5

The Strong Programme and the Sociology of Knowledge

The Strong Programme

In the 1970s a group of philosophers, sociologists, and historians based in Edinburgh set out to understand not just the organization but the *content* of scientific knowledge in sociological terms, developing the "strong programme in the sociology of knowledge" (Bloor 1991 [1976]; Barnes and Bloor 1982; MacKenzie 1981; Shapin 1975). The most concise and best-known statement of the programme is David Bloor's "four tenets" for the sociology of scientific knowledge:

1. It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. . . .
2. It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.
3. It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.
4. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. (Bloor 1991 [1976], 5)

These represent a bold but carefully crafted statement of a naturalistic, perhaps scientific, attitude toward science and scientific knowledge, which can be extended to technological knowledge as well. Beliefs are treated as objects, and come about for reasons or causes. It is the job of the sociologist of knowledge to understand these reasons or causes. Seen as objects, there is no *a priori* distinction between beliefs that we judge true and those false, or those rational and irrational; in fact, rationality and irrationality are themselves objects of study. And there is no reason to exempt sociology of knowledge itself from sociological study.

Since the strong programme STS has been concerned with showing how much of science and technology can be accounted for by the *work* done by scientists, engineers, and others. To do so, the field has emphasized Bloor's stricture of symmetry, that beliefs judged true and false or rational and irrational should be explained using the same types of resources. Methodological symmetry is a reaction against an asymmetrical pattern of explanation, in which true beliefs require internal and rationalist explanations, whereas false beliefs require external or social explanations. Methodological symmetry thus opposes a variety of Whig histories of science (Chapter 2), histories resting on the assumption that there is a relatively unproblematic rational route from the material world to correct beliefs about it. Whig history assumes a foundationalism on which accepted facts and theories ultimately rest on a solid foundation in nature (Box 2.2). As the problems of induction described so far show, there is no guaranteed path from the material world to scientific truths, and no method identifies truths with certainty, so the assumption is highly problematic. Truth and rationality should not be privileged in explanations of particular pieces of scientific knowledge: the same types of factors are at play in the production of truth as in the production of falsity. Since ideology, idiosyncrasy, political pressure, etc., are routinely invoked to explain beliefs thought false, they should also be invoked to explain beliefs thought true.

Although there are multiple possible interpretations of methodological symmetry, in practice it is often equivalent to agnosticism about scientific truths: we should assume that debates are open when we attempt to explain closure. The advantage of such agnosticism stems from its pushes for ever more complete explanations. The less taken for granted, the wider the net cast to give a satisfactory account. In particular, too much rationalism tends to make the analyst stop too soon. Many factors cause debates to close, for science to arrive at knowledge or technology to stabilize, and therefore multiple analytic frameworks are valuable for studying science and technology.

In addition to statements of the potential for sociology of knowledge, strong programmers Bloor (1991) and Barry Barnes (1982; also Barnes, Bloor, and Henry 1996) have offered a new and useful restatement of the problem of induction. The concept of "finitism" is the idea that each application of a term, classification, or rule requires judgments of similarity and difference. No case is or is not the same as cases that came before it in the absence of a human decision about sameness, though people observe and make decisions on the basis of similarities and differences. Terms, classifications, and rules are extended to new cases, but do not simply apply to new cases before their extension.

Actors and observers normally do not feel or see the open-endedness that finitism creates. According to Barnes and Bloor, this is because different kinds of social connections fill most of the gaps between past practices and their extension to new cases. That is, since there is no logic that dictates how a term, classification, or rule applies to a new case, social forces push interpretations in one direction or another. This "sociological finitism" (see Box 5.1) opens up a large space for the sociology of knowledge! As Bloor says:

> Can the sociology of knowledge investigate and explain the very content and nature of scientific knowledge? Many sociologists believe that it cannot . . . They voluntarily limit the scope of their own enquiries. I shall argue that this is a betrayal of their disciplinary standpoint. All knowledge, whether it be in the empirical sciences or even in mathematics, should be treated, through and through, as material for investigation. (Bloor 1991 [1976]: 1)

Box 5.1 Sociological finitism

The argument for finitism is a restatement of Wittgenstein's argument about rules (Box 3.2). Rules are *extended* to new cases, where extension is a process. Rules therefore change meaning as they are applied. Classifications and the applications of terms are just special cases of rules.

The application of finitism can be illustrated through an elegant case in the history of mathematics analyzed by Imre Lakatos (1976), and re-analyzed by Bloor (1978). The mathematics is straightforward and helpfully presented, especially by Lakatos. The case concerns a conjecture due to the mathematician Leonard Euler: for polyhedra, $V - E + F = 2$, where F is the number of faces, E the number of edges, and V the number of vertices. Euler's conjecture was elegantly and simply proven in the early nineteenth century, and on the normal image of mathematics that should have been end of the story. However, quite the opposite occurred. The proof seemed to prompt counter-examples, cases of polyhedra for which the original theorem did not apply!

Some mathematicians took the counter-examples as an indictment of the original conjecture; the task of mathematics was then to find a more complicated relationship between V, E, and F that preserved the original insight, but was true for all polyhedra. Other mathematicians took the counter-examples to show an unacceptable looseness of the category *polyhedra*; the task of mathematics was then to find a definition of *polyhedra* that made Euler's conjecture and its proof correct, and that ruled the strange counter-examples as "monsters." Still others saw opportunities for interesting classificatory work that preserved the original conjecture and proof while recognizing the interest in the counter-examples.

It seems reasonable to say that at the time there was no correct answer to the question of whether the counter-examples were polyhedra. Despite the fact that mathematicians had been working with polyhedra for millennia, any of the responses could have become correct, because the meaning of polyhedra had to change in response to the proof and counter-examples. On Bloor's analysis, the types of societies and institutions in which mathematicians worked shaped their responses, determining whether they saw the strange counter-examples as welcome new mathematical objects with just as much status as the old ones, as mathematical pollution, or simply as new mathematical objects to be integrated into complex hierarchies and orders.

Interest Explanations

The four tenets of the strong programme do not set limits on the resources available for explaining scientific and technological knowledge, and do not establish any preferred styles of explanation. In particular, they do not distinguish between externalist explanations focused on social forces and ideologies that extend beyond scientific and technical communities, on the one hand, and internalist explanations focused on forces that are endemic to those communities, on the other. This distinction is not perfectly sharp or invariant, nor are many empirical studies confined to one or other side of the divide.

Though the strong programme can cover both externalist and internalist studies, it was early on strongly associated with the former. When the strong programme was articulated in the 1970s, historians of science were having historiographical debates about internal and external histories, particularly in the context of Marxist social theory. The strong programme slid too neatly into the existing discussion.

Externalist historians or sociologists of science attempt to correlate and connect broad social structures and events and more narrow intellectual ones. Some difficulties in this task can be seen in an exchange between Steven Shapin (Shapin 1975) and Geoffrey Cantor (Cantor 1975). Shapin argues that the growth of interest in phrenology – psychology based on external features of people's heads – in Edinburgh in the 1820s was related to a heightened class struggle there. The Edinburgh Phrenological Society and its audiences for lectures on phrenology were dominated by members of the lower and middle classes. Meanwhile, the Royal Society of Edinburgh, which had as members many of the strongest critics of phrenology, was dominated by the upper classes. Reasons for these correlations are somewhat opaque, but Shapin claims that they are related to connections between phrenology and reform movements.

Cantor makes a number of criticisms of Shapin's study: (1) Class membership is not a clear-cut matter, and so the membership in the societies in question may not be easily identified as being along class lines. In addition, on some interpretations there was considerable *overlap* in membership of the two societies, raising the question of the extent to which the Phrenological Society could be considered an outsider's organization. (2) Shapin does not define 'conflict' precisely, and so does not demonstrate that there was significantly more conflict between classes in Edinburgh in the 1820s than there had been at some other time. (3) While the overall picture of membership of the two societies may look different, they had similar percentages of members coming from some professional groups. To make this point vivid, Cantor calls for a social explanation of the *similarity* of composition of the two societies. A correlation may not be evidence for anything.

While Cantor's criticisms of Shapin are specific to this particular account, they can be applied to other interest-based accounts. Many historical studies follow the same pattern: They identify a scientific controversy in which the debaters on each side can be identified. They identify a social conflict, the sides of which can be correlated to the sides of the debate. And finally, they offer an explanation to connect the themes of the scientific debate and those of the social conflict (e.g. MacKenzie 1978; Jacob 1976; Rudwick 1974; Farley and Geison 1974; Shapin 1981; Harwood 1976, 1977).

Exactly the same problems may face many internalist accounts, but are not so apparent because the posited social divisions and conflicts often seem natural to science and technology, and so are more immediately convincing as causes of beliefs. Conflicts between physicists with different investments in mathematical skills (Pickering 1984), between natural philosophers with different models of scientific demonstration (Shapin and Schaffer 1985), or between proponents of different methods of making steel (Misa 1992) involve more immediate links between interests and beliefs, because the interests are apparently internal to science and technology.

Steve Woolgar (1981) has developed a further criticism of interest-based explanations. Analysts invoke interests to explain actions even when they cannot display a clear causal path from interests to actions. To be persuasive, then, analysts have to isolate a particular set of interests as dominant, and independent of the story being told. However, there are indefinitely many potential interests capable of explaining an action, so any choice is underdetermined. Woolgar is criticizing "social realism," the assumption that aspects of the social world are determinate (even if aspects of the natural world are not). Woolgar, advancing the reflexive part of the strong programme, points out that accounts in STS rhetorically construct aspects of the social world, in this case interests, in exactly analogous ways as scientists construct aspects of the natural world. STS should make social reality and natural reality symmetrical, or should justify their lack of symmetry. Actor-network theory attempts the former (see Chapter 8; Latour 1987; Callon 1986). Methodological relativism adopts the latter strategy (Collins and Yearley 1992).

Interest-based patterns of analysis thus face a number of problems: (1) analysts tend to view the participants in the controversy as two-dimensional characters, having only one type of social interest, and a fairly simple line of scientific thought; (2) they tend to make use of a simplified social theory, isolating few conflicts and often simplifying them; (3) it is difficult to show causal links between membership in a social group and belief; and (4) interests are usually taken as fixed, and society as stable, even though these are as constructed and flexible as are the scientific results to be explained.

Despite these problems, STS has not abandoned interest-based explanations. They are too valuable to be simply brushed aside. First, interest explanations are closely related to rational choice explanations, in which actors try to meet their goals. Rational choices need to be situated in a context in which certain goals are highlighted, and the choices available to reach those goals are narrowed. The difficult theoretical problems are answered in practical terms, by more detailed and cautious empirical work. Second, researchers in STS have paid increasing attention to scientific and technical cultures, especially material cultures, and how those cultures shape options and choices. They have emphasized clear internal interests, such as interests in particular approaches or theories. As a result, researchers in STS have shown how social, cultural, and intellectual matters are not distinct. Instead, intellectual issues have social and cultural ones woven into their very fiber. Within recognized knowledge-creating and knowledge-consuming cultures and societies this is importantly the case (see Box 5.3), but it is also the case elsewhere. Third, while situating these choices involves rhetorical work on the part of the analyst, this is just the sort of rhetorical work that any explanation requires. Woolgar's critique is not so much of interests, but is a commentary on explanation more broadly (Ylikoski 2001).

Box 5.2 Testing technologies

A test of a technology shows its capabilities only to the extent that the circumstances of the test are the same as "real-world" circumstances. According to the finitist argument, though, this issue is always open to interpretation (Pinch 1993a; Downer 2007). We might ask, with Donald MacKenzie (1989), how accurate are ballistic missiles? This is an issue of some importance, not least to the militaries and governments that control the missiles. As a result, there have been numerous tests of unarmed ballistic missiles. Although the results of these tests are mostly classified, some of the debates around them are not.

As MacKenzie documents, critics of ballistic missile tests point to a number of differences between test circumstances and the presumed real circumstances in a nuclear war. For example, in the United States, at least until the 1980s when MacKenzie was doing his research, most inter-continental ballistic missiles had been launched from Vandenberg Air Force Base in California to the Kwajalein Atoll in the Marshall Islands. The variety of real launch sites, targets, and even weather was missing from the tests. In a test, an active missile is selected at random from across the United States, sent to Texas where its nuclear warhead is removed and replaced by monitoring equipment, and where the missile is wired to be blown up in case of malfunction, and finally sent on to California. To make up for problems introduced in transportation, there is special maintenance of the guidance system prior to the launch. It is then launched from a silo that has itself been well maintained and well studied. Clearly there is much for critics of the tests to latch on to. A retired general says that "About the only thing that's the same [between the tested missile and the Minuteman in the silo] is the tail number; and that's been polished" (MacKenzie 1989: 424). Are the tests representative, then? Unsurprisingly, interpretations vary, and they are roughly predictable from the interests and positions of the interpreters.

Trevor Pinch (1993a) gives a general and theoretical treatment of MacKenzie's insights on technological testing. He points out that whether tests are prospective (testing of new technologies), current (evaluating the capabilities of technologies in use), or retrospective (evaluating the capabilities of technologies typically after a major failure), in all three of these situations projections have to be made from test circumstances onto real circumstances. Judgments of similarity have to be made, which are potentially defeasible. It is only via human judgment that we can project what technologies are capable of, whether in the future, in the present, or the past.

Knowledge, Practices, Cultures

From the perspective of many of its critics, the strong programme rejects truth, rationality, and the reality of the material world (e.g. Brown 2001). It is difficult to make sense of such criticisms. The strong programme does not reject any of these touchstones, but rather shows how by themselves truth, rationality, and the material world have limited value in explaining why one scientific claim is believed over another. In order to understand scientific belief, we need to look to interpretations and the rhetorical work to make those interpretations stick (Barnes et al. 1996).

For other critics, the strong programme retains too much commitment to truth and the material world. Strong programmers (e.g. Barnes et al. 1996) reject what are sometimes seen as idealist tendencies in STS. It may be that arguments on this issue weaken STS's commitment to methodological agnosticism about scientific truths – probably the strong programme's largest contribution to the field. Whether this is right remains an open question.

Finally, the strong programme has been criticized for being too committed to the reality and hardness of the social world: it is seen as adopting a foundationalism in the social world to replace the foundationalism in the material world that it rejects. As Woolgar argues, there is no reason to see interests as less malleable than anything to be explained. The critique of interests has been amplified by arguments that interests are translated and modified as scientific knowledge and technological artifacts are made (Latour 1987; Pickering 1995). Society, science, and technology are produced together, and by the same processes – this results in a "supersymmetry" (Callon and Latour 1992).

Since the 1970s, the pendulum in STS, and elsewhere in the humanities and social sciences, has swung from emphasizing structures to emphasizing agency (e.g. Knorr Cetina and Cicourel 1981). The strong programme was often associated with structuralist positions, though its statements leave this issue entirely open. Thus as a philosophical underpinning for STS the strong programme has been supplemented by others: constructivism (e.g. Knorr Cetina 1981), the empirical programme of relativism (Collins 1991 [1985]), actor-network theory (e.g. Latour 1987), symbolic interactionism (e.g. Clarke 1990), and ethnomethodology (e.g. Lynch 1985). These positions are briefly described in other chapters. For the most part, this book avoids anatomizing theoretical positions and disputes, except as they directly inform its main topics, but readers interested in philosophical and methodological underpinnings might look at these works and many others to see some of the contentious debates around foundational issues.

Box 5.3 Pierre Bourdieu and an agonistic science

The French anthropologist Pierre Bourdieu (1999 [1973]) argued that we can understand scientific achievements as resulting from the interplay of researchers on a scientific field. Bourdieu introduced the term *cultural capital*, as a counterpart to *social capital* (a term that his use helped to make common) and economic capital. Actors come to a field with a certain amount of each of these forms of capital, and develop and deploy them to change their relative status within the field. This is true even of scientific fields: Bourdieu says, "The 'pure' universe of even the 'purest' science is a social field like any other, with its distribution of power and its monopolies, its struggles and strategies, interests and profits." Action in a field is agonistic.

All scientific moves of an actor are simultaneously moves on the field: "Because all scientific practices are directed toward the acquisition of scientific authority . . . , what is generally called 'interest' in a particular scientific activity . . . is always two-sided" (Bourdieu 1999). Every idea is also a move to increase capital; it is artificial to separate the pursuit of ideas from the social world.

There are, then, many different strategies for increasing capital. For example: "Depending on the position they occupy in the structure of the field . . . the 'new entrants' may find themselves oriented either towards the risk-free investments of succession strategies . . . or towards subversion strategies, infinitely more costly and more hazardous investments which will not bring them the profits . . . unless they can achieve a complete redefinition of the principles legitimating domination."

Bourdieu's work is isolated from other work in STS, and has been picked up by relatively few researchers. However, we can see his agonistic theory of science as respecting the principles of the strong programme in an individualistic way, explaining the content of science as the result of actions of individual researchers.

The strong programme provided an argument that one can study the content of science and technology in social and cultural terms, providing an initial justification for STS. As the field has developed, scientific and technological practices themselves have become interesting, not just as steps to understanding knowledge. Similarly, science as an activity has become an important and productive locus of study. Key epistemic concepts – such as experimentation, explanation, proof, and objectivity – understood in terms of the roles they play in scientific practice, have become particularly interesting. The strong programme's attention to explaining pieces of knowledge in social terms now seems a partial perspective on the project of understanding science and technology, even if it is a crucial foundation.