Ethnography and the Development of Science and Technology Studies

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Because the term 'ethnography' has widely variant meanings across the disciplines, it should not be surprising that within an interdisciplinary field such as Science and Technology Studies (STS) the practices of fieldwork and the conventions of ethnographic writing also vary dramatically. This chapter will explore some of the differences between two 'generations' or networks of ethnographic researchers in the STS field, then discuss some possible standards for a good ethnography in the field. The heuristic of two generations provides a useful, albeit simplified, point of entry into the literature, its methods, and its theoretical frameworks.

Methodological Issues in the First Generation

During the early 1980s, social scientists (primarily sociologists) published several fieldwork-based studies that are sometimes referred to as the anthropology of science. The first generation of STS ethnographers included both Europeans and nonEuropeans (mostly Americans), but during the early 1980s the British dominated the field.1 Overall, the first generation occurred within a current of STS known as the sociology of scientific knowledge (SSK), which contrasted with the largely American sociology of science (or scientific institutions) associated with Robert Merton (1973) and colleagues. For SSK the central research concept was the social construction of knowledge, that is, the problem of how decisions about the credibility of knowledge claims and methods involve a mix of social and technical factors. The first generation of STS ethnographies tended to be defined in contrast with a naive view of scientific work as a purely rational process of representing a nature that revealed itself in transparent observations. The term 'rational' in this context suggests that universalistic, technical decision criteria such as concerns with evidence and consistency are the dominant shaping factors in the outcomes of controversies and other decisions regarding theories, methods, and knowledge claims in science. Instead, the SSK researchers emphasized the way in which concerns with evidence and consistency were interwoven with situationally contingent events, local decision-making processes, negotiation among a core set of actors in a controversy, the interpretive flexibility of evidence, additions and deletions of rhetorical markers (modalities) to knowledge claims, and other social or nontechnical factors that shape the outcome of what comes to be constituted accepted knowledge and methods in a field.

Notwithstanding the common ground of SSK ethnographies, there were substantial differences. For example, although this group of studies is known sometimes

as 'laboratory studies,' some of the ethnographies went beyond observations of laboratory science. Theoretical judgments about the nature of knowledge had implications for the choice of fieldwork site and method. For example, Collins's (1983a) emphasis on the role of community negotiation led to fieldwork in broader research communities rather than laboratories (e.g., Collins and Pinch, 1982) and to an interpretive method that he termed 'participant-comprehension' in contrast with the more positivistic term 'participant-observation' (Collins, 1983b). Collins and Pinch (1982: 20) were concerned the problem of achieving competence in the sciences of the field site; like anthropologists in a foreign culture, they viewed a core ethnographic problem to be achieving understanding across the different disciplinary cultures of the social sciences and the field site science. In contrast, Latour and Woolgar (1986) were more concerned with the rhetorical markers of the persuasion process that converted observations into widely accepted facts, and consequently their fieldwork focused on the laboratory and writing processes. They also were more concerned with the problem of going native, that is, accepting scientists' accounts of their work at face value. As a result, they emphasized the value of playing stranger to the experimental culture of the laboratory.2

Another major difference involved the changing conceptualization of the construction rubric. Over time the tradition of empirical studies of science took an increasing 'turn to technology' (Woolgar 1991), and concern with the coshaping of knowledge (or technology) and society displaced microsociological accounts. New terms such as 'coconstruction' or simply 'construction' tended to displace the older term 'social construction.' Research methods also tended to be based more on documentary sources and interviews than on fieldwork; however, fieldwork-based research in this tradition has continued to take place into the 1990s.3 Actor-network theory is an influential example of the increasing concern with technology and with the coconstruction problem (Callon 1986, 1995). Of significance for ethnographic method is the theoretical question of how nonhuman entities achieve a delegated agency within sociotechnical networks. A trivial but simple example is the role of a traffic light for in a busy intersection, which constitutes a sociotechnical network of pedestrians, drivers, police, traffic laws, vehicles, roads, crosswalks, etc. The light has a delegated agency that shapes human action in the system. A theoretical position on the agency of things will influence fieldwork choices about how to define a fieldwork site. Likewise, a well-chosen fieldwork site (such as the nocturnal traffic culture of urban Brazil) might lead to interesting theorizing of the cultural contingency of agency in sociotechnical systems.

In a few cases, researchers associated with the SSK ethnographies made excessive claims that suggested they believed that the consensus knowledge of a scientific field at any point in its history was solely the product of social factors. In other words, they suggested a plasticity to the interpretation of observations and production of evidence that left little room for the material world to intervene as a constraining force in scientific research or a decisive factor in the resolution of controversies. The excessive epistemological relativism of the radical versions of constructivism led to strong reactions from some philosophers and eventually from scientists of science wars fame. The latter tended to want to return to a preconstructivist era in which histories and ethnographies of science excluded consideration of the social shaping of content. It is probably fair to say that neither extreme is feasible to many in the STS community today. For example, the outcome of controversies is frequently shaped by battles of evidence;

thus, there is no doubt that a technical, universalistic decision criterion is influential and that the world has a kind of agency in decision-making of this sort. However, the ability to produce good evidence is shaped by research traditions that govern its interpretation, access to resources that govern its production, control over what counts as good methods, and the ability to mobilize rhetoric and colleagues to win arguments over the interpretation of data. Yet, even when taking such strong social factors into account, it is also the case that outgroups are sometimes able to defeat the orthodoxies of a scientific field based on higher quality evidence or more logical argumentation, even when the orthodox methods are used to judge such evidence and argumentation. Thus, a moderate view of constructivism suggests a both-and framework for interpreting the outcome of controversies and other scientific decision processes.

The Neutrality Question in STS

Some of the first-generation of STS ethnographies were informed by the basic methodological principles known as the 'strong program.' The program involved four basic principles: causality, impartiality, symmetry, and reflexivity (Bloor 1991: 7 [orig. 1976]). Causality meant that social studies of science would explain beliefs or states of knowledge. The impartiality principle held that social scientific accounts of science would be impartial with respect to the truth or falsity, rationality or irrationality, or success or failure of knowledge. The symmetry principle held that the same types of cause would explain both true and false beliefs; in other words, one would not explain 'true' science by referring it to nature and 'false' science by referring it to society. Reflexivity held that the same explanations of science would also apply to the social studies of science. Although the principles were formulated for SSK, presumably they could be extended to the study of technology.

Not all ethnographies of science were influenced by the strong program, nor were all of the principles equally influential. Latour and Woolgar made explicit and favorable reference to the strong program (1986: 105), particularly its principles of impartiality (149) and symmetry (23). Likewise, Collins and Pinch (1982: 17) adopted a position of impartiality regarding true and false beliefs in their study of a parapsychology controversy, and subsequently Collins articulated his own research program with the strong program's symmetry principle (1983a: 86; also 1996). Woolgar (1988) later developed the reflexivity tenet in relationship to ethnography. In contrast, for Lynch the overall orientation was ethnomethodological, and mention of the strong program was more as a point of comparison (1985: 200; 1992). Likewise, Chubin and Restivo (1983) developed an opposing 'weak' program that in some ways antedates the developments of the second generation of ethnography.

Although the question of influence is complicated, the strong program does provide a point of reference for the first generation, and the principles of impartiality and symmetry serve as valuable point of comparison between the first and second generations of ethnography in STS. As methodological principles, impartiality and symmetry proved to be, up to a point, valuable heuristics to guide empirical research projects, particularly those focused on the origins and outcomes of scientific controversies. In brief, the principles prevented a presentist type of explanation. For example, position A of a controversy won because it was based on the truth as we understand it today, whereas position B lost because it was biased by social factors. Although one might draw on

today's knowledge and conclude that advocates of position A may have indeed developed a more accurate map of the world, one cannot assume that the evidence for A was better at the time of the controversy, that arguments for the evidence for A were more persuasive, that evidence itself was the only factor that led to the closure of the controversy, or that today's knowledge may not be reversed at some later point in time. In practice, the principles of impartiality and symmetry led to more nuanced explanations of empirical material in which social and technical explanations were interwoven. In the context of ethnography, the principles invited--although did not always lead to--a perspective that began with the views of the scientists of the field site, rather than with categories imposed by the observing ethnographer. As starting points, the principles therefore had value in helping researchers to avoid some methodological pitfalls.

Notwithstanding the value and general influence of the impartiality and symmetry principles as methodological heuristics, the principles were at the heart of ongoing debates and criticisms. Some criticisms were largely internal to SSK and were the result of continuing attempts to extend the symmetry principle, such as to the analysis of humans and things mentioned above regarding actor-network theory (see Bijker, 1993, and the epistemological chicken debate in Pickering, 1992). However, the more profound criticisms came from outside SSK. For example, SSK researchers argued that they had opened the black box of the content of science, but critics charged that upon opening the black box, they had found it politically empty (Winner 1993) or that the strong program principles represented the academic depoliticization of STS's roots in activist struggles (Martin 1993). One reading of the symmetry and impartiality principles is that they underplay or even fail to make distinctions between the truth and falsity of scientific claims or the success and failure of technological designs. If one accepts the reading, then there are no grounds for making a decision about what course of action one ought to take, as in a policy recommendation. The broader topic of the politics of impartiality and symmetry has received substantial attention during the 1990s (e.g., Ashmore and Richards, 1996; Radder, 1998). In some ways the second generation of ethnography begins with the recognition that the task of ethnography cannot be limited to the objectivizing framework of pure description/explanation and to the politics of scientific and value neutrality.

Methodological Issues in the Second Generation

The second generation or network of ethnographic studies in STS has a different social address: there are more anthropologists, feminists, and cultural studies researchers in this network, and it has a more American flavor.4 Second-generation ethnographies have tended to be more oriented toward social problems (environmental, class, race, sex, sexuality, and colonial) in addition to theoretical problems in the sociology and philosophy of knowledge. Consequently, the second-generation tends to have a wider field site than the laboratory or core set of a controversy. Second-generation examinations of knowledge and technology also tend to go outside the citadel of expert knowledge to the viewpoints of lay groups, activists, social movements, the media, and popular culture; to examine the contours of orthodoxy and heterodoxy in a discipline's development, including the political, institutional, and economic forces that govern the selection of research fields and programs; and to examine variations in expert knowledge and technology across cultures. Consequently, the research tends to be 'multisited'

(Marcus, 1998; Rapp, 1999a), and ethnographic projects tend to require more time in the field. In fact, some of the projects span more than a decade of field research.

The concepts of culture and power (and the related family of concepts that includes gender, race, class, sexuality, and nationality) are generally more central to theoretical frameworks of the second generation than the concept of the construction of knowledge and technology. Although the claim that scientific knowledge is in some sense socially constructed is widely accepted, the claim no longer seems to require proof. Indeed, when one takes into account the broad comparative perspective that includes studies of an immense literature on non-Western knowledges and material cultures, it is clear that each society produces a knowledge about the world that encodes its cultural traditions even as it maps real structures and processes in the material and social worlds. 'Western science' is no different--for example, in the resonances of key concepts such as natural law, atomism, and evolution with similar concepts in the political and social systems (e.g., legislative law, individualism, and progressivism). It is probably more accurate to say that in the second generation the construction problem shifts from the SSK focus on how social and technical factors are interwoven in knowledge and technology production (social construction) or how sociotechnical networks and societies are mutually constituted (co-construction) to how cultural meanings or legitimating power relations are embedded in science and technology (cultural and political construction) and how different actors interpret science and technology (reconstruction).

Researchers in the second wave have tended to avoid the science wars problems that emerged in SSK partly because they often view the knowledge-culture relationship as both-and rather than either-or (Toumey, 1998). In other words, the cultural and political shaping of knowledge does not prevent it from also providing reasonably accurate maps of the world. For example, a hunter-gatherer people may have a complex mythological system that organizes categories of plant classification, but at the same time categories of plant classification follow empirical observations about structural and functional differences among species. The structures of both nature and culture codetermine knowledge; in other words, moderate or realistic constructivism is a starting, rather than ending, point of a research tradition. The view is not necessarily in conflict with the strong program; Bloor recognizes that "there will be other types of causes apart from social ones which will cooperate in bringing about belief" (1991: 7). However, the applications of the strong program emphasized social variables in their explanations.

A second point of comparison and contrast with SSK in general and the strong program in particular is the relationship between the principle of cultural relativism and the strong program principles of impartiality and symmetry. Just as the strong program principles suggest an analysis that begins with the frameworks of the participants of a field site or controversy--what Bloor (1991: 176) calls 'methodological symmetry'--so the methodological principle of cultural relativism holds that ethnographic research should begin with the point(s) of view on one's informants. However, ethnographers in the anthropological/feminist/cultural studies traditions are careful to distinguish the moment of cultural interpretation in the research process from the complete analysis. Analysis may begin with local interpretations and meanings, but it does not end there. In the process, the second wave of ethnographers tends to distinguish cultural relativism as a methodological heuristic from epistemological or moral relativism. Failure to engage in the 'stepping in' and 'stepping out' process constitutes 'going native,' which is usually

rejected as a departure from a completed analysis (Powdermaker, 1966; Forsythe, 2001). Like Collins and Pinch, the first concern is to understand how the world works from the point of view of one's informants, thus to achieve competence in the culture. The distancing or strangeness that Latour and Woolgar wanted occurs with the stepping back process of social scientific analysis of one's observations. In a way, a contrast in the first generation of ethnography comes together as two phases of a research project in the second generation.

The analytical half of second-wave STS ethnography implies asymmetry, and the most frequently given example is belief in supernatural phenomena. Social scientists and historians generally do not believe in supernatural phenomena, and they do not take supernatural forces into account in their explanations of, for example, witchcraft or sorcery as social phenomena. Likewise, Bloor recognizes a higher level asymmetry in the afterward to the second edition of *Knowledge and Social Imagery* (1991: 176). He argues that a sociological explanation of witchcraft--that is, as opposed to a supernatural explanation--"will logically imply that the witchcraft beliefs (taken at their face value) are false" (1991: 176). The logical asymmetry implicit in a sociological explanation of witchcraft is distinguished from the methodological symmetry of asking why members of a culture would choose the false belief--witchcraft is based on supernatural powers--over the true belief, witchcraft is not (p. 177). Bloor recognizes the problem of higher level asymmetry that arises from methodological symmetry, but his exploration of the implications of higher level asymmetry is limited.

Consider the complexities of the play of symmetry and asymmetry that occur in a social scientific explanation of the genesis and outcome of a scientific controversy. The explanation is inherently asymmetrical because it presumes that the social scientist's account can be, even if it is not always in fact, superior to the more limited explanations provided by most scientist-participants in the controversy. Participants generally have access to less complete technical and social information about the controversy than do post-hoc analysts, and they also do not have access to the accumulated science studies research on controversies. In this sense, scientists' accounts of controversies are like the traditional accounts of anthropologists' informants; they need to be analyzed in light of an accumulated, cosmopolitan base of research as well as all sources of knowledge local to the controversy. However, there is a difference in the asymmetries of a social scientific explanation of, for example, why one shaman defeats another and why one side of a scientific or technical controversy prevails. An emic explanation of the outcome of a shamanic conflict would hold that one shaman defeated another because the first had stronger supernatural power or access to stronger spirits. The emic explanation would not enter into the social scientist's account except to the extent that belief in the emic explanation had an effect on the outcome. By extension, one might argue that a social scientist's account of the outcome of a scientific controversy would not rely on emic explanations such as stronger evidence or logic except to the extent that belief in stronger evidence and logic had an effect on the outcome. Yet, this application of symmetry precludes the social scientist from making the claim that whereas one side of the controversy believed it had better evidence and logic, in fact it only had access to greater resources, better rhetoric, or more political clout. Whereas few if any social scientists would want to make a similar distinction for shamanism (e.g., one side had stronger

supernatural power versus stronger social clout), for the analysis of scientific controversies in a policymaking context such an ability should not be surrendered.

The higher-level asymmetry that I am defending goes together with a higher level partiality. At the second, higher level of analysis, when one reassesses all the evidence and argumentation, and puts it together with all the social factors, it is possible to arrive at the conclusion that the minority or lost position was in fact 'better.' Rejected technologies such as the gas refrigerator (Cowen, 1985) or rejected theories such as the infectious etiology of cancer (Hess, 1997a) may have been wrongly rejected, at least partially or in some circumstances, and there are defensible grounds for making that evaluation. One can ground the verdict on the very standards that were used to dismiss the lost choices, such as cost and efficiency for a technological choice or evidence and consistency for a research program choice. Such a strategy is the most convincing, but one can also move up a level of analysis to argue that the methods or standards of evaluation in place at the time were biased in favor of the status quo, and an alternative set of criteria that inverts the established orthodoxy would better serve a general public interest. The necessity of beginning an analysis with a principle of cultural relativism, which I have shown to have some parallels with the impartiality and symmetry principles, is therefore linked to the equal and opposite necessity of concluding the analysis with a framework that is partial and asymmetrical, and likewise that is grounded in an epistemological and moral antirelativism. The back-and-forth movement is essential if the social scientific analysis of science is to escape the incoherences revealed by critiques the strong program and to move on to contribute to policy debates of public importance.

What Constitutes a Good Ethnography of Science and Technology?

The ethnography of science and technology shares several features with other contemporary ethnographic projects, but it also has some relatively unique features. First, as has occurred with much contemporary anthropological ethnography (Marcus 1998), the traditional anthropological fieldwork narrative of the lone ethnographer who goes off to a remote village is clearly not appropriate. Fieldwork sites in the ethnography of science and technology are rarely remote, rarely disconnected from the world system, and frequently part of one's own society. Second, the ethnography of science and technology shares with contemporary ethnographic projects a new relationship with informants. As Michael Fischer (1998) has pointed out, in the traditional fieldwork model the ethnographer is the naive child or student who learns the culture from informants or teachers. In contrast, in ethnographies of emerging worlds the rapidly changing character of the field site(s) and sciences/technologies means that ethnographers and informants are groping together to understand what is going on. Third, there is usually an existing social science or historical literature on the science or technology in question, and ethnographers are challenged to produce something new against a backdrop of a pre-existing interdisciplinary social science literature. As occurs in, for example, medical anthropology, this epistemo-political situation will tend to drive the ethnography of science and technology toward a social science, as opposed to humanities, orientation.

In the STS context there are some additional twists that are less common in other contemporary ethnographic projects. As Forsythe (2001) noted, ethnographers are likely to be collaborating with informants who will read very carefully what they write. While the situation is shared with some other contemporary ethnographic projects, in the

science and technology context there are some cases in which ethnographers are also employed by their informants. Likewise, there is a much greater frequency in which informants or their colleagues serve as reviewers of the work of ethnographers. The situation creates the possibly that informants can directly restrict what the ethnographer can or cannot say. For example, Forsythe became involved in a legal battle over who owned her fieldnotes.

A second difference, at least of emphasis, between the ethnography of science and technology and some of the other contemporary ethnographic projects is that a social or cultural analysis is frequently taken as threatening in and of itself. Because the frameworks of the scientists tend to equate the 'social' or 'cultural' with the nonscientific or unscientific (that is, they assume an asymmetrical framework as a starting point), any attempts to show how their work is social and cultural will tend to be interpreted as a discrediting maneuver. In the context of heightened competition for funding and public support, such interpretations can lead to counterattacks on the ethnographer. Consequently, any sociocultural analysis of science will therefore tend to produce discomfort that could trigger the science wars.

How, then, does one assess the quality of an ethnography of science and technology? In the STS context, the term 'fieldwork' comes to include many points of exposure and triangulation: attending conferences (for the second wave of STS ethnographies, probably a preferred field site to laboratories), working in laboratories and schools, attending virtual chat rooms and real-world colloquia, interviewing a wide range of persons associated with the community, reading a vast technical literature, working in archives, developing long-term relationships with informants (who may, over time, become friends or even co-researchers), interviewing outsiders and laypeople about their perceptions of the expert community and its products, becoming a part of activist and social movement organizations, and providing services and help to the community (such as writing or lecturing on social, historical, or policy aspects of the community). Over time--generally at least two years of sustained contact but frequently five or ten years--a deep knowledge of the field community develops, so that the ethnographer achieves a rigorous standard of fieldwork quality. In George Marcus's phrase, the standard means 'being able to inform someone of your own community (scholarly and otherwise) what is going on in the frame of your project and fieldsite to the full extent of his or her curiosity' (1998: 18).

From the perspective of this standard of 'good ethnography,' the ethnographer develops near native competence in the technical aspects of the science and technology involved. The standard of near native competence does not mean that one necessarily could pass, for example, a general doctoral exam that covers a wide variety of subfields in, for example, biology. Rather, the technical competence of the fieldworker tends to be narrow band--limited to specific subfields--where one's control of the literature is equivalent to that of the experts and, in some cases, superior to it. (The latter circumstance occurs most frequently when one delves into the archives that are often unread by contemporary researchers, who may have a bias against reading literature that is more than five years old and therefore may not know how current controversies repeat old ones.) More generally, the standard of near-native competence means that good ethnographers are able to understand the content and language of the field--its terminology, theories, findings, methods, and controversies--and they are able to analyze

the content competently with respect to the social relations, power structures, cultural meanings, and history of the field. This is a high standard that often requires years of research.

In addition to a standard of competence, there are other criteria that should be included in a standard of a 'good ethnography' of science and technology. In the direction of the humanities, good ethnographies frequently interrogate or complexify the taken-for-granted, such as common-sense categories employed by social scientists, policymakers, activists, and scientists. Good ethnographies usually involve an element of surprise or subversion; the fieldworker finds phenomena, meanings, terms, practices, social relations, institutions, capital flows, culture-power connections, and so on that might not have been expected. Here, the ethnographic voice is one of thick description (Geertz, 1973), as in the work of historical interpretation or textual exegesis, although not necessarily restricted to the textualist limitations of Geertzian interpretive anthropology.

I also submit that good ethnographies are positioned explicitly with respect to a social science research tradition, either theoretical or empirical, and they move the tradition forward by providing new concepts and categories, new empirical findings, new explanations or explanatory models, or reasons for questioning unquestioned theoretical assumptions. The second, social science-direction is more evident in the classical ethnographic debates over, for example, kinship, but also in the more recent ethnographies that are situated in interdisciplinary social science research traditions such as social studies of medicine, science, and technology. There is a tension between the tendency to immerse oneself in the complexities of ethnographic detail and the tendency to produce an explicit contribution to a research tradition of theoretical models and empirical findings, but I would maintain that good ethnography can and should do both. In short, good ethnographies reveal competence, interpret complexity, interrogate the taken-for-granted, and make an explicit empirical or theoretical contribution to a literature.

Making Good Ethnography Better

Some ethnographers would argue that the standard described above is good enough. Can a mere contribution to the STS literature justify the tremendous investment of an intelligent, educated citizen, not to mention taxpayer dollars that might have supported the research project? An additional criterion for a good ethnography is that ethnographers develop ways of intervening in their field sites as a citizen-researchers and of making their competence applicable to policy problems. The concept of policy does not have to be restricted to government science and technology policy; following Beck (1997), the policy application may be more at the 'subpolitical' level of how scientific and technical communities might change practices to achieve goals such as increased participation from underrepresented groups.

As a social scientist who understands the relevant science and technology at a level close to or equivalent to the experts *and* who understands the social/cultural/political aspects of the field in ways that often surpasses the grasp of the experts in the field, the ethnographer has not only the unique opportunity, but also the civic obligation, to become part of the conversation about the relationship between the research field and the broader public that ultimately supports it. One therefore tends to find STS anthropologists speaking openly of 'intervention' and activism (Downey and

Dumit, 1997). Against this position some have criticized all talk of intervention or activism as sacrificing explanatory or interpretive rigor on the altar of politics. However, the issue should be seen as both-and rather than either-or. One can maintain a high standard of descriptive analysis while at the same time providing the grounds for making prescriptive recommendations for ongoing policy problems. Furthermore, grappling with policy and prescriptive issues often tends to clarify descriptive work.

In this way, a good second-generation STS ethnography can be described as postconstructivist. Rather than focusing on how knowledge and technology are socially constructed, the analysis examines ways in which they might be *better* constructed, with the criteria of 'better' defined explicitly and their contestability openly acknowledged as both epistemological and political. For example, what alternatives are there to the current configuration of the production of content in the science and technology in a specific field of study? Usually, research fields are polarized by controversies over roads not taken, over research programs that have become dominant while others have fallen into backwater status. The polarization of fields along lines of orthodoxies and heterodoxies is particularly true in the applied fields such as medicine, public health, agriculture, management, policy, education, and engineering. Often the connections are not obvious until one follows out the linkages between basic research and its applications.

Another approach is to ask similar questions about existing social institutions in science. For example, why are there so few women and underrepresented ethnic groups in most research fields in science, and what are the experiences of those who stay and leave? How do national research communities in a scientific field form a hierarchy, how do they relate to each other, and what is the experience of scientists in postcolonial societies? The institutional focus of the topic may appear to be old-fashioned to the SSK ethnographers, but here is another way in which a postconstructivist STS differs from its constructivist predecessor. The institutional or "Mertonian" side of science studies should not be rejected as a backwater or outdated paradigm. Indeed, it should be reconjugated with ethnographic research to reveal insights from the perspective of policy and intervention. For example, we now know that when underrepresented groups enter scientific fields, they tend to see biases of both theory and method that were not evident before, and they tend to lead innovations in the content of the field (Haraway, 1989). We also know that, in the U.S. at least, the educational process for technical fields such as engineering (Downey, 1998) involves socialization into a habitus that is most comfortable for white males and less so for women and members of underrepresented ethnic groups. Scientific fields such as artificial intelligence (Forsythe, 2001) and physics (Traweek, 1988) are not only dominated by men but also constructed around practices, slang, and methods that embody masculine values. Ethnographically based research of this sort suggests that policy discussions need to involve more than the pipeline problem; in other words, the gender and ethnic problem in the social composition of scientific and technical professions will not be solved by getting more underrepresented groups into the pipeline. Rather, good ethnography points the way to ideas for redesigning the pipe itself.

Intervention: Some Comparisons

Within the second generation of STS ethnography there is a tendency to move toward a prescriptive discourse that engages various types and levels of policy questions.

Although the concept of intervention is no more universally accepted in the second generation than symmetry and impartiality were in the first, intervention may have a comparable role as a point of reference. For example, the concept of intervention provides the framework for the introductory essay for the volume *Cyborgs and Citadels* (Downey and Dumit, 1997), which provides a prominent sampling of the second generation of ethnography in STS.

The scope and meaning of intervention as a central concept remains controversial. Eglash (1999b) suggests that the concept can be stretched too thinly, for example by arguing that a critique of theory--that is, a 'theoretical intervention' either within STS or within the science of the field site--might water down the concept of intervention to the point of inaction. Likewise, in a multisited ethnography of the Bhopal disaster and global environmentalism, Fortun (2001) queries the concept of intervention through her analysis of environmental advocacy. She suggests that the idealized ways of conceptualizing advocacy are inadequate because they underestimate the amount of uncertainty that advocates must confront. In environmental disputes such as Bhopal, advocates move in a world of dubious facts and ambiguous political alliances. Because a similar situation also characterizes most of science at the research front, as well as in many applied fields, her arguments can be generalized. As in the case of other ethnographers of this generation, Fortun played an active role in her field site; she provided her activist informants/partners with skills and labor in a mode that might be characterized as partnership-action or participant-action. However, as a writer-analyst she is skeptical of the prescriptive discourse that characterizes some of the other intervention projects in the second wave of ethnography. As she writes, 'Heroic images of scholars as activists without double-bind madden as much as they lure' (2001: Postscript.2).

Gary Downey and colleagues provide a model of intervention that involves positioning the ethnographer within a research community. Downey and Lucena describe 'hiring in' as involving 'a willingness on the part of social researchers to allow their work to be assessed and evaluated in the theoretical terms current in the field of analysis and intervention' (1997: 119). They regard 'hiring in' as a subcategory of various types of 'partner theorizing,' or short-term cooperative work relationships between ethnographers and, in this case, scientists or engineers (Downey and Rogers, 1995). Working in the belly of the beast creates opportunities to influence technical research and institutions directly, for example by challenging engineers to revise their curriculum to make it more friendly to a more diverse student body. However, at the same time Downey and Lucena recognize that the role creates 'complementary risks of cooptation and social engineering' (1997: 120).

Although Downey and Lucena suggest that 'hiring in' does not necessarily involve becoming the employee of scientists, the development did occur with Forsythe (2001). Her research demonstrates some of the dilemmas that can occur when 'hiring in' involves putting the ethnographer in the position of an employee of her scientist informants. Forsythe's early papers showed how the technicist assumptions of AI engineers led to the design of systems that could have been more successful if the engineers had had a more ethnographically grounded understanding of what knowledge is and how it can be elicited. Although a member of the SSK network attacked her critiques as ethnocentric and asymmetrical (Fleck, 1993), Forsythe was writing as a member of the AI lab who was engaged in ongoing dialogue with the 'boys' in the lab, who valued her

alternative perspective. The relationship was one of mutual criticism--often focused on gender issues--combined with mutual respect. As time went on, her work and that of other ethnographer colleagues became influential in the AI community, and eventually AI researchers adopted ethnographic methods in the design of expert systems. The development is most interesting from the perspective of a theory of ethnography as intervention and the unintended consequences that all historical action carries in its wake. Forsythe and colleagues won the battle and lost the war: ethnography became accepted in the AI field, but ethnography was redefined by the AI researchers. Furthermore, funding for her work dried up while ethnography by the 'natives' remained well-funded. The dual development led Forsythe to another level of criticism, in which she argued that the AI scientists' understanding of ethnography was colored by the same technicist assumptions that she originally documented for the AI culture, and therefore would produce similar failures.

Partner theorizing and hiring in belong to the same family of interventions that Heath (1997) characterizes as 'modest interventions.' As part of her fieldwork on a genetic disorder known as Marfan syndrome, Heath organized roundtable discussions at a conference that brought together researchers, clinicians, and advocates in an open-ended discussion (1997: 79; see also Martin, 1996). The encounter between her scientist-informant and frustrated patients created some tensions, and Heath found her scientist-informant somewhat annoyed by the threat to autonomy that the ethnographer's intervention had created. At the same time, the scientist-informant also saw her research in new light, that is, as embedded in a more complex social context that, when taken into account, could lead to shifts in research priorities.

A less modest approach to intervention (perhaps one might call it "immodest interventions") is developed in my own research project on alternative medicine, which brings ethnographic research to bear on a well-recognized policy failure: the war on cancer (Hess, 1997a; 1999; Wooddell and Hess, 1998). The project develops the issue of intervention around the concept of 'evaluation': how one should evaluate lost or suppressed therapies and research traditions, current clinical and research practices, and ongoing failures in regulatory and research policy. Situated alongside a social movement of clinicians, patients, and researchers who are advocating changes in cancer research and treatment, I might also be described as a partner theorist or advocate. As in other communities, the alternative cancer therapy community itself is quite diverse and even internally split on crucial issues, so there is no easy way to advocate policy changes from 'the' community's perspective. The focus on evaluation provides a model of how differences both within the alternative medicine community and between it and conventional medicine might be resolved in a more universalistic way that serves a broader public interest than current policies allow. Through ethnographic interviews, I crystallize the community's knowledge into a framework for opening up the evaluation question to a complex set of epistemological/policy proposals that better serves the broad public interests of patients and their clinicians. In addition to presenting such work in academic fora, I have tried to bring the ideas into the general public sphere of debate through trade books, radio interviews, networking with patients and activists, presentations at alternative medicine conferences, and literature supplied to a congressional committee that was holding hearings on the failure to research alternative medicine.

A general issue that emerges from the comparisons made here is the willingness to engage in prescriptive discourse--such as calls for policy reform--within the ethnographic text, as opposed to banishing such writing and action to a separate sphere of action as a citizen. Debates over the scope and meaning of intervention seem likely to characterize the second wave of ethnography in a way similar to debates over constructivism in the first wave. Whereas debates over constructivism often took the form of the value of realism versus relativism, debates over intervention seem to be developing on the parallel issues of the relative emphasis on a policy focus versus language-symbolism focus in styles of intervention, or the relative place of prescriptive discourse within versus outside the ethnographic text.

Conclusion

Whereas the first generation of STS ethnographies focused on opening the black box of the social content of science and technology, the second generation of the ethnography of science and technology has tended to open the brown, yellow, purple, red, pink, and other multicolored boxes of the culture and politics of science and technology. Just as feminism taught that the personal is the political, so this approach to STS teaches that the technical is the cultural and the political. To develop an analysis that is both culturally profound and politically relevant, one must have a point of comparison and some sense of an alternative, and perhaps no method is better suited to developing alternatives--or even to having the ability to perceive them in the first place--than is wide-ranging, multisited fieldwork. It is perhaps the sense of alternatives that underlies both the scope of ethnographic inquiry in the second generation (outside the laboratory or even the expert community of science and technology producers) and the concern with intervention. The alternative perspective might be found in the viewpoint of a Japanese physicist, a Mexican oncologist, a woman engineering student, or a religious, working-class amniocentesis patient. The power of an ethnography rooted in alternative perspectives is the ability to perceive science and technology differently, and consequently to imagine the design of new research programs, technologies, and policies.

Furthermore, the ability to articulate alternatives puts the ethnographer in a unique position of being able to become a voice of leadership in policy discussions of public interest. To restrict the ethnographer's voice to one of social scientific explanation or humanistic interpretation represents a failure of nerve when confronted with the prospect of intervention. Rather, ethnographers need to meet the opportunity and obligation to provide much-needed leadership as articulators of public interest, even as they face their own double-binds and senses of uncertainty. Such leadership is increasingly important in a world characterized by the globalization of capital and the privatization of public spheres.

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Footnotes

1 Prominent studies include Collins and Pinch (1982), Knorr-Cetina (1981), Latour and Woolgar (1986 [orig. 1979]), Lynch (1985), and Zenzen and Restivo (1982). Those studies and others are reviewed in Knorr-Cetina (1983, 1995) and listed in Lynch (1985: xiii-xiv); see Shapin (1995) and Hess (1997c) for points of entry into the SSK literature in general.

2 See Collins (1994a, 1994b) for a further discussion of his view of the stranger concept in the context of ethnography and social scientific research. The ethnomethodologist Lynch (1985: 2) also drew attention to the problem of achieving competence in the field science.

3 Examples of the empirical case studies in the technology vein are the volumes edited by Bijker, Hughes, and Pinch (1987) and Bijker and Law (1992). Two very different examples of continued fieldwork-based or observational research in the SSK tradition are Knorr-Cetina (1998) and Wynne (1996), which, like Traweek (1988) and the work of some of the American sociologists (e.g., Casper and Clarke, 1998; Fujimura, 1996; and Star, 1989, 1995; also Bowker and Star, 1999), are examples of projects that cross the two-generation heuristic. Likewise, see Kleinman (1998) for a laboratory study that includes an analysis of macrostructural issues.

4 See reviews by Downey and Dumit (1997); Franklin (1995); Franklin, Lury, and Stacey (1991); Hakken (1993); Harding (1998); Hess (1995, 1997b, 1997c); Traweek (1993); and Watson-Verran and Turnbull (1995). Examples of recent ethnographic projects (including some mixings of ethnography and history) that comprise this second network of researchers include Allen (1999); Blomberg (1997); Casper (1998); Clarke (1998); Davis-Floyd and Dumit (1998); De Laet (1998); Downey (1998); Dubinskas (1988); Dumit (1997, 2000); Eglash (1999a); Fischer (1999); Fortun (2001); Forsythe (2001); Franklin (1997); Franklin, Lury, and Stacey (1991: Part Three); Gamradt (1997); Gusterson (1996); Hakken and Andrews (1993); Haraway (1989, 1997); Heath (1997); Heath and Rabinow (1993); Helmreich (1998); Hess (1997a, 1999); Hogle (1999); Horn (1994); Koenig (1988); Layne (2001); Martin (1987; 1994); Morgan and Michaels (1999); Nader (1996); Nardi (1993); Nardi and Reilly (1996); Nyce and Bader (1993); Orr (1997); Perin (1998); Pfaffenberger (1992); Rabinow (1996); Rapp (1999b); Stone (1996); Suchman (2000a, 2000b); Taussig (in press); Timmermans (1999); Toumey (1994); Traweek (1988; 1992); and Zabusky (1994).

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