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HERE AND EVERYWHERE: Sociology of Scientific Knowledge

Steven Shapin
Department of Sociology and Science Studies Program, University of California, San Diego, La Jolla, California 92093-0102

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ABSTRACT

The sociology of scientific knowledge (SSK) is one of the profession's most marginal specialties, yet its objects of inquiry, its modes of inquiry, and certain of its findings have very substantial bearing upon the nature and scope of the sociological enterprise in general. While traditional sociology of knowledge asked how, and to what extent, "social factors" might influence the products of the mind, SSK sought to show that knowledge was constitutively social, and in so doing, it raised fundamental questions about taken-for-granted divisions between "social versus cognitive, or natural, factors." This piece traces the historical development of the sociology of scientific knowledge and its relations with sociology and cultural inquiry as a whole. It identifies dominant "localist" sensibilities in SSK and the consequent problem it now confronts of how scientific knowledge travels. Finally, it describes several strands of criticism of SSK that have emerged from among its own practitioners, noting the ways in which some criticisms can be seen as a revival of old aspirations toward privileged meta-languages.

There is no shortage of reviews and assessments of the sociology of scientific knowledge (SSK). Most have been written by critics or by participants meaning to put their special stamp on a contentious and splintered field.[1] I too am a participant: My views about what the field is, and ought to be, are strongly held; they have been canvassed elsewhere, and they will be unavoidably evident in this survey. Yet my purpose here is less to score points than to offer a critical survey

[1]Special note should be taken of Lynch (1993:Ch 2–4), which, while it argues a vigorous case for the virtues of ethnomethodology, is a detailed critical survey of recent trends in the social studies of science.

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of how SSK developed, and continues to develop, in relation to sociology, and to make the leading concerns of the field rather more comprehensible to sociologists in general than they have been. For this specialty, such a purpose is not banal, as neither the place of SSK in the sociological culture nor its implications for the future of sociology—especially social theory—have been adequately canvassed before.[2] The "here and everywhere" of my title refers at once to the problematic place of SSK within academic sociology and to a central problem it has generated and now confronts—how to interpret the relationship between the local settings in which scientific knowledge is produced and the unique efficiency with which such knowledge seems to travel.

Sociology of Scientific Knowledge and the Academic Culture

SSK must count as one of sociology's notable recent successes. Emerging not more than 25 years ago, in the 1970s and early 1980s it was an almost exclusively British practice (Collins 1983a:266–71). Now there are influential practitioners throughout North America, as well as in France, Germany, the Netherlands, Scandinavia, Israel, and Australia; and key Anglophone works have been translated into French, Italian, Japanese, Polish, Russian, and Spanish. Programs in "Science Studies," "Science and Technology Studies," or "Science, Technology and Society"—several elaborately funded by the US National Science Foundation—employing sociologists of scientific knowledge have sprung up at leading American universities; relevant professional societies flourish. Journals and academic publishers, once unaware of or uninterested in the field, now actively seek out contributions, creating a situation in which demand outstrips quality supply. Seminal monographs have been reprinted and advertised as "classics" (Bloor 1991, Collins 1992); anthologies, primers, synthetic surveys, and candidate textbooks have appeared and been superseded by new texts bidding to redefine a fast-changing field (Barnes 1972, 1985, Mulkay 1979, Barnes & Shapin 1979, Barnes & Edge 1982, Law & Lodge 1984, Yearley 1984, Woolgar 1988a, Cozzens & Gieryn 1990, Callon & Latour 1991, Jasanoff et al 1994).

Projects have been launched to intercalate the findings of SSK into programs of science communication and liberal education (Collins & Pinch 1993, Chambers & Turnbull 1989)[3] and into the analysis and formulation of science and

[2]As I shall note below, many sociologists of scientific knowledge were not professionally trained in sociology, and neither was I. (My training and much of my work belong more to history than to sociology.) Such amateurism often betrays itself in naiveté, far less often in insight into fundamentals. Not coming to sociology through a normal career-route, I find myself "unprofessionally" interested in what it *is* to have a sociological understanding of science.

[3]A measure of the provisional success of these suggestions is a recent series on scientific experiment, published in *The Economist* (Morton & Carr 1993), which draws heavily upon SSK research.

technology policy (Jasanoff 1990, 1992, Wynne 1992, Collins 1985, Fuller 1993, Cambrosio et al 1990, Travis & Collins 1991, Epstein 1993), while the potential of SSK (broadly construed) to recast the traditional categories of social and cultural theory as a whole has been asserted (Latour 1993, Law 1994).[4]

Most importantly, the general academic culture has shown great interest in what has been done in this field. Unlike many other sociological specialties, SSK has strongly engaged the attention of historians and philosophers (e.g. Shapin 1982, Shapin & Schaffer 1985, Rudwick 1985, Golinski 1990, Dear 1995, Fuller 1988, 1992, Rouse 1987, Toulmin 1990), and the boundary lines between what counts as historical or philosophical and what as sociological practice in the area have been blurred to the point of invisibility.[5] Meanwhile anthropologists, literary and feminist theoreticians, and a loosely defined but trendy "cultural studies" community have been attracted in significant numbers to the study of science largely through work in SSK. The social study of science is one of the modern academy's most unremittingly interdisciplinary projects.

Twenty-five years ago it was a truth almost universally acknowledged that there might be a legitimate sociological understanding of scientific error, of "the blind alleys entered by science," of the state of scientific institutionalization, and, perhaps, of the overall dynamics of scientific foci, but that there could be no such thing as a sociology of authentically scientific knowledge (Ben-David 1971:11–13). Now, while assent to the validity of SSK is scarcely universal, a number of central claims have quietly passed into common academic currency, and the recent paths of the history and philosophy of science, technology, and medicine have been fundamentally shaped by practitioners' appreciation of opportunities opened up or problems posed by SSK research.

At the same time, the field shows many signs of being in serious trouble: Some problems are of very long standing, while others must be seen as the bitter fruits of success itself. The very achievement of SSK in establishing the possibility, legitimacy, and interest of a thoroughly sociological (and social historical) understanding of scientific knowledge has attracted so great a range of scholars from other disciplines that neither the boundaries of the field nor

[4]Recent social theorists continue to comment centrally on modern science and technology while engaging only obliquely or not at all with the SSK literature (e.g. Bauman 1993:199–209, Giddens 1993:9–15, Bourdieu 1990, 1991).

[5]When the speciality was last reviewed in this journal, HM Collins (1983a:272) acutely noted that the relationship between SSK and relevant history was seamless and that a "proper description" of the field "would treat the history of science as integral." Yet, largely restricting his treatment to ethnographic studies of contemporary science, Collins did not there attempt to offer such a "proper" account. Two years earlier (Collins 1981a), he had voiced doubts that historical work was capable in principle of attaining the ethnographer's understanding of science, and four years later (1987) he proclaimed that historical studies represented some of the best SSK. Shifting judgments are possibly best read as reliable reflections of shifting realities.

its intellectual goals and foci are any longer at all clear. What appears to some practitioners as an admirable "diversity of voices" seems to others lamentable incoherence and lack of seriousness of purpose. The "social study of science," as opposed to SSK "proper," has developed into one of the modern academy's most centrifugal, most argumentative (at times uncivil), as well as most vital terrains (e.g. Pickering 1992). Just because what is at stake is nothing less than the proper interpretation of our culture's most highly valued form of knowledge—its truth—the struggle for interpretative rights has become fraught and bitter. Names are called and mud is slung. The weight of the world's injustices is dumped firmly on the shoulders of those maintaining "incorrect" methodological views. This is not a practice for the cardiovascularly challenged.

Fundamental issues of methodological propriety are fervently debated. Choices between Durkheimian objectivism and Weberian subjectivism, explanatory and interpretative goals, stress on structure and agency, micro and macro foci, theoretical and empirical methods—all are often fought out in relative disengagement from the career of parallel debates in mainstream sociology, with results ranging from rediscovered wheels to important respecifications of the terms of debate (Callon & Latour 1981, Collins 1981a, 1983b, Knorr-Cetina 1981a,b, Law 1974, 1984, Turner 1981). Metaphysical and ontological schemes are proffered, and it is asserted that sociology of science requires the adoption of the correct scheme, while skeptics wonder why interpretative projects should be supposed to require a metaphysics (Latour 1993, Shapin 1992:354–60). Leading sociologists of science discover that the practice has contained social-theoretical entities, such as "interests," and announce their gleeful despair that "definitive" descriptions or explanations of science can ever be attained, while other practitioners express bemusement that anyone could ever think to construct accounts free of theorizing or pretending to definitiveness (Woolgar 1981, Barnes 1981).

Relativism is attacked (far less often than it is actually commended) as an insidious threat to the fabric of social order, while advocates argue that methodological (not moral or ontological) relativism is simply necessary for the naturalistic interpretation of variation in belief.[6] Practitioners agonize over the proper posture of the analyst, as between disinterested and committed. The original claim that SSK was just the extension of science to the study of itself (Bloor [1976] 1991) has been countered by the increasingly insistent—though perhaps not yet dominant—voices of writers meaning to "expose" science (as

[6]For an analyst to say that the credibility of two different beliefs about the world should be interpreted using the same *methods* is, thus, not necessarily the same thing as saying that they are equally "true" or that the world(s) to which they refer is(are) multiple. Almost all SSK relativists set aside ontological questions and treat truth-judgments as topic rather than as resource. So far as morality is concerned, the dominant tendency here is not to celebrate moral anarchy but to interpret how locally varying moral standards acquire their obligatory character.

"hegemonic," as "masculinist," as "dehumanizing," as "mystifying") and by those who reckon that a proper task for scholars is to open up alternative visions of what science might be and how its social relations ought to be constituted (Martin 1993, Restivo 1989, Lynch & Fuhrman 1991, Scott et al 1990; cf Collins 1991, Lynch 1992b).

Quite recently, small numbers of eminent natural scientists have become aware of SSK, and, cavalierly neglecting crucial differences in tone and intent among practitioners, have sought to expose them all as motivated by hostility to science, purportedly animated by hidden political agendas (Gross & Levitt 1994, Wolpert 1992). Alleged crises in public confidence in, and support for, science have been traced—incredible as it may seem—to the sinister influence of SSK and fellow-traveling philosophy of science (e.g. Theocharis & Psimipoulis 1987). The political vulnerability of one of the few sociological specialties that, so to speak, "studies up," that aims to interpret a culture far more powerful and prestigious than itself, and that offers accounts at variance with that culture's official myths, is only now being made manifest. As the Chinese proverb has it, he who rides on the back of the tiger may wind up inside.

The number of sociologists working in the area continues to be very small. The rise of SSK to relative popularity coincided with the Thatcher government's systematic reduction in British university funding, from which several of the original homes of this sort of work suffered significantly, eroding or eliminating their ability to train the next generation. A surge of interest in this area among American institutions from the mid-1980s was also checked by recession and a consequent retrenchment in graduate student support and opportunity. Hard times discourage intellectual adventurousness, on the part of both students and recruitment committees. Time and improving economies may heal these wounds, but endemic structural difficulties beset SSK.

First, the sociological study of science makes demands upon initiates which all but a handful find difficult to fulfill. Despite the continuing scientistic bent of North American sociology, few students come equipped with relevant competences in the natural sciences. The genuine incapacity of many to get to grips with the scientific technicalities involved is added to the fear of others that such competences will be extremely hard to acquire. Despite much liberal educational rhetoric and distribution requirements, the gap between the "Two Cultures" described by CP Snow in 1959 has not noticeably been bridged. There is a widespread, and partly justified, sense that SSK is "hard," and students searching for a secure career-track are encouraged to look elsewhere.[7]

[7]Yet it has to be noted that the study of *any* culture possessing esoteric knowledge—e.g. that of machinists, soldiers, nurses, or Azande magicians—demands similar dedication and similar commitment to technical mastery. It is arguably not the "difficulty" of science but its "prestige" and "sacredness" that beget this heightened anxiety.

Nor does the poor availability of undergraduate courses in the area do much either to inform students about what the specialty is like or to give them even a smattering of familiarity to set against structurally induced "technophobia." Moreover, scientistic North American sociological traditions and, to a lesser extent, traditions in Britain and Europe continue actively to disseminate a picture of scientific "method" and scientific knowledge radically at variance with those offered by SSK. There is an argument that the last great bastion of faith in simplistic images of science and its "method" is not to be found in the natural but in the social sciences.

Students thus trained often find the body of recent SSK—when they encounter it—not just unfamiliar but shocking. Few sociology texts prepare them for the claim that scientific truth is amenable to a thoroughgoing sociological scrutiny, while some of the most senior and eminent authorities remain among the unconvinced or unapproving. Joseph Ben-David (1981:41–47, 54–55), judging work in the area to be largely "programmatic," pronounced SSK to be "sociologically irrelevant" and a "failure." Stephen Cole (1992: 81), while making irenic gestures toward SSK, nevertheless gave his opinion that it had "failed to generate a single example or case study" that shows that social processes "actually influence the specific cognitive content of science." And TS Kuhn (1992:8–9), dissociating himself from sociological appropriation of his work, has recently proclaimed that SSK, or, more ambiguously, what "has been widely understood" as its claims, is "an example of deconstruction gone mad."[8] Compared to other specialties, SSK has few senior advocates or practitioners in the sociological profession, nor, despite its persistent characterization as "fashionable," is association with SSK evidently a sound strategy of career advancement.

SSK and Sociology

The founding father of the sociology of science, Robert K Merton, worked from the late 1930s through the 1960s to constitute the study of science as a legitimate branch of structural-functionalist sociology, while at the same time he attempted to constitute sociology as "scientific." What counted as "being

[8]Harriet Zuckerman's recent full-scale survey (1988) of the sociology of science is, by comparison, notably informed about and courteous toward strands of SSK, while she labors to assimilate this work to the structural-functional tradition with which it is often seen to be in conflict. I should add that, as a participant, I am, of course, wholly satisfied that a sociology of scientific knowledge is both possible and necessary, and that it has accumulated a large body of outstanding empirical work. Likewise, I am satisfied that much—not all—criticism of SSK continues to proceed from an obtuse—and possibly willful—misrepresentation of its central methods and claims (cf Barnes 1994:22–25, Bloor 1991:163–85). Yet my purpose here is not merely to reiterate old arguments in defense of SSK but to try to note some features of the cultural framework in which that misrepresentation is so deeply entrenched.

scientific" was overwhelmingly taken from formal and informal philosophical models of the natural scientific "method" (e.g. Parsons 1949:Ch 1). The same sensibility that persuaded Merton and his associates that sociological accounting had to stop at the door of scientific method and scientific knowledge (e.g. Merton 1970:xviii–xix, 75, 199–200) also supported the claims of sociology to be a genuine science. Accordingly, the very idea of a sociology of scientific knowledge butted against the self-understanding and legitimation of dominant strands of sociology. It is this circumstance, more than others, that makes the place of SSK so problematic within the overall sociological culture, especially in its North American form.

Therefore, Peter Winch's (1958) critique of enterprises that tried to base an understanding of social action on the methods of natural science was decisive for several practitioners of SSK (Collins 1975:216, 1981a:373, Knorr-Cetina 1981a:148–49, Lynch 1993:40–41, 163, 183, 228). If social science could be construed as fundamentally different from natural science—in its objects and in its appropriate methods—then it followed that the opening up of the natural sciences to sociological understanding need not be seen as a threat to sociology. The pertinence of Winch's views indicates the importance to SSK of intellectual resources coming from the margins of the American sociological profession, and, indeed, from outside of sociology proper.[9] Winch's book significantly stimulated curiosity about the later philosophy of Wittgenstein, especially its analysis of the indeterminacy of "rules," while other British practitioners disputed Winch's distinction between sociology and natural science (Bloor 1983). The intellectual mix that in the 1970s inspired the early sociological studies of such British writers as Barry Barnes, David Bloor, HM Collins, Donald MacKenzie, Michael Mulkay, Richard Whitley, and Steve Woolgar included, to be sure, elements of the classic sociological theory of Durkheim and, more diffusely, of Marx, but also the historiography of TS Kuhn, the comparative cultural anthropology of EE Evans-Pritchard, Mary Douglas, and Robin Horton, philosophical work on the categories of sociological explanation by Alisdair MacIntyre, Basil Bernstein's revisionist sociology of language and education, the relativist philosophy of Nelson Goodman, Mary Hesse's neo-Bayesian philosophy of science, and, especially, a vast body of detailed historical work on the natural sciences in their social and cultural contexts.

In the early and mid 1980s, SSK received an infusion from practitioners trained in, or attracted by, phenomenological and ethnomethodological traditions. Studies by Michael Lynch, Steve Woolgar, Steve Yearley, and Eric Livingston drew significantly on work by Alfred Schutz and Harold Garfinkel

[9]Winch's work, while influential in British sociological theorizing, is referred to little or not at all in standard American surveys.

(Lynch 1988, 1993:Ch 4). [It was predominantly these writers who imported the tag "social construction" into SSK, most immediately by way of Berger & Luckmann (1966), though others not primarily indebted to phenomenology soon elaborated a modified conception of "social constructionism," different from both its theoretical begetters and from sociological "labeling theory."][10] More recently, such sociologists as Susan Leigh Star, Adele Clarke, Joan Fujimura, and Chandra Mukerji have effected links between SSK and "Chicago School" interactionist sociology of work, occupations, and culture (e.g. Clarke 1990, Clarke & Montini 1993, Star 1989, Star & Griesemer 1989, Mukerji 1989, Fujimura 1987, 1988). And all through the 1980s, social studies of science have been increasingly preoccupied by challenges to several central descriptive and explanatory categories emerging from a Parisian circle centered on Bruno Latour, whose work was itself fundamentally shaped by Nietzschean and Heideggerian philosophical traditions as well as by the techniques of semiotics and anthropological ethnography (Latour 1987, 1988a, 1993).

Only in one respect is SSK typical of the sociological profession: Its practitioners disagree about the very identity of sociology, and, therefore, about the identity of a legitimate sociological framework for the study of their objects. In the late 1970s and early 1980s, Ben-David, meaning to be rude about SSK, observed (1978:203–08, 1981:43–47; cf Zuckerman 1988:513) that few of its leading practitioners were properly trained as sociologists, that they meddled with epistemological concerns best left to philosophers, and that, owing to their amateurism, they were unfamiliar with the history of disaster that was said to be the career of systematic sociological attempts to account for scientific knowledge. Ben-David's description was, to be sure, correct on several points. Few of the founding figures *were* professionally trained as sociologists (Collins 1983a:267–68). On the other hand, a number had natural science backgrounds that discouraged them from confusing the reality of scientific knowledge-making with textbook idealizations. The field was also particularly receptive to the sociological exploitation of historical and philosophical frameworks developed by such writers as Michael Polanyi (1958) and Thomas Kuhn (1962), who did have extensive natural scientific experience.

Moreover, as Ben-David rightly noted, the leading concerns of "British" SSK *were* philosophical and, in particular, epistemological. If scientific judgment and the growth of knowledge could be adequately accounted for by impersonal canons of evidence, logic, rationality, and, especially, of "the

[10]By the early to mid 1970s, phenomenologically inclined sociologists were widely appropriating the tag, and it remains especially fashionable in work on sexuality, deviance, and crime. So far as I can discover, the first uses of the term in titles of studies concerned with science appear in 1976 and 1977; evidently the term reached its height of SSK popularity in the late 1970s and early 1980s (e.g. Latour & Woolgar 1979, MacKenzie 1981a, and Collins & Pinch 1982).

scientific method," then, indeed, neither a sociological nor an historically contextual account was appropriate for the interpretation or explanation of scientific knowledge. The "Great Tradition" of Vienna Circle logical empiricism was concerned with providing not a naturalistic account of scientific change and judgment but (as Rudolf Carnap and Hans Reichenbach said) its "rational reconstruction." Yet other philosophers wrote as if "method-stories" were historically adequate, and still others continued to conceive of sociological considerations as potential "pollutants" of authentic science, to be guarded against or relegated to the contingent domain of "contexts of discovery."

Accordingly, early SSK took it as a primary task to create a legitimate space for sociology where none had previously been permitted, in the interpretation or explanation of scientific knowledge. In just that sense, SSK set out to construct an "anti-epistemology," to break down the legitimacy of the distinction between "contexts of discovery and justification," and to develop an anti-individualistic and anti-empiricist framework for the sociology of knowledge in which "social factors" counted not as contaminants but as constitutive of the very idea of scientific knowledge (e.g. Bloor 1975, Law 1975; cf Fuchs 1992:Ch 2). SSK developed in opposition to philosophical rationalism, foundationalism, essentialism, and, to a lesser extent, realism. The resources of sociology (and contextual history) were, it was said, necessary to understand what it was for scientists to behave "logically" or "rationally," how it was that scientists came to recognize something as a "fact," or as "evidence" for or against some theory, how, indeed, the very idea of scientific knowledge was constituted, given the diversity of the practices claiming to speak for nature (Bloor 1984a,b, Collins 1981b). The current philosophical tag corresponding to SSK is "social epistemology" (Fuller 1988, 1992).

Analytic philosophers of science have not much appreciated, nor in many cases comprehended, the gesture—a "social epistemology" seemed to some a contradiction in terms—and the career of SSK continues to be marked by trench warfare between its practitioners and the dominant tendencies in the philosophy of science (e.g. Brown 1984, 1989, Bloor 1991:163–85). For these reasons, SSK developed partly through efforts to exploit some traditional and nontraditional sociological resources to show—both theoretically and empirically—how a sociology of scientific knowledge was possible, and not as a professional extension of mainstream disciplinary practices into this terrain. On the whole, mainstream sociological practitioners did not want sociology to go in such directions or did not believe that it could be so extended. The over-publicized "warfare" between SSK and the "Mertonians" was, in fact, but a brief early episode in the career of the field and was mainly concerned with such questions of possibility (Collins 1983a:266, 271).

SSK practitioners soon found it more satisfying to do the sociology of scientific knowledge than to argue whether it was possible, and by the early

1980s, they were content to point to a body of detailed empirical studies as strong evidence of that possibility (Shapin 1982). Indeed, early practitioners systematically argued that scientific knowledge could be understood in just the same way as one would go about interpreting any other area of culture—there were no special resources or methods required to account for science (Barnes 1974). So a number of important writers, having established that point of possibility to their satisfaction, saw no special reason to persist with the particular study of the natural sciences and moved on (in whole or in part) to applying the methods and resources of SSK to other areas of culture (notably technology and economics), to debates in the philosophy of knowledge and theories of representation, to social theory, and, notably, to participation *within* such scientific practices as artificial intelligence (e.g. Barnes 1988, Collins 1990, Ashmore et al 1989).

SSK and the Forms of Cultural Inquiry

If intellectual influences on SSK and its diverse disciplinary affiliations make the field marginal to the profession of sociology, its preoccupations, circumstances, and several of its findings ought to make it central to the sociological enterprise, and, indeed, to cultural inquiry as a whole. On the one hand, SSK, like any descriptive or explanatory practice, inevitably deploys our current stock of knowledge about what the world, natural and social, is like. However much practitioners in this area may mean to show that such items as "neutrinos," "neurofibrillary tangles," or "social class" are theorized and socially constructed, the realist mode of speech is ineliminable in practice, and the "phenomenological bracketing" that allows analysts to be curious about how such items are constructed is dependent upon a robust realist idiom in speaking about other items. Skepticism, as Wittgenstein said, takes place on the margins of trusting systems, and radical skepticism is radically disruptive of communicative order (Douglas 1986, Shapin 1994:Ch 1). This is no more than to say that sociologists of scientific knowledge "know" the world that science has depicted as securely as any other competent members of the culture, and that they use this knowledge in producing their accounts.

On the other hand, the practice that seeks to understand science as an historical and social enterprise also demands that analysts be curious about its findings, including the findings about the natural and social worlds that have to be used to implement that curiosity. The realist mode of speech itself becomes an object of curiosity. In this sense, SSK is prone to tension between how it speaks and what it says, and its practice is irremediably embedded in the objects of its inquiry (Barnes 1981:484, 493). While many philosophical and everyday forms of inquiry seek to justify our intuitions about science—its correspondence, its objectivity, its efficacy, and its progressiveness—SSK takes those intuitions as matters to be interpreted and explained (Collins

1981b). That makes SSK at times uncomfortable—to both practitioners and readers of their work—but also fundamental to our culture's self-understanding. Uneasiness in inquiry is often—not invariably—a sign that the inquiry is nearing the heart of the matter, and the claimed hyper-awareness of "post-modernity" is played out in SSK in one of its most acute forms.

The second reason SSK may arguably be central to the sociological enterprise and to cultural inquiry as a whole flows from the categories that traditionally comprised the sociology of knowledge and the changes wrought on these categories by work over the past quarter-century. As Merton surveyed the field in 1945, the sociology of knowledge was the practice that sought to show the influence of social (or "existential") factors upon "mental productions" (Merton 1973; cf Parsons 1949:14). How did social factors condition the form, content, and dynamics of cognitive products? There was social stuff and there was intellectual stuff, and there were (varying) narratives concerned to bridge the Cartesian gulf. That dualism, and that resulting problematic, were accepted by all theorists, no matter what scheme they proposed for doing the connecting (causal, functional, or symbolic), and no matter what exemptions (typically the mental productions of logic, mathematics, and the natural sciences) they stipulated.[11]

The dualism that provided traditional sociology of knowledge with its frame of reference was inherited from ancient lay and philosophical discourse. From the Greek philosophical tradition to early Christianity and on into the culture of seventeenth-century English empiricism and nineteenth-century high romanticism, knowledge was considered to be properly philosophical, sacred, or genuine insofar as the circumstances of its attainment were removed from the domains of the practical and the political (Shapin 1991a,b). Disengagement and disembodiment were ancient tropes of value: Removing knowledge-making from the polis was seen as a technique of transcendence. Accordingly, to say that knowledge was produced in and through mundane interactions between people, as well as between people and reality, was taken just to say that its truth, objectivity, universality, and power were compromised. So far as genuine philosophical knowledge was concerned, the polity was a pollutant. In this way, interpretative and explanatory tasks were embedded—largely unwittingly—in traditional tropes of evaluation. Bacon's idols of the theatre and the marketplace marked the social contamination of knowledge no less

[11]It has often been insisted that Merton himself (1973) was the father of that "Copernican revolution" in the sociology of science which took true as well as false belief for its legitimate subject, from which it follows, in Bourdieu's opinion (1990:297–98), that writers like Barnes and Bloor were merely "crashing through an open door." It is, for all that, remarkable that Merton never purported to produce a sociological account of what has been called "the technical content" of scientific knowledge, while some of his followers continue to insist very vigorously on the impossibility of any such account.

than the presentation of Greek and early Christian thinkers as withdrawn and disengaged. From the late 1930s through the 1960s and beyond, the discourse of "internalism" and "externalism" that so fundamentally structured the practice of history and sociology of science took the dualistic juxtaposition of "social" versus "rational," "intellectual," and "evidential" for granted. "The social" was taken as that which was "external" to science, and it was persistently debated by what means authentic science kept "the social" at bay, how and to what extent "social influences" infiltrated science without deleterious effects, or how what seemed to be properly scientific knowledge was "in fact" socially marked ideology (Shapin 1992).

Some strands of early SSK and related social historical work did indeed deploy the same society-mind vocabulary as traditional sociology of knowledge. Here the task was taken to be the showing of "social influences" on properly scientific knowledge where such "influences" had previously been reckoned not to act. The taken-for-granted equation between the social autonomy and the truth of knowledge was challenged, and a series of empirical studies sought to establish—without a tone of exposé—that even the "hard cases" of claims within the physical and mathematical sciences, taxonomic sciences, and observation-reports were so "influenced": Society, and its concerns, nevertheless "got in" (Shapin 1979, MacKenzie 1978, 1981a). To a number of critics, that sums up the case that SSK argued: Its bearing upon the truth and objectivity of science was taken over from traditional schemes that conceived the social as a "contaminant" (Brown 1989). Where there was "social influence," there the roles of natural reality and rationality were regarded as compromised.

However, this sensibility in fact grossly misrepresents SSK's case for "the social." Rather, the claim was that "the social dimension" of knowledge needed to be attended to in order to understand what counts as a fact or a discovery, what inferences are made from facts, what is regarded as rational or proper conduct, how objectivity is recognized, and how the credibility of claims is assessed. The target here was not at all the legitimacy of scientific knowledge but the legitimacy of individualist frameworks for interpreting scientific knowledge. Attention was drawn to "the social dimension," accordingly, not as a pollutant but as a necessary condition for making, holding, extending, and changing knowledge. In just that sense, the language of "the social" as a "dimension," an "influence," or a "factor" to be juxtaposed with the "factors" of evidence and rationality was rendered problematic (Lynch 1991b). And here, arguably, SSK was the primary field in which that challenge to the traditional dualism was laid down.

The challenge was expressed in varying idioms. From 1979 Bruno Latour repeatedly pointed out that there was undeniably as much (and arguably more) "politics" within the walls of scientific workplaces as there was outside, and that the securing of credibility for scientific claims was a thoroughly social and political process. Thus he highlighted as a potential topic of inquiry the cultural scheme that simply assumed otherwise (Latour 1987: 30, 62). At the same time, the performance of modern political action fundamentally implicated scientific knowledge of what sorts of things existed in the world and how these things acted upon humans. The "missing masses" in existing social and political theory were the "nonhumans" predicated by science and technology. A defensible sociology of science and technology, therefore, had the potential to recast the terms of social theory generally. Signaling the sensibility that sought to remove "the social" from its status as "factor," the second (1986) edition of Latour & Woolgar's *Laboratory Life* deleted the word "social" from its original (1979) subtitle: *The Social Construction of Scientific Facts.* To remove "the social" from the idea of scientific knowledge was said to remove its status as knowledge.

In a more familiar (anti-)epistemological idiom, Mulkay, Barnes, and Bloor sought, from the early 1970s, to establish the inadequacies of individualism for interpreting scientific, or any other form of, knowledge. Here the Kuhnian framework assumes central significance, not least for appreciating the place of SSK vis-à-vis existing sociological traditions. If, for Merton, the answer to the Hobbes/Parsons social-order problem was supplied, in the case of science, by a set of allegedly unique social norms making up the "ethos of science," for Kuhn-inspired SSK, the regulative principles of social order in science were furnished *by scientific knowledge itself.* Within traditions of "normal science," authoritative socializing institutions schooled practitioners in exemplars ("paradigms") of what it was to do good science in particular domains. For early modern chemists, Robert Boyle's J-tube experiment defined a model problem and its model solution, including the embodied representation of what it meant for evidence to confirm or disconfirm a theoretical hypothesis; for late twentieth-century molecular biologists, the "central dogma" (by which DNA produces RNA produces protein) similarly structures practitioners' sensibilities about relevant domains of inquiry, about the directionality of molecular cause, and about the locus of biological meaning.

From a sociological point of view, Kuhnian SSK is at once conservative and radical. On the one hand, it seeks inter alia to answer traditional questions about the grounds of a communal order, and it does so by pointing to the regulative role of norms. While the regulatory relevance of social maxims ("Be skeptical," "Be disinterested") is doubted, the significance of norms for ensuring order and for marking the boundaries of communities is vigorously respecified and reaffirmed in a new idiom. The solidarity of specialist communities—or such solidarity as is found to exist—is coordinated through their specialist knowledge. Good and bad, proper and improper, interesting and banal scientific behavior is recognized and sanctioned by members' knowledge of the natural world. On the other hand, by arguing that the relevant norms are made of the same stuff as the community's technical knowledge, the Kuhnian move overturns the existing sociology-of-knowledge scheme that asks how "society might influence knowledge."

Just because the sway of an evaluative individualism in interpreting our society's most esteemed knowledge has been so strong, SSK's insistence upon a quite elementary feature of the sociological sensibility has seemed to acquire a shockingly radical, even subversive, character. If sociology is the study of the collective aspects of human conduct, then a basic role for the sociological study of scientific knowledge is showing in what ways that knowledge has to be understood as a collective good and its application as a collective process. If there is a fundamental and irreducibly sociological point to be made about scientific knowledge, it is this one. Society—including the specialist societies of scientists—might properly be regarded as a distribution of knowledge, just as the very idea of knowledge depended upon the social relations of knowers (Barnes 1988, Shapin & Schaffer 1985).

Following such writers as Simmel (1950: 313) and Polanyi (1958), it has been noted that modern systems of scientific and technical knowledge are highly differentiated and distributed: No one individual keeps the whole of a discipline's knowledge in his or her head, and even the technical knowledge involved in the conduct of a single experiment in modern physics or biology is typically distributed across a range of specialist actors. In a symbolic interactionist idiom, actors in different "social worlds" are invariably involved in the making of scientific goods (Star & Griesemer 1989). And, while this distributed character is very evident in modern scientific practice, in principle it is arguably just as pertinent as a description of the "simpler" scientific cultures of past centuries. The director of a large-scale experiment in high-energy physics does not have direct knowledge of every aspect of that experiment, just as an individual seventeenth-century English natural philosopher would typically not have direct evidential warrant for his knowledge of icebergs, comets, or the flora of the Americas. As a general matter, practitioners rely massively upon others for their knowledge. For there to be solutions to the problem of knowledge there have to be practical solutions to problems of trust, authority, and moral order (Barnes 1985:49–58, 82–83). Individualist philosophies of knowledge at least since Locke have persistently argued that knowledge is genuine and secure when its warrants are direct, experiential, and individual (Shapin 1994:Ch 5). If that is the case, then the sociological sensibility would suggest that there is perishingly little genuine and secure scientific knowledge in the world. Yet that is *not* what sociologists of scientific knowledge have argued: Scientific knowledge is as secure as it is taken to be, and it is held massively on trust. The recognition of trustworthy persons is a necessary component in building and maintaining systems of knowledge, while the bases of that trustworthiness are historically and contextually variable. This core sociological insight into the collective nature of knowledge has enormous potential to generate detailed comparative studies of the moral economies of science, but, perhaps owing to the largely philosophical concerns of many sociologists of scientific knowledge, the point has as yet been made, for the most part, at a programmatic level.[12]

A fundamental sociological collectivism applies not just to describing the conditions in which it can rightly be said that individuals have knowledge but also to the means by which knowledge is acquired, applied, and changed. Scientists' knowledge of specialist domains of the natural world, like that of children, is for the most part initially acquired via trusted sources. The proper applications of terms like "chicken," "dog," "electron," and "ideal gas" are not logically fixed; rather, how such terms are used, whether by scientists or laity, is adapted to a range of contingent circumstances, including the weight of custom and convention and the purposes people may have in representing the world. This is the sense in which it is said, following Durkheim and Mauss, that the classification of things reproduces that of people (Bloor 1982). When people confront the experience of their senses, they do so within an already existing structure of knowledge given them by their community and within a structure of purposes sustained by their community. Nor, when new experience is confronted, is it logically determined how such experience is to be sorted out with respect to existing schemes: whether it is to be counted as evidence confirming or disconfirming some theory, whether it is to be bracketed, subjected to taboo, or filed away, to be dealt with another time. It is people's goal-orientation—the pragmatic structure of the community to which they belong—that judges among possible courses of action. Much of the theoretical devel- opment of SSK through the 1970s and early 1980s concentrated upon elaborating a fully general sociological framework for interpreting knowledge-acquisition and concept-application (Barnes 1982a,b,c, 1983). And, despite the fact that this work developed without evident specifiable intellectual "influence" from American pragmatist philosophy, it is wholly compatible with pragmatism, and, by extension, with strands of academic sociology—those of Mead, Blumer, and their progeny—that drew inspiration from James and Dewey.

In this way, SSK opposed philosophical rationalism—the view that scientific judgment is sufficiently determined by unambiguous criteria of method—by asserting the contingency and the locality of judgment. Rules did not suffi-

[12]An insistence that SSK should be concretely operationalized in such ways has informed some criticism that it "has not developed a fully-fledged sociological account of science" (Fuchs 1992:Ch 2, Hagendijk 1994:135). Once more, an accusing finger is pointed at excessively philosophical concerns.

ciently explain scientific judgment; the way in which rules were identified and used was itself a topic for contextual inquiry. Why is it that, since one can "rationally" continue the series 2, 4, 6, 8 ... in any number of ways, the "right way" of going on in an arithmetic class is "10" whereas at an American sporting event it is more likely to be "who do we appreciate?" (Collins 1992:12–16).[13] Right conduct is tied to place and purpose. The in-principle "interpretative flexibility" of rules is securely settled in practice by local notions of decorum.

By contrast with rationalism, such SSK writers as Barnes and Bloor explicitly endorse a robust realism and, indeed, have noted that the idiom of sociological realism presupposes a corresponding natural realism: "No consistent sociology could ever present knowledge as a fantasy unconnected with our experience of the material world around us" (Bloor 1991:33) or "[T]here is indeed one world, one reality, 'out there,' the source of all our perceptions ... " (Barnes 1977:25–26, cf 1992, Barnes & Bloor 1982).[14] What one cannot do, if one proposes disinterestedly to interpret varying beliefs about nature, is to use one particular account—usually that of modern science—to gauge the validity of others. That would be to include the answer in the premises (Barnes 1992). All institutionalized beliefs about nature are causally connected to reality, and all are on a par with respect to the manner in which their credibility is to be interpreted. Judgments of what is the case, like judgments of what is rational, are locally accomplished.

Situated Knowledge and Its Travels

Indeed, the best way of summing up the thrust of a great deal of work in SSK, and in related history and philosophy, produced from the mid-1970s to the present, is to see it as concerned to show in concrete detail the ways in which the making, maintaining, and modification of scientific knowledge is a local and a mundane affair. Here the case-study method—occasionally belittled as piling on more "proof" of "the same sociological theory"—is beautifully suited to the business at hand, since its "theory" of science is more "shown" than "said, and since its practitioners are rightly skeptical of narratives that purport to distill the "essence" of practices as varied as those that are, and have been,

[13]The sociological *locus classicus* for treatment of Wittgenstein on rule-following is Winch (1958), and in SSK, Collins (1992:Ch 1), Bloor (1983, 1992), Lynch (1992a, 1993:Ch 5).

[14]The puzzle of why, despite these insistences, critics of SSK make it out as a recommendation of "social variables" *versus* the "data from the empirical world" (e.g. Cole 1992:2, 12, 229) can best be resolved by noting the hold of *individualistic empiricism* that makes such dualistic language seem natural. Even Collins's famous dictum (1981c:54, cf Collins & Cox 1977:373, Collins 1981b:216, 1992:16, 174) that "the natural world in no way constrains what is believed to be" is repeatedly specified not as an epistemological or ontological judgment but as a "methodological prescription"—how analysts should proceed if they are genuinely curious about the bases of varying beliefs.

called "scientific."[15] Quite unlike past traditions in the sociology of science, SSK case studies are typically tightly focused upon specific passages of scientific practice. Their detailed ethnographic or historical character is geared to breaking down the "enchantment" produced by distance (Collins 1992:144–45)—and hence the appeal of idealized "method-stories"—and to displaying the contingency, informality, and situatedness of scientific knowledge-making.

These "localist" arguments have proceeded along a number of lines. First, science-making is identified as a mundane matter. Exploiting work by such writers as TS Kuhn (1970), Peter McHugh (1970), Jeff Coulter (1975), Harvey Sacks (1984), and Melvin Pollner (1987), much empirical and theoretical research has been devoted to showing that the making of scientific knowledge can be sufficiently accounted for by ordinary human cognitive capacities and ordinary forms of social interaction (Barnes 1976, Feyerabend 1978, Lynch 1985, Collins 1992, Shapin 1994). Once the grand narratives of unique scientific "norms" and unique scientific "method" lost their compulsion, curiosity was unleashed about how scientists used "secular" ways of thinking and acting to build up their exceptionally authoritative systems of knowledge (Barnes 1974, Lynch 1985, Latour 1987, 1988a, Latour & Woolgar 1986, Turner 1989). Almost needless to say, mundane means can produce widely differing products—just as stone, mortar, and rules of thumb can produce results as varying as a worker's cottage and Durham Cathedral—and saying that science ought to be understood as a typical form of culture is, of course, not the same thing as saying that it is no different from other forms of culture. Arguably, sociologists and historians are only now in a fit position naturalistically to address relevant questions about the character and bases of cultural difference.

Second, since it is argued that no scientific claim "shines with its own light"—carries its credibility with it—sociologists and historians have become intensely interested in the specific processes of argumentation and political action whereby claims come to be accepted as true or rejected as false. The gap between individual experience and public knowledge must always be filled by persuasion, and the resources available to make claims persuasive can include any tools the local culture makes available and is responsive to. The "rhetorical turn" in SSK has now yielded a large body of empirical work on the techniques of scientific exposition—the textual and informal means by which scientists labor to persuade others, to extend experience from private to public domains, to assure others of their disinterestedness, to assert the significance of their claims, to argue that their body of knowledge is indeed "scientific" (Woolgar 1976, 1989, Yearley 1981, Gilbert & Mulkay 1984, Shapin

[15]The link between the case-study method in science studies and the attempted revival of the casuistical tradition in ethics is worth pursuing. Both instantiate doubt about the regulatory role of abstract theories (see e.g. Jonsen & Toulmin 1988, Bauman 1993).

1984a, Pinch 1985, Latour 1987, Bazerman 1988, Myers 1990, Dear 1991, Gieryn 1992).

Third, stress has been put upon the embodied character of scientific knowledge. It is noted that scientific competences are not effectively transferred from one individual to another, and from one place to another, solely by recipes, algorithms, or formal rules of proceeding. Much empirical work has addressed the embodied nature of scientific know-how and the embodied vectors by which it travels, whether that embodiment is reposed in skilled people, in scientific instruments, or in the transactions between people and knowledge-making devices. Collins's now-classic study (Collins & Harrison 1975) of the transfer of laser-building skills as embodied tacit knowledge built upon an appreciation of science as craftwork, and that work has in turn been extended by ethnomethodological and symbolic interactionist studies of modern biology (Lynch 1985, Jordan & Lynch 1992, Clarke & Fujimura 1992, Cambrosio & Keating 1988) and mathematics (Livingston 1986), and by historical work on physics (Shapin & Schaffer 1985:Ch 6, Morus 1988, Schaffer 1989, 1992a,b, Warwick 1992–1993), astronomy (Schaffer 1988, Van Helden 1994), chemistry (Roberts 1991, Golinski 1994), genetics (Kohler 1994), and medicine (Lawrence 1985).

Finally, empirical and theoretical work has addressed the physical situatedness of scientific knowledge-making (Ophir & Shapin 1991). The grand narrative of inherent scientific universality deflected attention away from place: Situatedness was the mark of lower cultural forms, and science, as Durkheim announced (1972:88), was "independent of any local context." Again, structures of evaluation weighed against localist perspectives on science. Yet, from the point of view of naturalistic inquiry, science is undeniably made in specific sites, and it discernibly carries the marks of those sites of production, whether sites be conceived as the personal cognitive space of creativity, the relatively private space of the research laboratory, the physical constraints posed by natural or built geography for conditions of visibility and access, the local social spaces of municipality, region, or nation, or the "topical contextures" of practice, equipment, and phenomenal fields (Lynch 1991a, Gooding 1985, Shapin 1988). Here SSK has not merely attempted a resuscitation of interest in the "contexts of discovery" abandoned by philosophers, it has also opened up new curiosity about structures of "justification" and the translation of knowledge from place to place.

It is impossible to treat localist sentiments in the study of science without engaging with the contribution of feminist writers, and it is equally impossible briefly to summarize one of the modern academy's most heterogeneous and politically charged genres. (Feminist views of science, and their vexed relations with SSK and social theory, merit systematic survey on their own by someone competent in this contested domain.) One strand of feminist writing on science—that which views the whole of post-seventeenth-century science as "essentially masculinist"—is not, indeed, compatible with post-Kuhnian sociological localism: Grand narratives about what science "essentially is" or about its "essential preoccupations" were just what the contextual and naturalistic turns were meant to reject. To say that science, across a broad sweep of history and cultures, was "essentially" informed by gender preoccupations, or, with the "standpoint" theorists, that women-as-victims are "epistemologically privileged," represents much the same kind of sensibility as those that announced that science was "essentially" about class relations, or about the abstraction from common sense, or that a class of free-floating intellectuals existed and enjoyed epistemological privilege. Yet other versions of feminist science studies are perhaps best seen as tributaries of SSK and related streams feeding the river of embodied localism. In criticizing individualist, rationalist, and disembodied views of science, such feminists as Dorothy Smith and Lorraine Code urge perspectives similar to those of phenomenologically informed SSK, while Donna Haraway's flamboyant antimodernism tackles the great Enlightenment dualisms—nature/culture, human/nonhuman, etc—in order to display their historical specificity and thereby to reject them. Such feminist work often has its own intellectual and frankly political agenda, but it is, nevertheless, intelligible to see it as proceeding from sensibilities similar to SSK localism. It is another idiom for identifying and interpreting "situated knowledges" (Haraway 1991: Ch 9).[16]

The localist thrust of recent SSK has generated one of the central problems for future work. If, as empirical research securely establishes, science is a local product, how does it travel with what seems to be unique efficiency? One appeal of the modernist grand narratives of reason, reality, and method was the table-thumping response they offered to questions about the travel of science. If, however, universality can no longer be accepted as an assumption flowing from the very nature of the knowledge or the "method" for making it, then what are the mundane means that so powerfully effect the circulation of science? And is that travel, indeed, to be treated as real, or is what circulates yet another illusory grand narrative?

In this connection, SSK has thrown up one particularly well-developed framework for engaging with the problem of travel. Bruno Latour and his associates have offered what is best taken as a descriptive vocabulary for construing scientific success and power (Callon et al 1986, Latour 1987, 1988a). "Technoscientific" knowledge—both propositional claims and the

[16]An entry to the contest between "standpoint," "empiricist," and "postmodern" feminist writing on science can be secured via Bordo (1987), Harding (1986), Code (1991), Haraway (1991), Keller (1983, 1986, 1988), Longino (1990), Merchant (1980), Noble (1992), Richards & Schuster (1989), Schiebinger (1989), and Smith (1990).

knowledge embodied in technology—are held stable and treated as true, insofar as they are constituted as obligatory passage points for many actors' work. Think, for example, of the physical knowledge embodied in a thermometer. To contest that knowledge would be to fight on many fronts against many institutionalized activities that depend upon treating the thermometer as a "black box." Intercalating science or technology into larger and larger networks of action is what makes them durable. When all the elements in a network act together to protect an item of knowledge, then that knowledge is strong and we come to call it scientific. The central modern scientific phenomenon to which attention is directed is thus metrology—the development of standards and their circulation around the world (Schaffer 1992b, O'Connell 1993, Barry 1993, Shapin & Schaffer 1985:Ch 6). The suggestion is that the wide distribution of scientific knowledge flows from the success of certain cultures in creating and spreading standardized contexts for making and applying that knowledge. Phrased in this way, Latour is offering a new, but sociologically recognizable, vocabulary for describing institutionalization.[17]

The resources available to effect this intercalation include a range of discursive and technical means. Artfully deployed rhetorical maneuvers delete the grammatical modalities that qualify claims: The move from "Bloggs says," to "It is the case," to the submergence of a claim in taken-for-granted background assumptions in yet another claim is a way of describing the ascent to truth. Scientific rhetoric induces readers to go in only one direction, that pointed out by the author. Theatres of persuasion can be mounted: The dramatic staging of such field trials as those laid on by Louis Pasteur at Pouilly-le-Fort were at once spectacles of confidence and of efficacy. Husbandmen who wanted their livestock protected from anthrax were shown that, to achieve their ends, they had to go through Pasteur's Parisian laboratory and that Pasteur had to be treated as a transparent spokesman for natural reality. Interests can be generated and translated. Potential consumers of technoscientific goods can be told that they really need these goods in order to attain their existing goals, or that their goals should be modified so as to achieve even more benefits than they had envisaged. Allies have to be enrolled by such persuasive acts and then controlled so that they do not fall out of alignment. Technical means can be found that make the exercise of power over a distance effective. The "immutable mobiles" represented by print and graphic technologies can cir-

[17]Here and elsewhere I knowingly "make a mistake"—common to Anglophone readers—of assimilating Latour's work to existing currents of sociological theorizing. This is to set aside the radical recasting of the terms of theorizing sought by Latour's "amodernist" metaphysics and its bearing on a proper ontological vocabulary for referring to human and nonhuman actors. Ironically, however, this very "misunderstanding" is proving to be the major vehicle for absorbing his work outside of the French cultural context. In Latourian vocabulary, therefore, "enrollment" is proceeding apace while the "control" of allies is notably slack.

culate with minimum modification and represent a world-to-be-controlled on the convenient scale of a tabletop (Latour 1987, 1988a).

Latour's inventory of the means by which technoscientific knowledge is extended amounts to a descriptive vocabulary of power as well as of institutionalization. Pasteur grows great and powerful, his knowledge is extended and made durable, insofar as these effects are achieved. And, while Latour repeatedly disavows both psychological theorizing and explanatory intent (Latour 1988b), the agent deploying these resources is recognizable from Machiavellian and Hobbesian accounts of human nature: Pasteur is displayed as animated by a will to power and domination, and his readers' decisions to acquiesce or submit are treated as those of pragmatic maximizers-of-marginal-advantage. The language of militarism and imperialism is natural to this account, and its suitability is explicitly asserted.

Indeed, one way of situating the Latourian framework within sociological traditions would be to see it as unwinding the solution of a social-order problem which Parsons proffered. The "dog that doesn't bark" in Latour's sociology is, indeed, a conception of normative order. All these effects of order and its extension are to be achieved by constant practices of enrolling, controlling, and invigilating. Latourian social order appears all natural fact and no moral fact. Therefore, the onus on those who suspect the adequacy of Hobbesian accounts of order would be to produce a post-Mertonian picture of the moral economies of science—the locally distributed conceptions of legitimacy, authority, and trust by which scientific knowledge comes to be a collective good, the moral-pragmatic preconditions for intersubjectivity, and the mundane means by which moral orders of scientific knowledge-making come to be distributed around the world.

Despair and Decorum: SSK Dissolved?

No sooner had the dust settled on the first claims of SSK "success" than a number of leading practitioners announced that SSK was a failure and required replacement by more "radical" next-things. The grounds of this despair were several. The program of "discourse analysis" launched in the early 1980s by Michael Mulkay and his students criticized SSK as a form of overenthusiastic sociologizing (Gilbert & Mulkay 1984, Mulkay et al 1983). Rightly observing that scientists' accounting procedures were heterogeneous—sometimes they talked as if work were governed by evidence and method and sometimes as if it were shaped by contingent personal and social factors—Mulkay announced that sociologists could never produce "definitive" descriptions or explanations of science, dependent as they were on the jumble of scientists' talk. At most and at best, sociologists should document and classify scientists' accounts. Definitive description could presumably still be attained, but only by shifting down a referential level, from accounts of what science is to accounts of scientists' accounts of what it is.

In an allied move some of Mulkay's students cast a skeptical eye on the particular form of "interest-explanations" produced by writers in the "Edinburgh School." These too were condemned as instances of sociological over-optimism. How could one use "social interests" as explanations of scientists' judgments when those "interests" ought properly to be seen as objects of negotiation, constructed in the course of interaction (Woolgar 1981, Yearley 1982)? Interests were said to be inadequately established on empirical foundations. They were circularly inferred from the effects they were meant to explain, and they were, for these reasons, illegitimately smuggled into sociological explanations. Here too the "radical next move" out of SSK was, by another description, the recommendation of judicious retreat from a methodological impasse.

Discourse analysis and closely related critiques of SSK have now largely been abandoned. SSK writers embraced the theoretical character of their explanatory notions and wondered what other status "interests" could have. Nor were they content to reduce "interests" to "interest-talk." As Barnes sourly put it, "With cream-cakes there is a chance of satisfying hunger—with accounts of cream-cakes there is not" (Barnes 1981:492–93; cf MacKenzie 1981b, Shapin 1984b, Collins & Yearley 1992:303–04). If proponents of SSK and many philosophers of science claimed that scientific theorizing can never be fully justified—uniquely determined by the evidence—then, of course, the same condition applied to social science theorizing. Nor were the foundational claims made for "discourse" any less vulnerable than explanatory items: The forms of talk discerned by discourse analysts went "beyond the evidence" no less than any other sort of theoretical construct. The "radical" program of discourse analysis was identified as a form of that not-very-radical doctrine, positivism.

Emerging together with the discourse analytic critique was a "reflexive" program. Proponents noted that the discursive forms in which much SSK work was embedded shared with science a realist mode of speech in which authority-claiming authors referred "disinterestedly" to real states of affairs in the social world. This was said to be an unsatisfactory situation, protecting from inquiry that which ought properly to be the object of inquiry. Here the proposed "radical next move" was the purposeful subversion of realist and referential modes of speech. "New literary forms" shattering these univocal and referential modes were to be put in place of descriptions and explanations of scientific conduct, and the objects of inquiry were to be shifted away from "science" and "society" to the "referring self" which had traditionally reported upon "science" and "society." Such questions were asserted to be deeper and more fundamental, and the overarching problem to which reflexivity addressed itself was no less than that of how we know anything at all (e.g. Ashmore 1989, Woolgar 1988b, Mulkay 1991:xvii). SSK was to be not exploded but imploded. To the objection that such practices were getting nowhere, it was robustly replied that "getting nowhere should be seen as an accomplishment" and that the "somewhere" purportedly reached by SSK was in fact nowhere at all (Collins & Yearley 1992: 305).

"Next-step" radicalism again appeared to those defending SSK as yet another counsel of despair (Pinch 1993, Pinch & Pinch 1988, Collins & Yearley 1992:305–9). "New literary forms" arguably have the claimed capacity to break up authority only in the case of quite dim readers. Either no specifiable arguments or claims about science are being advanced through these forms (in which case no note need be taken of them by those concerned with describing or explaining science) or some definite proposition is being advanced (in which case readers would attempt to discern it in the mélange of voices). As with discourse analysis, reflexive writers, for all their trying, could not wholly avoid the realist mode of speech, and one could scarcely imagine that their claims would be in any way comprehensible if they had.

Discourse analysts and reflexivists were partly inspired by ethnomethodology, and, indeed, the specifically ethnomethodological critique of SSK[18] shares their suspicion of allegedly over-confident sociologizing and their attempt to shift attention from "why-questions" to "how-questions."[19] Just as ethnomethodologists condemn the formalism, the reductionism, and the scientism of academic sociology, so they consider the social explanations of science proffered by SSK to be impoverished. Like the stylized accounts of social behavior produced by mainstream sociologists, SSK is considered to be insufficiently curious about the methods by which both scientists and those who study them produce accounts. Ethnomethodologists also reject asocial philosophical rationalism as a response to questions about the grounds of social order in science: The production of social order in scientific disciplines is said to be, in Lynch's formulation, "inseparable from the dense texture of understandings and concerted practices that make up disciplinary specific language games." The traditional concepts and methodological stances of sociology are "simply overwhelmed by the heterogeneity and technical density of the language, equipment, and skills through which [scientists] make their affairs accountable" (Lynch 1993:298–99).

[18]It is notoriously difficult to pin down ethnomethodological doctrine. Here I broadly follow the leading ethnomethodological analyst of science, Michael Lynch (1993:Chs 1, 4–7).

[19]Here it is unclear whether the position is (i) that "how-questions" are more fundamental and should *precede* posing "why-questions"; (ii) that existing responses to "why-questions" are inadequate; or (iii) that "why-questions" are illegitimate in principle and ought to be given up. In the event, it remains uncertain how, in any strong sense, "how-questions" could be thought to *replace* "why-questions."

Consequently, ethnomethodology, like strands of SSK, has commended ever more finely grained studies of day-to-day scientific practice. It has been a major inspiration to work displaying the mundane and everyday character of knowledge-making, while, on a programmatic level, it has expressed doubt that sociologists currently possess the conceptual resources to explain or even schematically to describe scientific order. To that extent, the ethnomethodological posture is a form of asceticism. Yet that same unremitting asceticism has made ethnomethodologists reluctant to advance some of the more expansive methodological claims staked out by other critics of SSK. Ethnomethodology, at least in Lynch's form, does not assert a privileged stance for any form of sociological accounting; it does not see foundations or Archimedean points available anywhere; and it recognizes no reason to be troubled by or to abandon a realist mode of speech. What makes critics of ethnomethodology despair is just the scope of its ascetic modesty.

Finally, for the past ten years or so, Bruno Latour and his associates have publicized their view that sociological explanations of scientific judgment are outmoded, fundamentally flawed, and due for replacement. The traditional vocabulary of the sociology of knowledge, which asked how "social factors" influenced scientific knowledge, needed to be replaced with studies of how nature and society were "co-produced." SSK was to be applauded for its devastating critique of philosophical rationalism, while its residual ambition to explain nature by reference to society was to be definitively rejected. Just as philosophers were wrong to use natural reality to explain scientists' beliefs, so sociologists were wrong to use social reality toward that end. Analysts were told to be as curious about how society was constructed as they were about the construction of natural knowledge. What was wrong with SSK was that it was, after all, a form of sociology, using the categories and seeking the goals of the sociological realist: "[T]he social sciences are part of the problem, not of the solution" (Latour 1988b:161).

The notion of the agent—taken as the volitional human actor—is central to the sociologist's vocabulary, and it is in connection with Latour's attempted reconceptualization of actors that his work has generated the greatest excitement, bafflement, and exasperation. Agency, like "interests" and "nature," is to be regarded as the outcome of controversies, and we must not use such outcomes to explain the career of controversies. Accordingly, Latour means to develop a mode of talking about science and society that does not prejudge the location of agency, in particular as between humans and nonhumans: "[I]t is very important ... not to impose any clear distinction between 'things' and 'people' in advance" (Latour 1987:72). In present-day science studies, confusion reigns about whether what is being offered is a scheme identifying the semiotic equivalence of human and nonhuman "actants"—which, while exotic to Anglophone cultural inquiry, does not necessarily impinge upon ordinary realist speech—or whether genuine ontological claims are being made, with attendant prescriptions for proper speech in science studies and in the wider culture.

It is this aspect of Latour's work that is currently proving most attractive to analysts of science with "posthumanist" sensibilities. So Andrew Pickering—formerly a leading exponent of SSK—now advocates a "drastic overhaul of some of our most basic intuitions ... about the world, human and nonhuman"; "[o]ne very distinctive feature of modern technoscience is ... its capacity to unleash upon the world new and nonhuman actors ... " (Pickering 1993a:104, 112; also 1993b, Law 1986a,b). Latour's "actor-networks" and Haraway's "cyborgs"—part human, part nonhuman—transcend the "discredited" humanist and modernist dualisms and are the appropriate units of analysis for writers who wish to talk about making society and making science in the same idiom and without commitment to a putatively modernist ontology. Anyone who wishes to understand modern science and modern society must supply themselves with a new way of talking that reflects the new realities.[20] Like the seventeenth-century "moderns," some postmoderns evidently still yearn for a privileged language whose recommendation over alternatives is that it mirrors the order of existence.

Archimedes' s Return

These critiques of SSK are a heterogeneous lot, and it would be wrong to assimilate them too confidently to a common source or sentiment. There are, nevertheless, some family resemblances. First, the critiques proceed largely through identifying SSK as a form of sociology. Its sins are said to consist in its genetic relationship with the parent that commonly denies the offspring as its own. That this irony has largely escaped practitioners presumably stems from the circumstance that so few of them have substantial commitments to the parental discipline. Almost needless to say, there is no reason automatically to deprecate that circumstance or these criticisms. Neither commitment to fundamental sociological resources nor the capacity to contribute to sociological inquiry necessarily depends upon the forms of professional membership. Nor is it a prudent course for an academic discipline to ignore or seek to ban fundamental criticism. Indeed, the baroque reflectiveness of the science studies community throws into relief major features of the sociological enterprise which more complacent and peaceable specialities are less commonly obliged to confront.

Second, these critiques of SSK, and, by extension, of sociology, have a

[20]It is not at all clear whether such claims are indeed specifically tied to nineteenth- or twentieth-century realities, or whether they are meant to have wider temporal scope.

skeptical character. Typically, they are skeptical about the claimed capacity of sociological categories to explain or reliably describe the scientific objects of inquiry. Skepticism has an ancient pedigree; it corrodes complacency and convention, and for that reason alone the skeptic who makes life so awkward for the securely institutionalized practitioner should be cherished like the most maddening of mad uncles in a well-knit family. As Collins (1992:6) puts it, skepticism has the virtue of being a "safe, legal and inexpensive [way] to loosen the trammels of commonsense perception." In this case, the skeptic's voice has challenged the legitimacy with which sociological descriptive, interpretative, and explanatory categories have been applied, and they have challenged the validity of the categories themselves. Versions of this skepticism target not only the categories of academic sociology but, importantly, those of realist modes of speech entrenched in our own culture.[21] SSK itself is, after all, a form of skepticism—for example, with respect to the traditional vocabulary of "social versus cognitive factors." The effect of this skepticism—both that of SSK and of its critics—has been, in my view, overwhelmingly constructive. If, indeed, there was any taken-for-grantedness about what it was to give a sociological description, interpretation, or explanation of science, it has now been buried under an avalanche of methodological self-consciousness.

Third, and arguably in tension with the skeptical posture, these critiques—with versions of ethnomethodology probably excepted—have also typically betrayed a millenarian optimism. Existing sociology is said to be insecurely founded. Yet if only we could get our concepts or discourse right, if only we could take one more reflexive turn, if only we could go down one more analytic level, if only the right, theoretically neutral metalanguage could be devised, then at last we would reach intellectual terra firma and all would be well. However, far from being a "radical next move," there are no intellectual aspirations more traditional than the quest for foundations: a pure and uncompromised place beneath, above, beyond, or apart from the compromised categories of the culture to which intellectuals mundanely belong. In other moods, critics of SSK have themselves made major contributions to discrediting foundationalism. Yet in their struggle to escape the constraints of sociology, they have fallen into the oldest temptation ever to afflict intellectuals. If the move from traditional sociology of knowledge to SSK was the abandonment of pretensions to privilege and of "the Archimedean point," then the unwitting thrust of these critics of SSK is that such a point can, after all, be found.

[21] And here the break between *interpretative* sociological goals and strands of postmodern science studies and Latourian practice is most apparent, since, to my knowledge, no past or present-day scientific community trades in "stronger or weaker heterogeneous networks of actants" while all consequentially mark out domains of the human versus the nonhuman.

If there is an authentic sociological voice to be set against individualism, empiricism, and positivism, then that voice says "It cannot be done"—not in science and not in the study of science. The "cage" from which the critics evidently seek escape is not just sociology, but the realist mode of speech which sociology shares with everyday talk. That robust realism is said to be the problem to which there must be a remedy. To be sure, the categories of mainstream sociology are not immune from important criticism just because they are a version of the realist mode of speech, but neither can criticism intelligibly suppose that the realist mode can be replaced. The "cage" from which "escape" is sought is, in fact, a condition of such liberty as we enjoy. Intellectuals are not obliged to leap free from their culture in order to subject their culture to questioning, nor must the great, and allegedly "modernist" or "humanist" dualisms be replaced in order to be skeptical of them. Notice, for example, that Latour's idea of "heterogeneous networks" is wholly intelligible, and that the condition of its intelligibility is reference to entities plucked from the culture's existing realist repertoires: human, nonhuman, science, society. And if "modernist" dualisms were a "trap," then it would follow that late twentieth-century culture could contain no such thing as a "materialist theory of the mind." The fact that there is such a theory indicates that we are not, evidently, ensnared by the categories of realist language at all.

There are, however, limits to skepticism about the categories of the common culture, and those limits are posed by the boundaries of communication. We can develop and put in place arcane languages, but we cannot ensure that others will hear us. Communicative orders are grounded in local natural attitudes and local realist idioms.[22] If we wish effectively to speak to a specified community, we are obliged to share its realist idiom. And if we want to communicate at all then we are obliged to employ some version of the realist mode of speech. That obligation is, properly speaking, a constraint. It means, in the present case, that intellectuals' intelligible communication about modern scientific culture will always be compromised by the cultural categories shared between ourselves, the laity, and the scientists we talk about. And if that speech is not so compromised, then it will not be intelligible. Discontent with that formulation would, indeed, be a measure of the extent to which sociology has been rejected or ignored.

[22]See Collins's argument (versus Latour) in favor of "sociological realism" as sociological decorum (Collins & Yearley 1992). No one realist mode is privileged, but we can and should, Collins says, seek to "alternate" between realisms. We suspend irony about our local realist presumptions as a "methodological convenience."

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