# IF YOU'RE THINKING OF LIVING IN STS / A Guide for the Perplexed / David E. Hess

CULTURAL AND ANTHROPOLOGICAL STUDIES of science and technology in the United States have become something of a growth industry in the 1990s. The list of North American anthropologists interested in science, technology, and computing issues now includes more than two hundred names.* The topic is covered in growing numbers of panels on the programs of the American Anthropological Association and the Society for Social Studies of Science as well as in a burgeoning number of publications. Yet anthorpologists and their siblings in cultural studies who move into this area sometimes assume that they will be living in a remote village that no one else has ever studied. It does not take long before they begin bumping into others who claim authority as students of science and technology and who may also expect anthropologists to prove that they have something new or interesting to say. In this essay I provide in somewhat idiosyncratic terms a partial map of STS (science and technology studies) that focuses on researchers and research likely to be of interest to readers of this book.

The discussion takes the form of a critical literature review, but it is rooted in several years of field work. As an anthropologist I have done fieldwork among Spiritist intellectuals in Brazil and various alternative medical and scientific groups In the United States, and in the process I have negotiated theories and frameworks from the social studies of science and cultural anthropology I have also lived for the better half of a decade in one of the leading departments of science and technology studies in the United States, where I have negotiated the interdisciplinary intersection of anthropology with STS. As a result, I can offer a perspective of both "insider" and "stranger."

## STS and SSK

"STS" is usually token to mean science, technology, and society studies, although on occasion it is glossed as science and technology studies. At Rensselaer and some other schools the faculty tend to think of STS as an interdisciplinary field with constituent disciplines in the anthropology, cultural studies, feminist studies, history, philosophy, political science, rhetoric, social psychology, and sociology of science and technology. In North America STS Is organized at a professional level around a number of disciplinary societies, each with its own acronym and affiliated journal. Among the major organizations are the History of Science Society (HSS, Ms), Philosophy of Science Association (PSA, Philosophy of Science), Society for the History of Technology (SHOT, Technology and Culture) Society for Literature and Science (SLS, Configurations), and Society for Social Studies of Science (4S, Science, Technology, and Human Values). Usually the societies hold their annual meetings separately, but occasionally two or more convene for joint meetings. There is also a Society for Philosophy and Technology with an annual volume titled Research in Philosophy and Technology, and in 1993 yet another organization was formed, the American Association for the Rhetoric of Science and Technology (AARST). That list covers only the major North American organizations. Probably the most relevant institutions for social scientists outside North America are the European Association for the Study of Science and Technology (EASST) and the European (but not EASST) journal Social Studies of Science.

My forthcoming book (Hess 1997b) provides an overview of some of the key concepts in the major constituent disciplines of STS, including the philosophy of science, the Institutional sociology of science, the sociology of scientific knowledge, critical/feminist STS, and cultural/historical studies of science and technology. There are also several other reviews of various aspects of the Interdisciplinary field (Fuller 1993; Rouse 1991, 1996b; Webster 1991; Woolgar 1988b; the review articles in Jasanoffet al. 1994). Traweek (1993) has provided perhaps the most comprehensive overview of the field for those Interested In anthropological, feminist, and cultural studies in the United States.

In this essay I will focus on the particular branch of STS known as the "sociology of scientific knowledge" (SSK), its relations to anthropology and ethnography, and the role of anthropology and cultural studies In shaping the future of the interdisciplinary STS dialogue. Given the prominence of SSK in this dialogue, it usually is not long before a newcomer encounters its texts and members. Furthermore, because there is a tradition of "anthropological" or "ethnographic" studies within SSK, it should be of particular Interest to anthropologists.

The "core set" of SSK members, according to Malcolm Ashmore's (1989-16-19) reflexive sociological study of SSK, includes Ashmore, Barry Bames, David Bloor, Harry Collins, Nigel Gilbert, William Harvey, Jon Harwood, Karin Knorr-Cetlna, Bruno Latour. Michael Lynch, Donald MacKenzie, Michael Mulkay Andrew Pickering, Trevor Pinch, Jonathan Potter, David Travis, S^eve Woolgar and Steven Yearley. Of course, conjuring up a network or school and naming its main members is problematic. As Ashmore himself recognized, other names could be added to his list. Candidates would include Wiebe Bijker, Michel Cal-

Ion Dovid Edge, ]ohn Law, and Brian Wynne. Conversely, some of the people on the list might not classify themselves as part of SSK. For example, m an article published after Ashmore's study, Lynch (1992) distinguishes between SSK and his own program of ESW (ethnomethodological studies of work in sciences and mathematics) ,

Furthermore, the term "SSK" is now somewhat out of date. Given the subsequent "turn to technology" and "practices" in what was originally known as "science studies" (Pickering 1992; Woolgar 1991a), the subfield might better be called SSKP or SSKT. Many outsiders also refer to the group not as SSK but as "constructivists"; however, the term "constmctivism" or "social constmctivism is not universally accepted within the group and there are many people not affiliated with SSK who accept some version of the social construction of knowledge and technology or the co-construction oftechnoscience and society. Within the SSK point of reference, constmctivism or social constmctivism may reler more narrowly to the programs associated with Michael Mulkay and his students as well as with continental Europeans such as Knorr-Cetina and Latour.

As the attentive reader has already noticed, almost all the SSK members are men. Most are British; a few ore from other countries, mostly in western Europe. Corridor talk of the interdiscipline suggests that many of them have scientific or technical backgrounds, and several passed through the British polytechnics rather than the elite Oxbridge system. 1 have heard that their apparent proclivity toward theory, programs, and acronyms was influenced by their socialization in the polytechnics, but it is also similar to the use of jargon in philosophical circles Their non-elite background has sometimes been used to explain their hostility to the traditional philosophy and history of science of the elite universities. I have heard the suggestion that the entire debate between SSK and the traditional philosophies of science Is shaped by the cultures of the British class system; a similar dynamic may be at work in the US in the opposition between STS programs, which are often housed in technical universities, and the more traditional history and philosophy of science programs.

Certainly the SSK social scientists view themselves as radicals, if only epistemologlcal ones, and in the 1970s and early 1980s they were the Young Turks of thesociology, philosophy, andhistory of science. Overtime it seems, the Young Turks have become silverbacks (to mix metaphors) and they now find themselves occupying what is in some ways a conservative position with respect to the increasingly international, diverse, and politicized field of science and technology studies.

Corridor talk or folk sociological theorizing on SSK can only go so far; it soon runs into the problem of internal diversity that undermines generalizations of the type made in the previous paragraphs. Perhaps a better way of generalizing about SSK is to say that its members share a belief that knowledge and artifacts are socially shaped or "socially constructed," a central rubric that, as a kind of Burkean God term, might best be left undefined. In addihon to the belief in some version of the social shaping or construction of knowledge and technology, one often encounters a shared origin narrative that positions the SSKers against several Others, usually positivist/Popperian philosophers, internalist historians, and institutional sociologists of science (sometimes erroneously lumped together as "Mertonian" and sometimes with overtones suggesting the vulgarity of ugly American empiricism). These Others all would and do contest the SSK narrative. Furthermore, the SSK origin narrative varies from person to person and from context to context, and those variations constitute significant rhetorical resources that mark internal identities. For the purposes and space limitations of this essay, however, I construct one narrative that gives an overall flavor of SSK. If pressed, I could locate shreds and patches of this narrative throughout the SSK literature.

## An SSK Narrative

In the 1920s and 1930s Kari Mannheim (1966) extended the project of a sociology of knowledge as it had been handed down from ancestors such as Marx, Durkheim, and Weber. However, Mannheim suffered a loss of nerve and ruled out social studies of the content of science (in other words, its theories, facts, methods, and so on). In subsequent decades Robert Merton (1973) built a sociology of^dence that focused on institutions and social structure but left the content in a black box. Merton assumed that the knowledge-production process was governed by the institutional norms of universalism, communality, organized skepticism, and disinterestedness, and by technical norms such as a concern with evidence and simplicity. In effect, he saw the content of knowledge production as objective and asocial, and he left theorizing about content to the philosophers.

Thomas Kuhn's The Structure of Scientific Revolutions (1970) helped pave the way for the new sociology of science in the form of SSK by stirring up waves in the philosophy and history of science. However, Kuhn soon backed away from the radical philosophical implications of his research, and today many regard him as something of a traitor to his own cause who may have even impeded the development of a thoroughly sociological approach to the study of scientific knowledge. Several researchers (e.g., Restivo 1983) also argued that Kuhn's work was similar to that of Merton in fundamental ways and not nearly as revolutionary as some had claimed. Nevertheless the black box of content had been opened, and soon the new sociologists of science were finding other, more reliable precedents. For example, Ludwik Fleck's Genesis and Development of a Scientific Fact (1979) is now seen as a precursor to Kuhn, and SSK researchers often point to a tradition of conventionalist accounts of knowledge within the philosophy of science. Most frequently mentioned is the controversial Duhem-Quine thesis of underdetermination, which holds that theories can be maintained in the face of contradictory evidence provided that sufficient adjustments are made elsewhere in the whole theoretical system (e.g., Knorr-Cetina and Mulkay 1983:3).

In the 1970s a group of primarily British sociologists completed the dismantling of the legacies of Mertonian sociology and positivist/Popperian philosophy. For example, Barnes and Dolby (1970), Mulkay (1976), and others showed the nonnormative nature of Mertonian norms; Collins (1975) showed how replication rested on social negotiation; and in Knowledge and Social Imagery, first published in 1976, Bloor (1991) articulated the "strong program" in the sociology of scientific knowledge. Thus, by the mid-1970s sociology of science had witnessed a dramatic shift from the Mertonian paradigm to the SSK paradigm.

The narrative of a dramatic rupture or paradigm shift has been hotly contested. Institutional sociologists of science have pointed out that the dismantling of Mertonian norms began with a paper by Merton (1957) that marked the transition to reward and stratification studies in the American sociology of science The May 1982 issue of Soda; Studies of Science was devoted to a debate between Merton's student Thomas Gieryn and the SSKers over the extent to which the strong program was new or worth pursuing. Likewise, in "The Other Merton Thesis," Zuckerman (1989) argued that Merton's early work on Protestantism and science anticipated constructivism in his discussion of shifts of foci of inquiry and problems within and among sciences. Philosophers of science were even more contentious: many argued that the new sociology of scientific knowledge did not have the revolutionary philosophical implications sometimes claimed for it; rather, SSK led to a radical relativism and philosophical incoherence (e.g., Hull 1988; Laudan 1990).

At the heart of the strong program were four controversial principles: (1) causality: social studies of science would explain beliefs or states of knowledge; (2) impartiality: SSK would be impartial with respect to truth or falsity, rationality or irrationality, or success or failure of knowledge (and, presumably, technology); (3) symmetry: the same types of cause would explain true and false beliefs, and so on (in other words, one would not explain "true" science by referring it to nature and "false" science by referring it to society); and (4) reflexivity: the same explanations that apply to science would also apply to the social studies of science.

The symmetry principle Is probably the most important tenet of the strong program. Bijker (1993), following Woolgar (1992), has characterized the intellectual history of the sociology of science in terms of progressive extensions of the symmetry principle: from Merton's symmetry between science and other social Institutions to Bloor's symmetry between types of content to later developments that argue for symmetry between science and technology, the analyst and analyzed, humans and machines, and the social (context) and technical (content).

An early version of empirical research related to the strong program was interests analysis, associated with Bames, MacKenzie, and (at that time) Pickering and Shapin. They, like Bloor, were at Edinburgh and are sometimes referred to collectively as the Edinburgh school. The interests studies explained historical controversies in science by reference to interests ranging from the Habermasian to the more identifiably Marxist conflict of classes. In several of the more notable studies, the scholars explained two rival theories by referring them to two conflicting social networks that in turn were related to class antagonisms (see Bames and Shapin 1979; Bames and MacKenzie 1979).

The interests approach soon encountered a number of criticisms even from within networks that were broadly friendly to the SSK project. From the perspective of laboratory- or interview-oriented methods, the historical studies of the Edinburgh school suffered from problems of interpretation. In Chubin and Restivo'S (1983:54) phrase, interest theory seemed to explain "everything and nothing-and [did] so retrospectively." Perhaps even more damaging was a detailed criticism from Woolgar (1981b:375), which included the memorable complaint that science studies had almost returned to the original sin of

Mertonianism except that "instead of norms we have interests." A debate erupted in the STS journals, after which discussions of class interests took on a deddedly retro flavor (Bames 1981; Gallon and Law 1982; MacKenzie 1981, 1983,1984; Woolgar 1981a, 1981b; Yearley 1982). The debate is significant because today the analysis of how class or macrostructural interests shape the technical content of science and technology has largely disappeared from the SSK agenda. Instead, the concept of interests survives in a slightly different form via the actor-network analysis of how scientists and other actors can create interest in their work, to be discussed below.

Another of the early empirical research programs is sometimes called the Bath school. In effect, the Bath school is Harry Colllns, but it is also associated with his collaborator Pinch and his student Travis. Collins accepted the symmetry principle of the strong program but was less enthusiastic about some of the other principles (Ashmore 1989). His "empirical program of relativism" (EPOR) postulated three stages for the analysis of controversies: (1) documenting the "interpretive flexibility" of experimental results, that is, showing how a number of positions were possible among the "core set" of actors in a scientific controversy; (2) analyzing the mechanisms of "closure," or showing how the core set came to ah agreement, such as through a social process of negotiation of replication; (3) relating the mechanisms of closure to the wider social and political structure, a problem that Collins (1983) tended not to tackle and instead relegated to Edinburgh-style interests analysis. Subsequently, Pinch and Bijker (Bijker, Hughes, and Pinch 1987; cf. Bijker 1993) extended EPOR to technology via the "sodal construction of technology" (SCOT) program that posited a similar series of stages moving from "relevant social groups" to "stabilization."

A third area of research in SSK involved field studies of laboratories, sometimes called "laboratory ethnographies" and usually assodated with construetivism proper. Latour and Woolgar's Laboratory Life, first published in 1979, introduced a number of significant new concepts. Perhaps most influential was the analysis of fact construction as a rhetorical process that involves increasing deletions of markers of the social origins of the fact. In other words, the idea of a fact can be interpreted as a deletion of "modalities" that qualify a given statement. Facts were then viewed as historical outcomes of a process of movement across "types" of facts ranging from conjectures that are connected to specific people and contexts to anonymous, taken-for-granted knowledge of the sort that is found in textbooks or that everyone merely assumes to be true. As facts move from the former to the latter, the connection with their producers and social contexts is progressively deleted. The study also developed the related "splitting and inversion" model of the discovery process, in which "discoveries" were invented, then split from their inventors, and finally inverted to be seen as products of a real, natural world rather than the social world of their inventors. Furthermore, the study presented a modification of economic models of scientific behavior that saw scientists as investors of credibility and reapers of credit.

Knorr-Cetina's Manufacture of Knowledge (1981) developed the idea of the construction of knowledge in somewhat different terms. She used the metaphors of fabrication and manufacture to portray the constructed nature of the "discovery" process in the laboratory. She pointed to the locally situated nature of knowledge production, in which inquiry and products were "impregnated" with indexical and contingent decisions. She also presented a critique of the concept of scientific communities as well as of market models (for which the "market" Involved similar assumptions about a community) and posited the Idea of trans-sclentlfic fields.

Mulkay and students such as Gilbert, Potter, and Yearley developed a related area of SSK known as "discourse analysis" (e.g., Mulkay, Potter, and Yearley 1983). Their studies demonstrated how sdentists' accounts of their actions varied considerably overtime and across genres or registers (such as conversations, letters, and reports). As a result discourse analysts could destabilize accounts of sderice that rested on one type of Informant's account, such as reports or biographies. They also argued that by falling to study the full range of variability of participants' accounts, social scientists would naively take over native accounts and make them their own. At least some of their destabilizing studies were directed at fellow SSK accounts from Edinburgh and Bath.

Other students of Mulkay, most notably Woolgar and Ashmore (1988), developed the "reflexive" tenet of the strong program. Essays in the reflexivist vein attempt to Inscribe the constructed nature of constructivist accounts In their texts. The more theoretically interesting reflexive studies have turned constructivism back on itself to explore philosophical and theoretical paradoxes. Ashmore (1989), for example, did meta-analyses of attempts to replicate Collins's replication finding as well as variable accounts of discourse analysts regarding the variability of sdentists' discourse. Woolgar (1983,1988b) explored the paradoxes of what he called the "reflective" or naive view of the relationship between sdentists' accounts and the out-thereness of reality, which SSK researchers rejected only to have it reappear in their practice. As in some discussions of reflexlvity in anthropological fieldwork, the theorization of reflexivity In SSK tended not to consider reflexivity in broader social terms that include the relations between discursive communities (Hess 1991,1992).

The actor-network approach of Gallon and Latour returns, in a sense, to the naturalistic flavor of the earlier Bath and Edinburgh studies (see Gallon 1980, 1986; Gallon and Law 1982; Latour 1983,1987, 1988). Actor-network analysis views the truthfulness of knowledge and the success of technology as the outcome of processes ofsodal negotiation and conflict that involve marshaling resources via sociotechnical networks that in turn produce changes in sodety. Thus, unlike sodal constmctivism, in which the context of sodety (either macro or micro) shapes the content of sdence and technology, the actor-network analyses point to the "seamless web" or co-construction of technosdence and society. (This form of analysis may therefore be better termed "constmctivism" in contrast to "social constmctivism.")

The political process of knowledge/technology construction is conceptualized through yet a new set of terms, which in a very rough and preliminary way can be glossed as follows: the problematization of the issues that forces others to go through one's own network as an "obligatory passage point"; the interessement of other actors that locks them into roles defined by one's own program; enroHment strategies that interrelate the roles that one has allocated to others; and the mobilization of the spokespersons of the relevant social groups to make sure that they continue to represent or control their constituencies (Callon 1986). Networks are heterogeneous conglomerations of "octants": people, institutions, and things, all of which have agency in the sense that they generate effects on the world. In general, the concept of heterogeneous networks has been highly influential, although American social historians of technology are more likely to refer to a similar theorization by Thomas Hughes. Hughes's work brings yet another concept to the study of networks: the concept of "reverse salients," or bottlenecks that stall the expansion process (see, for example, Bijker, Hughes, and Pinch 1987).

A much misunderstood point, which Gallon clarified in a conversation with me, is that his framework does not ascribe agency to things but instead focuses on the ways in which agency is attributed or delegated to things. In this way he provides a counterargument to the criticism I raised with him that his theory involves a version of reification, commodity fetishism, or even animism (see also Lotour 1992). To the extent that actor-network analyses do indeed examine attributions of agency, the framework provides a point of contact with a cultural perspective more familiar to anthropologists, because the analysis of attributions in case studies could be tilted in the direction of a methodology that enters into the cultural world of the people involved. By studying the historical processes by which people grant nonhumans a degree of agency, such as conferring the legal status of the person on a corporation, it is possible to bring out the critical potential of Gallon's approach to agency.

Through the actor-network approach, the SCOT program, and other developments, SSK has diversified in recent years toward the study of technology and of science in society. As a result, SSK has come closer to issues that are of concern in "post-Mertonian" American sociology of science (e.g., Cozzens and Gieryn 1990; Nelkin 1992) as well as the "social worlds" approach of the American sociology of Anselm Strauss. Students of the latter approach have creatively blended their own sociological tradition with SSK frameworks (see Clarke and Fujimura 1992; Fujimura 1992). Likewise, American ethnomethodologists have produced careful analyses of conversation and texts that have led to collaboration and dialogue with the discourse analysis/reflexivist tradition within SSK (Lynch 1985; Lynch and Wcolgar 1985). Some philosophers, such as Steve Fuller (1993), who edits the journal Social Epistemology, have also developed a dialogue with SSK. The expansion of SSK and fuzziness of the boundaries is evident in Pickering's edited volume Science as Practice and Culture (1992), which even includes an American feminist and anthropologist, Sharon Traweek (1992). It is to the question of anthropology and ethnography, and its construction within SSK, that I now turn.

## Theorizing Knowledge: The Anthropologist as Resource

In a book review in Current Anthropology of an "anthropological" study of science, the sociologist Steve Wbolgar (1991b:79) asks, "What is 'anthropological' about the anthropology of science?" Although he admits that the ethnography under review repairs some of the "descriptive inadequacies" of the laboratory studies, he finds that it lacks "theoretical purchase." Woolgar then defines his own version of an approach that is recognizably "anthropological," which I shall outline later in the essay. Although I am not entirely comfortable with Woolgar's definition, I am here interested less in disputing his argument than in the phenomenon of a British sociologist writing in an American anthropology journal and telling us what anthropology is, using as text or touchstone a book in the "anthropology" of science that was written by Australian researchers who may not be anthropologists themselves.

To understand the phenomenon, it is necessary to begin with the point that anthropologists are latecomers to STS conversations. Of course there is a long and rich history within anthropology of studies of material culture, ethno-knowledges, culture and medicine, technology and evolution, magical and rational thought, and the social impact of technology in the development context. However, it was not until the 1980s and 1990s that anthropologists in significant numbers began to study contemporary, cosmopolitan science and technology and to take part in the interdisciplinary STS conversation. In contrast, in SSK there is a relationship with anthropology and ethnography that dates back to the 1970s. The role of anthropology/ethnography in the construction of SSK is another important aspect of STS that anthropologists and cultural studies scholars will soon encounter, and it warrants further inspection because the possibilities for misunderstanding are very high.

One early example of anthropology as a resource in SSK appears In "Homo Phrenologicus: Anthropological Perspectives on an Historical Problem," by Steven Shapin (1979a). As occurred with the Annales school, the (then) "Edinburgh-school" historian borrowed anthropology to write better history. (1 put "Edinburgh-school" in quotation marks because Shapin did his graduate work at Penn, where, according to my colleague and Penn graduate Tom Carroll, faculty and graduate students were combining anthropology and history independently of the Edinburgh school.) Why use anthropology to do a better history or sociology of science? Shapin answered as follows: "Cultural anthropologists have not been so frequently or so deeply committed to the forms of culture they have studied as have historians of science." Anthropologists might question the attribution of a lack of commitment; many of us in some way have shown deep commitment to political issues in the countries where we have lived. However, Shapin seems to be thinking less of anthropology's politically engaged side than of the image of the cultural relativist as a neutral, outside participant observer who, like an extraterrestrial, tries to make sense of radically different ways of life. Shapin therefore draws on a version of anthropology that could help historians to escape from their hagiographic tendencies; it could help them to think about science and technology as profane-that is, as not set apart from society.

What was Shapin's anthropology? He turned to the British school of Horton, Firth, Beattie, and Douglas, and he examined their different positions on the relationship between social structure and ideas, including both neo-Frazerian intellectualism and versions of functionalism. He then articulated those positions with a framework informed by Barnes's (1977) development of interest theory. The result was a sensitive portrait of the relationship between nineteenth-century Scottish phrenology and Scottish society. SSK researchers today would probably fault the essay for the unproblematic use of interest theory or the unproblematized division between knowledge and society. Anthropologists reading the essay today might fault Shapin for remaining within the narrow confines of British social anthropology without exploring alternatives posed by

American cultural anthropology, French structuralisms, or other anthropological research traditions. Nevertheless, the essay remains a competent application of anthropological theory to a history of science problem, especially for the tone when it was written. Furthermore, because Shapin located the heterodox science in a historical context of changing class relations, he made it possible to put on the agenda macrosociological issues involving class and power. Those questions have been largely lost in a number of subsequent strands of SSK research.

Another way in which anthropology entered into the construction of SSK involved more explicit uses of the principle of cultural relativism. Collins's empirical program of relativism, for example, used the term "relativism" as a heuristic to signal his stance of neutrality in the face of opposing native (scientific) views of true and false knowledge. That usage certainly was similar to cultural relativism, although when applied to science it can be Interpreted as endorsing epistemologlcal relativism. In general, the Impartiality and symmetry principles of the strong program came to be associated with anthropology's principle of cultural relativism. As Woolgar and Ashmore (1988:18) noted, "The espousal of a relativism traditionally associated with cultural anthropology enabled the social study of science to treat the achievements, beliefs, knowledge daims, and artifacts of subjects as socially /culturally contingent." - -

As a resource, then, not only did anthropology provide a theory of knowledge/society relationships (as in the Shapin paper), it also provided a metaphor of cultural relativism to aid in the application of the principles of the strong program. By imagining sciences as foreign cultures and themselves as anthropologists, sociologists and historians were able to describe their relativist position-epistemological, cultural, moral, or otherwise-regarding the content of scientific knowledge. At the same time, however, SSK researchers tended to be fuzzy on distinctions among the types of relativism, and consequently they became vulnerable to criticisms from philosophers who insisted that at least some variants of SSK self-destruct in the contradictions of social idealism and epistemological skepticism. (On the types of relativism and their relationship to constructivism, see Hess 1995:chap. 1; 1997b:chap. 2.)

Anthropology also served as a resource for SSK in the more general sense of providing a metaphor for the excitement that the SSK researchers felt as intellectual pioneers in the study of the content of science and technology. They became heroic explorers of test-tube jungles. For example, Latour and Woolgar began their classic Laboratory Life (1986:17) with an anthropological metaphor that Is found throughout the SSK literature:

> Since the turn of the century, scores of men and women have penetrated deep forests, lived in hostile climates, and weathered hostility, boredom, and disease in order to gather the remnants of primitive society. By contrast to the frequency of these anthropological excursions, relatively few attempts have been made to penetrate the intimacy of life among tribes which are much nearer at hand.

Armed with their colonialist and masculinist metaphors, much in the tradition of Carolyn Merchant's (1980) portrait of Fronds Bacon, the SSK researchers were ready to "penetrate" the secret of the content of science that Merton, like a good Puritan, had left modestly covered.

Anthropology also provided a method or, more accurately, a metaphor of method. Indeed, this use of anthropology came to displace the theoretical use as seen in Shapin's essay (1979a), and "anthropology" came to be synonymous with "ethnography." For example, in Laboratory Life, Latour and Woolgar developed the argument that historical studies (such as the Edinburgh school interests research) suffered from the limitation of having to rely on scientists' own statements about their work. An anthropology of science as a form of "ethnographic" observation provided a better alternative:

> Not only do scientists' statements create problems for historical elucidation; they also systematically conceal the nature of the activity which typically gives rise to their research reports. In other words, the fact that scientists often change the manner and content of their statements when talking to outsiders causes problems both for outsiders' reconstruction of scientific events and for an appreciation of how science is done. It is therefore necessary to retrieve some of the craft character of scientific activity through in situ observations of scientific practice. (Latour and Woolgar 1986:28-29)

Thus, whereas historical studies suffered from the problem of having to rely heavily on scientists' own retrospective accounts, in the laboratory studies sociologists were able to observe for themselves the unmasked and unclothed content of science.

In Science: The Very Idea, Woolgar (1988b:84) explained in more detail what the ethnography of sdence Involved as a method. Usually, the ethnographer takes a menial position in the laboratory and works there for eighteen months until becoming "part of the day-to-day work of the laboratory." In other words, in the Malinowskian tradition, one comes in off the library veranda of archives or surveys and instead lives with a people for a sustained period of time. Woolgar described the ethnographer's task as one of note-taking, interviewing, and collecting documents. Those descriptions of ethnographic method are likely to be familiar to anthropologists; however, another aspect of Latour and Woolgar's construction of ethnographic method, the stranger device, is apt to be less so.

In Laboratory Life as well as in Woolgar's Science; The Very Idea, Latour and Woolgar were concerned that laboratory culture was too familiar, a problem that anthropologists who work in cultures unlike their own are less likely to face. Because of the cultural proximity of sdentists and SSK researchers, Latour and Woolgar became preoccupied with going native and accepting uncritically the accounts of scientists about their work. In order to demonstrate the social construction of knowledge, Latour and Woolgar wanted to achieve distance from the sciences and sdentists under study, and they appealed to the idea of "anthropological strangeness" for that sense of distance. They cited as their theoretical inspiration a 1944 essay by the phenomenologicol sociologist Alfred Schutz (1971). Although in Science; The Very Idea, Woolgar (1988b:84) noted that "'ethno-graphy' means literally description from the natives' point of view," he added that the sdentists' point of view "must be perceived as strange." "just as in any good anthropological inquiry," Woolgar wrote, "the ethnographer of science must bracket her familiarity with the mundane objects of study and resist at all times the temptation to go native" (1988b:86). The hoped-for result was a demystification of science. As Latour and Woolgar (1986:29) wrote, "Paradoxically, our utilization of the notion of anthropological strangeness is intended to dissolve rather than reaffirm the exoticism with which science is sometimes associated."

For anthropologists who study non- or semi-Western cultures and who have been, like me, confronted with practices such as spirits who perform surgery, achieving a sense of strangeness or distance is not a problem. Rather, the trajectory tends to be in the opposite direction: to take ideas and practices that educated Westerners would describe as irrational and show how they form a coherent system once the different set of assumptions is understood. However, that trajectory is only half the journey. As Marcus and Fischer emphasize in Anthropology as Cultural Critique (1986), understanding other cultures provides a vantage point for critical inspection of the values and assumptions of Western culture (including modem science). In contrast, Laboratory Life starts with an assumed rationality for Western science, then exoticizes it through the stranger device, and,finally reveals a gap between the assumed rationality of the scientists' self-representations and a nonrattonal or other-rational practice that is revealed through observation. Rather than showing the hidden rationality of the scientific Other, Latour and Woolgar show the hidden irrationality of the scientific Self.

In the other laboratory studies, different aspects of anthropology as ethnography served as a resource. Knorr-Cetina (1981) used anthropology to help pose an alternative to the "frigid" methodologies of data collection in sociology and psychology (a metaphor that, like her use of "impregnated" above, I flag in contrast to "penetration" to suggest possible feminist resonances in her work). The frigid methods, Knorr-Cetina argued, rely on the questionable assumption that the meanings of scientists' language can be taken at face value. In their place she called for a more sensitive sociology that would achieve "an intersubjectivity which does not as yet exist." She suggested that this more sensitive sociology could "be found in a return to the anthropological method of participant-observation," and she described the history of anthropology as involving "progressive attempts to establish intersubjectivity at the core of the ethnographic encounter" (Knorr-Cetina 1981:17).

Collins and Pinch (1982) articulated a similar view in their "ethnography" of science, Frames of Meaning. They began the introduction to their book with a discussion of the rationality debate. Framed in terms of a "relationship between different cultures" that are likened to Kuhnian paradigms, the distance between the social scientist and the scientist is likened to a divergence between two cultures. Like Knorr-Cetina and unlike Latour and Woolgar, Collins and Pinch viewed the problem as one of achieving understanding across different scientific cultures rather than going native by taking scientists' statements at face value. Collins (1994:383) also showed concern with the stranger device and the means by which "ethnographers" of science may obtain an "estranged viewpoint."

For Collins and Pinch, the problem was not achieving strangeness and distance but instead achieving competence in another scientific culture. Achieving competence in turn involved both practical and theoretical difficulties. As a practical problem, the jobs of both the sociologist and the scientist are full-time positions that require years of socialization. As a theoretical problem, the sociologist never becomes an entirely native member of the other scientific discipline and consequently may be prevented "from understanding native members both by virtue of his untypical array of competences and by virtue of his position as sociologist/outsider with regard to the native community" (Collins and Pinch 1982:20). Sustained fieldwork in the culture of the scientific Other was the solution proposed by the Bath school, which espoused an interpretive sociology that in some ways was reminiscent of Geertzian cultural interpretation (Collins 1981). As in Geertzian cultural Interpretation, the Bath school's position did not imply that the goal was to accept uncritically scientists' accounts as their own; understanding the Other's world was instead a prerequisite to a more theorized account of that world (cf. Mulkay, Potter, and Yeariey 1983; reply by Pinch and Collins in Collins 1983).

To summarize, the understanding of anthropology, ethnography, the ethnographer-informant relationship, and related concepts was by no means uniform across the various members and texts of the S.SK school. Their understandings also changed over time. For example, Latour (1990a: 146) admitted that the first laboratory ethnographies, including his own work, "used the most outdated version of anthropology." Likewise Woolgar (1982,1988a, 1988b:91-95; Woolgar and Ashmore 1988) drew on subsequent discussions related to the "new ethnography" in anthropology, including the SAR seminar that produced Writing Culture (Clifford and Marcus 1986), to advance his own version of reflexive ethnography as the "second generation" of the ethnography of science that would replace the older "instrumental" ethnography. (Our current SAR seminar may someday be seen as an exemplar of yet another generation of ethnographic studies of science and technology.)

In Leviathan and me Air Pump, Shapin and Schaffer (1985) also showed some significant developments in comparison with Shapin's (1979a) essay on phrenology. They opened the historical study of Boyle and Hobbes with a distinction between the accounts of "members" of a culture and those of "strangers." In order to move away from self-evidence, they followed Latour and Woolgar in contrast to Collins and Pinch. They noted that in Laboratory Life Lotour and Woolgar were "wary of the methodological dangers of identifying with the scientists they study." Their position contrasted with that of Collins (1981:6), who argued "that only by becoming a competent member of the community under study can one reliably test one's understanding." Shapin and Schaffer (1985) argued that "we need to play the stranger," because the stranger to the experimental culture Is in the position of "knowing" that there are alternatives. Finally, after noting that "of course we are not anthropologists but historians," Shapin and Schoffer provided a method for playing stranger to the experimental culture.

At a theoretical level, Leviathan and the Air Pump deconstructs the laboratory/society division in ways similar to Latour's post-Laboratory Life work on Pasteur (Latour 1983,1988). Shapin and Schaffer show that Boyle was building not only a laboratory and an experimental method but also a new type of society that recognized a boundary between science and society. The argument is consistent with actor-network theory in general and with Latour's emphasis on the laboratory as a site for the coproduction of science and society. Latour (1990a) subsequently returned the favor to Shapin and Schaffer in a book review of Leviathan that called for an anthropology of science "without anthropologists." The review marks what is perhaps the final step in the SSK construction of anthropology and ethnography. In the review, Latour leaves the impression that SSK has done such a good job of appropriating anthropology that, as Modleski (1991) argues is the case for constructions of feminism without women, anthropologists are no longer necessary or interesting. Anthropology without anthropologists.

I will close this section with a simple question: Did they get it? Notwithstanding all the internal differences and the changes over time, there is a way in which the SSK laboratory studies and some of the related historical studies can be seen as a unity. This unity or doxa has to do with how those studies are all likely to appear "strange" to anthropologists who read them for the first time. As several other anthropologists have commented to me, when we read SSK laboratory "ethnographies" or the "anthropology of science" we have a sense that we are not reading ethnography or anthropology at all. For example, for me the question of whether one is a stranger or insider is less interesting than how the fieldwork begins to unravel connections among various cultural domains: exchange structures, funding flows, Institutional positions, theoretical allegiances and divergences, methodological preferences, and so on. In the SSK "ethnographies" there is little if any thick description or semiotfc analysis of local categories, contradictions, and complexities; there is little sense of cultivating informants, talking to people, finding out what they think, understanding their social relations, and analyzing the play of similarity and difference across domains of discourse and practice. In short, there is little if any culture. What tends to happen instead is that the sociological theories and (antl)phllosophical arguments upstage the stories and worlds of the informants.

By explicating this difference I do not mean to put down the achievements of the SSK laboratory studies, nor do I wish to engage in gratuitous boundary-work. The laboratory studies have produced theoretical arguments that merit consideration, even if they are ultimately rejected or reconstructed. However, the value that I place on those studies does not change my perception that the books do not read like anthropology. Anthropological ethnography is often more like a historical case study than a treatise in empirical philosophy or a sodal theory with fieldwork-based examples. The difference between anthropological and SSK ethnography could be a productive tension, but in order for that to be the case both sides would have to recognize first that the difference exists. When there is no mutual understanding and respect, anthropologists can experience SSKers as arrogant, dismissive, and imperialistic because they want to tell us what anthropology and ethnography are. The result can mean that anthropologists become just another of the excluded voices in the SSK conversation. I and other anthropologists have experienced this misunderstanding, and at the cost of slowed, blocked, or misunderstood publication and review (for examples in print, see Fleck 1994; Forsythe 1993a, 1994). Of course, as anthropologists become more integrated into STS networks, the process can go the other way (e.g., Gusterson 1992). It is to the question of looking at SSK from the other side of the mirror that I now turn.

## Other Voices: Toward Counternarratives

In the essay "The Critique of Science Becomes Academic," the radical Australian STS analyst Brian Martin (1993) examines a footnote in Harry Collins's book Artificial Experts: Social Knowledge and the Intelligent Machine (1990). The footnote reviews case studies in the sociology of scientific knowledge, and the usual suspects are rounded up: Collins, Harvey, Knorr-Cetlna, Latour and Woolgar, Pickering, Pinch, Shapin and Schaffer, and Travis. Martin takes Collins, and SSK in general, to task for citation practices that exclude radical voices. In their place, Martin provides a counternarrative that roots STS research in radical social movements: radical science, feminism, women's health, civil rights, environmental justice, peace, and so on. In providing another narrative for the history of STS, he also urges STS to forsake its current tendency toward professlonalization and to return to its roots in progressive social movements.

Brian Martin is one of the prominent voices in what I will call, for lack of a better name, "critical STS." The term seems least offensive to the largest number of people, and it has appeared in the literature in ways that explicitly link conventional radical agendas with feminist ones (see, for example, Restivo 1988; Restive and Loughlin 1987). Critical STS-which, again, is only one of the many neighborhoods of STS-is much less coherent than SSK; I would characterize it as a series of interwoven sodointellectual networks and countertraditions. There Is no closely integrated cocltation cluster, no single counternarrative, and no dialogue of clearly articulated programs with neat acronyms. Instead of appearing as a London men's club, in which vigorous but carefully chosen debates end with a good smoke being had by all, this branch of STS might better be likened to a querulous New York neighborhood In which there are many disciplinary transients and where many people do not know-or even want to know-their neighbors.

I think of the diversity of this wing of STS as a positive rather than negative feature, for diversity and anarchy may be one way to Insure the vitality of dissent that is at the core of democratic research. Here I echo Traweek (1992:433, 440) in her discussions of the positive aspects of diversity and diversification. Furthermore, by invoking critical STS as a counterpoint to SSK, I do not mean to imply that the relations between these two groups are entirely polarized. Still, there is considerable evidence that supports a conflictual characterization of the relationship: conflicts over naming a new 4S prize after a man or a woman, holding the 4S meetings at the same time as the meetings of the American anthropologists (a group that includes several feminists and profeminist men2), celebrating or condemning the supposed politidzation of the 4S, and deciding who controls the 4S board and the review process for journals and book series, not to mention what actually gets said in the book reviews, conference talks, essays, and books. I might also point to dtation practices and reviewer comments, which indicate mutual ignorance and at times mutual hostility.

What is this other neighborhood of STS like? I order this heterogeneity (and to some extent others do as well) in terms of clusters of people with related interests. Examples include, but are by no means limited to, the technology-and-society critics from Jacques Ellul (1964) to Richard Sclove (1995) and Langdon Winner (1986), and from feminist perspectives work on topics like reproductive technologies, such as Judith Wajcman (1991); feminist/critical philosophers of science such as Sandra Hording (1992), Helen Longino (1994), and Joseph Rouse (1996b); radical science studies frpm Hilary and Steven Rose (1976a, 1976b) to David Dickson (1984), Brian Martin (Martin et al. 1986), and Robert Young (1972, 1977); anttradst studies such as those by Robert Bullard (1990), Richard Lewontin, Steven Rose, and Leon Kamin (1984), and others gathered in Harding's The Racial Economy of Science (1993); radical work studies from Harry Braverman (1975) to David Noble (1984) for the workplace and Ruth Schwartz Cowan (1983) for domestic work; environmental and appropriate technology studies that followed in the decades after Rachel Carson's Silent Spring ([1962] 1987) and E. F. Schumacher's Small is Beautiful (1973); Third World and global perspectives such as Antonio Botelho (1993), Shiv Visvanathan (1991), and Richard Worthington (1993); and critical feminist and profeminist sociologists such as Adele Clarke and Theresa Montini (1993), Sal Restivo and Julia Loughlin (1987), and Susan Leigh Star (1991).

If I were to construct a narrative for this branch of STS, the ancestors would not be Mannheim, Duhem. or Fleck but instead—to name a few other dead white males who are frequently cited—Bemal (1939), Hessen ([1931] 1971) and Mumford ([1934] 1964); or, better, the intellectual precursors of antiracist and feminist science studies such as W. E. B. Dubois's Health and Physique of the Negro American (1906) and Simone de Beauvoir's Second Sex ([1949] 1989). Both Dubois and Beauvoir were studying biological ideas as constructions long before the idea became fashionable. Likewise, the "events" of the 1970s and early 1980s might be displaced from building a strong program to creating movements and related journals such as the British Society for Social Responsibility in Science (Science for People), Scientists and Engineers for Social and Political Action (.Science for the People), and Radical Science Journal (now Science as Culture),
as well as developing organizational sites such as the Radical Science Collective, the Rensselaer STS Department, and movement organizations such as the women's health movement organizations (see Clarke and Montini 1993). By the late 1980s and early 1990s, instead of a turn to technology I would posit a turn to race and gender or, more generally, culture-and-power perspectives that move away from foundational analyses rooted in a single dimension (such as class) to the interactions of race, class, gender, age, nation, sexual orientation, and other markers of difference, power, and hierarchy.

How are the two hundred-plus anthropologists and their siblings now working in cultural studies contributing to the STS dialogue, without reduplicating work already done in critical STS or SSK, not to mention any of the other disciplines and schools associated with STS? I suggest five interwoven strands that mark a distinctive anthropological/cultural studies contribution to STS. Perhaps the most obvious contribution of anthropologists has been our redefinition of ethnography as a research method and a way of knowing in general. The SSK "ethnographies" focused on the laboratory, addressed questions about theoretical issues in the sociology and philosophy of knowledge, and were the product largely of Europeans with training in sociology and philosophy. The anthropological ethnographies work with larger field sites such as transnational disciplines or geographic regions, address questions defined largely by a concern with various social problems (e.g., sexism, racism, colonialism, national/ethnic difference, class conflict, ecology) that are framed by hybrid feminist/cultural/social theories, and are much more the product of Americans with graduate training in anthropology. Traweek's ethnographic studies of physicists (this volume), based on over a decade of ethnographic fieldwork and substantial graduate training in anthropology (even if, as she has said modestly, she only has a green card), are often regarded as a landmark for the beginning of the second wave of ethnography.

A second contribution of the anthropology of science and technology has been to reframe research on the public understanding of science. Models based on how scientists protect their legitimacy through boundary-work, or on how expert knowledge can be most efficiently conveyed to a public that is sliding down the slippery slope toward antisdence and New Age occultism, have been modified by culturally rich accounts that show how nonexpert lay groups and geographically localized communities actively reconstruct science and technology, often with high levels of sophistication (for a review, see Hess 1995: chap. 6). Samples of this work include the reconstrurtion of medical knowledge (Martin 1994; Treichler 1991), workplace technologies (Hakken 1993), religiously relevant scientific theories (Hess 1991; Tourney 1994), theories of development (Escobar 1995), and environmental knowledge (Laughlin 1995).

Feminist anthropologists and cultural studies analysts have made a third contribution to STS by expanding feminist STS from the critique of reproductive technologies, the theorizatlon of standpoint epistemologies, and the analysis of career attainment patterns for women to a much more general study of the culture of science as female and the institution of science as patriarchal. For example, Studies by anthropologists Davis-Floyd (1992), Layne (1992), and Rapp (this volume) of reproductive/birth technologies provide a richness based on patient/user perspectives that was not evident in the first waves of feminist/STS critiques of reproductive technologies. Likewise, studies by Haraway (1989), Keller (1985), E. Martin (1987), Merchant (1980), and other feminists have brought semiotic, cultural, and related frameworks into STS accounts of the content of science as not merely constructed but gendered.

Closely interwoven with the third strand is the shift in the understanding of what the word "construction" means. Although SSK prided itself on opening the black box of the "content" of science and technology, the stories of content that emerge from SSK are themselves highly technical ones. Stories of content are often told in a causal sequence, in which contingent social factors "S" are variables that cause technical content "C": S->C, When content is conceived of in this way, it becomes difficult to discuss it in anything other than local, microsodological terms. From this perspective, broader markers of social difference such as dass, race, and gender become a problematic background set (BS) of social factors that only tenuously shape microsocial factors (MS): BS->MS-»C.

However, content can also be understood in a more anthropological sense. Consider an anthropological lineage of theories of cultural difference and meaning that runs from Boas, Benedict, Mauss, Peirce, and Saussure through Douglas, Dumont, Geertz, Levi-Strauss, Sahllns, and Turner, and on to the feminist, subaltern, and variously engaged "critics" of later generations. Rather than ask how class, gender, race, and so on serve as variables that shape science and technology, this tradition would ask what science and technology mean to different groups of people as marked by culturally significant categories of gender, class, race, and so on. Instead of opening only black boxes, one opens red boxes, pink boxes, purple boxes, brown boxes, and a rainbow of other boxes. The fundamental SSK insight that the technical is the social /political (like the old feminist adage that the personal is the political) is retained but recast in a different light. Divisions among facts, methods, theories, machines, and so on are seen as culturally meaningful and as interpretable in terms of locally constituted social divisions. In short, they are "technototems."

This relationship, unlike that of totemism as discussed in the SSK literature (Bloor 1982; Latour 1990a), opens the door to interpretive methods. Anthropology's culture concept via semiotic theories provides a new approach to the analysis of construction, one based on the interpretation of meaning rather than a sociological explanation of the content of science with reference to social factors or variables. To be clear, the two approaches—what I call cultural and social constmctivism—are complementary and work best when used together. The point is worth emphasizing because SSKers are already misinterpreting me to be advocating an acausal analysis; instead, I am showing how anthropology and related fields bring a symbolic/semiotic level to STS that complements the accounts of social constructivism.

Finally, anthropology and cultural studies have contributed to STS by shifting discussions of the posiUon(ality) of the researcher from reflexivity and policy (in the sense of how to manage science and technology) to issues linked to intervention, activism, and popular movements for social justice. This shift is taking place in a variety of ways illustrated in this volume, such as through theorizing intervention (Downey's partner theorizing, Heath's modest interventions), through studying scientists (Haraway's women primatologtsts [1989], or her comparison of Crouch and Hinchee), by analyzing technoscientific activism (as in Emily Martin's studies of AIDS activists and my own research on the alternative cancer therapy movement [1997a]), or by intervening in scientific controversies by helping one side get a hearing (Brian Martin 1996). The question of intervention and the problem of thinking about it in a rigorous way deserve more attention and, as I will argue, can benefit greatly from the resources of STS as a transdisdpline.

## Theorizing Intervention: The Sociologist as Resource

The tendency of many associated with critical STS is to make a blanket rejection of the ideas and arguments of SSK. The alternative considered here is to appropriate and reconstruct SSK as a resource in much the same ways that it appropriated and reconstructed anthropology. Two examples will suffice: the impartiality principle of the strong program and the analysis of networks.

A tempting move would be simply to reject the impartiality principle as a reinscription of the very positivism, value neutrality, and objectivity that at another level it attempts to put into question. The impartiality tenet is perhaps the most vulnerable of the strong program principles, and some critics have interpreted it as a continuation of the value-neutral social science tradition that most practitioners of critical STS, not to mention many in anthropology and cultural studies, have long left behind. The obvious question is: If the social studies of science and technology are supposed to be neutral or impartial regarding what counts as truthful knowledge or successful technology, how does one adopt an engaged position as a proponent of one side of a scientific or technological controversy? If the technical is the social/political, then this form of impartiality seems to imply political impartiality. But why should one buy in to impartiality, when science and technology often embody and legitimate social relationships that the researcher finds unjust? As Winner (1993:374) argues, "One must move on to offer coherent arguments about which ends, principles, and conditions deserve not only our attention but also our commitment."

Although I am sympathetic with this line of argument and agree with Winner and others who have challenged value neutrality as political indifference, 1 think there may be a way in which the impartiality tenet might be preserved under some conditions for use as a rhetorical resource in attempts at intervention. To understand those conditions, it is useful to refer to the literature on capturing in relation to neutrality. It has been noted that in many cases of polarized controversies, epistemologically balanced or "impartial" treatments of scientific debates are rarely interpreted as such. Woolgar (1983:254) also notes that when social scientists offer alternative accounts even in a rhetoric of neutrality, "the proffered alternative account will be heard as a comment on the adequacy of the original account." Moreover, neutral accounts will often lead to capturing by the proponents of controversies, usually by the ones with less authority (e.g., Hess 1993; Martin, Richards, and Scott 1991; Scott, Richards, and Martin 1990). In other words, the party with the lower credibility may seize a neutral account because it implicitly levels the playing field.

The theorizing on capturing suggests that in some circumstances a neutral account may be a more effective form of intervention than an engaged or positioned account. As a resource, then, neutrality or impartiality can be used strategically for more effective intervention. Although this argument by no means implies endorsing the impartiality tenet of the strong program, it suggests a way in which the strong program brings up ideas that can be useful for those concerned with intervention. Certainly this argument may have a more general application to considerations of the role of the social scientist in movement organizations.

For a second general example of the possibility for a fruitful dialogue between SSK and various projects of intervention and activism, consider an obvious and fairly frequent charge leveled against the actor-network approach: It tends not to ask why certain people are able to build successful networks and others are not. Structural issues regarding glass ceilings and the politics of exclusion are backgrounded or forgotten in a theoretical model that assumes a level playing field on which competing networks duke it out in a masculinist game that is somewhere between market competition and all-out warfare. As a result, actor-network theory seems largely irrelevant for those who are concerned with issues of fairness and justice.

However, as I read actor-network theory, I also think about my experiences in coalition politics, especially when I worked with the diverse progressive groups and complex identity politics of the San Francisco Bay Area in the late

1970s. Coalition politics are based on heterogeneous networks that seek to expand and make their truth flow through their networks and into the larger society. Likewise, as I have studied various groups of heterodox scientific and medical researchers-many of whom have good ideas that merit more inspection from the broader scientific community-1 am struck by the naivete of their sociology of science. They should all read Gallon, not to mention both Collinses (Collins 1985; Collins and Restivo 1983). Concepts such as enrollment and obligatory passage points can be useful as part of the package of tactics, strategies, and rules for radicals who go about organizing successful coalition politics inside and outside the citadel. If science is politics by other means, then coalition politics can be actor-networks with other ends. In other words, although actor-network theory has problems because of what is excluded from its analytical frame, some of its concepts can be of use for interventionist projects.

In short, critical STS analysts who are attuned to issues of power and culture (a general rubric that I prefer for issues such as gender, class, race, age, and so on) need to go beyond the strong program, but they should not reject it in a facile way. In my book on STS and its application to the evaluation of a medical controversy (Hess 1997a), I suggest that rather than explanation, impartiality, symmetry, and reflexivity, a set of rubrics that better describes a more viable program of critical/cultural studies of science and technology is power, culture, evaluation, and intervention.

First, the analysis is political; it explores the operation of power in the history of a field of knowledge that is constituted by a consensus and by attendant heterodoxies. For example, I (Hess 1997a) study several research trajectories on bacteria and cancer from a political perspective to show that a substantial body of research was systematically excluded- Intellectually suppressed, to use Brian Martin's phrase (Martin et al. 1986)-from what became mainstream cancer research.

Second, the analysis is cultural in the sense that it develops a sophisticated, noninstrumentalist explanation and explication of the dynamics of power that have been described in the first step. Although some researchers may prefer the term "sociological" or "social," the term "cultural" is used instead to flag a kind of analysis that does not reduce the explanation of consensus knowledge and heterodoxy to sociological variables and the explanation of power to what Marshall Sahlins (1976b) calls practical reason. In other words, it is far too easy to explain the history of repression and suppression as the result of a coalition of interested parties who act in a mechanical way to attain status, enhance symbolic capital, protect their interests, or simply gain and maintain power. Instead, Instrumental explanations are encompassed by a more complex interpretation of the growth of the autonomy of research cultures that respond with some internal integrity to theoretical developments and new research findings, ecological changes in the political economy, general cultural values involving standardization and gender, and cross-cultural Hows of patients and clinicians who support alternative approaches.

Third, the analysis is evaluative; it weighs the accuracy, consistency, pragmatic value, and potential social biases of the knowledge claims of the consensus and alternative research traditions. This step or principle assumes that a fully interdisciplinary STS analysis moves out of the traditional plane of social scientific analysis/critique (here formulated around the two strands of culture and power) to a prescriptive level. This level Involves two stages: the evaluation of knowledge claims and the evaluation of proposed policy or political changes. The evaluation of knowledge daims is necessary because of the capturing problem; it is accomplished in a heterogeneous manner that takes into account the cultural politics analyzed in the previous stages. The evaluation is based on the standards of the best scientific knowledge available at the time of the evaluator's analysis, but it also assumes that those standards may themselves be biased against the research under analysis due to the same political and cultural processes already analyzed.

Rather than provide an impartial or symmetrical analysis, I evaluate the content of the science itself from the philosophical perspective of constructive realism, that Is, a position that recognizes both the constructed and the representational aspects of knowledge. The view of knowledge is neither relativist (as for the Ideal typical radical constructivist, who does not allow for the power of the world to constrain evidence) nor algorithmic (as for the ideal typical naive realist, who believes that the crucial experiment can generally resolve disputes over evidence). Rather, the nature of knowledge is assumed to be more like that of the legal profession and the qualitative sodal sdences, in which evidence can be established but always within a social situation that recognizes the power of cross-examination and Interpretation. To establish criteria for evaluating the alternative research program, a wide range of sources in the philosophy ofsdence are used, Including the work of feminists such as Longino (1994).

Finally, the analysis Is positioned; it provides an evaluation of alternative policy and political goals that could result in beneficial institutional and research program changes. As a sodal scientist I therefore assume that I will be positioned inside the controversy, as the capturing literature demonstrates is inescapable, and that I am better off positioning myself rather than letting someone else do it for me. In the terminology of the STS field, this level of analysis can be described as a type of reflexlvity, but one that is more profoundly sociological or anthropological than previously discussed forms.

In short, an alternative to the strong program should move beyond a social sdentific analysis of sdence to the evaluation of competing knowledge claims: What alternative research traditions or theories are available or possible? Are they any good? If so, what kinds of institutional changes are necessary to move toward the alternatives? Yet moving beyond the strong program does not mean forgetting what it and SSK achieved; my argument is for a both-and rather than an either-or view of SSK and its Others in the neighborhoods of critical STS, cultural studies, anthropology, feminist studies, and so on. In arguing for this view, I hope I can make the interdisdplinary turf somewhat more inviting to readers who are thinking of living in STS or at least spending some time here. In constructing a map and countermap of SSK and making some articulations with anthropology, I have also been constructing a vision of a field that not only theorizes but also does more about exdusion, marginalization, hierarchy, and difference, including our own tendendes to reproduce those processes. That is the kind of community in which I would like to live.

Notes

I wish to thank Rayna Rapp for many helpful comments and for giving me the title, one for which I had a great affinity as a fellow New Yorker. I also wish to thank all the participants of the SAR seminar, as well as Brian Martin, Sal ResUvo, and Stewart Russell, for their criticisms and suggestions. I owe the use of the term "sllverbacks" In this essay to my colleague Roxanne Mountford, who introduced the term Into feminist circles at Rensselaer. According to Donna Haraway, it is also used in similar ways among primatologists.

1. The estimate is based on the current number of subscribers to the list moderated by loe Dumit ond run for CASTAC, the Committee for the Anthropology of Science, Technology, and Computing of the American Anthropological Assodation. To subscribe to this low-traffic list, send a message to LISTSERV@MITVMA.MIT.EDU with the following text: SUBSCRIBE CASTAC-L your name. In the post I edited a newsletter and list of publications by anthropologists interested in sdence and technology (TTic Anthropology of Science and Technology), but 1 discontinued the project after the list became available.

2. The term "profeminist" is sometimes preferred in the wake of male attempts at appropriation of feminism that results in a possible "feminism without women" (Modleski 1991).