among the first sociologists to engage in