SUSAN LEIGH STAR

Introduction

In 1967 Howard Becker wrote an article that became a clarion call to sociology, his presidential address to the Society for the Study of Social Problems. It was entitled "Whose Side Are We On?" and reminded sociologists that pretensions to value neutrality were themselves value laden. He argued that we must choose to recognize that all perceptions are located in a hierarchy of credibility. In other words, people consider the source of any statement or perception, and discount those produced by lower-status people.

Whose side are we on in social studies of science and technology? What hierarchies of credibility are we tacitly or explicitly assigning? And what language can we invent to investigate these questions honestly?

We could do well to borrow from Patricia Hill Collins's (1986) essay on black feminist thought and its contributions to the structure of sociological knowledge. She argues that African American women's radical explorations of the meaning of self-definition and valuation, the interlocking nature of oppression, and the importance of redefining culture constitute a challenge to sociology's basic beliefs about itself. The challenge takes the form of asking sociology to learn from "the outsider within"—the double glasses of insider and outsider, articulating the tension of both being a sociologist and being excluded by its frame of reference.

The papers in this volume are all attempts to frame the question of whose side we are on by examining science as a radically contextual, problematic venture with a very complicated social mandate, if any. Our purpose here is more than polemics; rather than valorizing or denigrating science as a monolith, we are taking an ecological view of work and politics. And we, too, are "outsiders within," as Woolgar's essay in this volume argues—both strangers and intimates in the world of science. Our work challenges the moral order of science and technology making—and in turn places us in a complex, often tense moral position.

Ecologies of knowledge[1] here means trying to understand the systemic properties of science by analogy with an ecosystem, and equally important, all the components that constitute the system. This is not a functional (or functionalist) approach, with a closed-system organic metaphor at its core. As Sal Restivo notes in his description of science as a social problem, we want to approach science as a set of linked interdependencies inseparable from "personal troubles, public issues and social change agendas," not a social structure with one or more *dysfunctional parts*. Science and technology become monsters when they are exiled from these sorts of questions (Law 1991; Haraway 1992b; Star 1991a; Clarke, in press b), just as other symbolic monsters have been borne from the exile of women's strength from the collective conscience, or in the demonizing of people of color. In Michele Wallace's words: "The absence of black images in the reflection of the social mirror, which such programmatic texts (from *Dick and Jane*, to Disney movies, to *The Weekly Reader*) invariably contract, could and did produce the void and the dread of racial questions . . ."(1990). In our exorcism, we simply want to see scientists and technologists as ordinary, as citizens, neither villains nor heroes.

Each of the authors in this volume calls in different ways for an ecological analysis, including a restoration of the exiled aspects of science. Thus by *ecological* we mean refusing social/natural or social/technical dichotomies and inventing systematic and dialectical units of analysis. I think this reflects the dissatisfaction with conventional ways of approaching organizational scale and units of analysis, a dissatisfaction brought on no doubt in part by our respondents (scientists and technologists), who themselves are continually plagued by these questions. Restivo and Croissant, in this volume, examine a large-scale set of relationships, as between science and all other institutions and social arrangements. Kling and Iacono put the context back into analyses of technological innovation, noting that only such an approach can overcome simpleminded technological determinism or technocracy. Star makes a similar point for the class of "artifacts" called formalisms, or formal representations, and their relationship to organizational complexity. Fujimura looks at the ecology of the workplace in a Hughesian sense, and thus goes beyond the simple adoption of new technologies as a determining factor in scientific social change. Law and Callon, Woolgar, and Latour each break the traditional boundaries of what can be included in an analysis of technology and social organization, recommending a broader, more democratic kind of analysis that is both moral and deeply ecological. In this, Law and Callon call attention to the local expertise of the scientist/engineers who, in their eyes, make no distinction between *technae* and *politaea* (Winner 1986). Woolgar refuses another "great divide" in so doing—that between individual psyche and collective repertoires of behavior. Clarke challenges us to examine the stuff of science—the material substrate—instead of ignoring it in the service of idealist theories. Lynch's work extends this point to a revaluation of the idea of "place" in science, arguing instead for distributed, material " topical contextures" in which to examine scientific genre. Latour uses the device of analyzing an ordinary, low-tech system—the door—and its surprising complexity to point to the inseparability of the technical and the social.

Our key questions here are those of general political theory and of feminist and third world liberation movements: *Cui bono*? Who is doing the dishes? Where is the garbage going? What is the material basis for practice? Who owns the means of knowledge production? The approach begins in a very plain way with respect to science and technology by first taking it "off the pedestal" (Chubin and Chu 1989)—by treating science as just something that people do together. Some of this means looking at science and technology as the occasion for people to do political work—not necessarily by other means, but fairly directly. Science as a job, science as practice, technology as the means for social movements and political stances, and science itself as a social problem—collectively, these articles take science/technology as the occasion for understanding the political and relational aspects of what we call knowledge. This introduction situates these papers with respect to science and technology studies (STS), and gives a sense of the problems they are addressing in social theory.

Most work in STS has not been seen as general social theory or as contributing in a fundamental way to social science theory. While historians, philosophers, and even computer scientists show a great deal of interest in the new sociology/anthropology of science, most social scientists view it as a kind of luxury, an arcane corner of the discipline offering only specialized insights. One purpose of this book is to demonstrate that social studies of science and technology are addressing a set of questions central to all social science. In this selection of science studies research, readers will see that science and technology are the vehicles for analyzing some very old questions. How do people come to believe what they believe about nature and social order? What are the relationships between work practices and social change? What is the trajectory of social innovations? Who uses them and for what purposes? As people from different worlds meet, how do they find a common language in which to conduct their joint work? How can we study people's work critically, yet as ethnographers or historians respect the categories and meanings they generate in the process? Finally, what are the boundaries between organism and environment; how fixed are they; how can we know them; and are they meaningful a priori?

Perhaps because learning another scientific language is a prerequisite for doing the kind of social studies of science/technology described herein, scholars in science studies tend to be an omnivorous bunch. We read history and philosophy of science as well as scientific tracts in the substantive fields we study, feminist theory, and *Science* magazine. We often borrow models from those writings as well as from other areas of social science. We work across national boundaries in informal groups clustered around an analytic topic: the use of metaphor, for example, or the extent of technological determinism or infrastructural change. The field is small (although growing), lively, and filled with debates, cross-fertilization, and often surprising collaborations. For example, sociologist Michael Lynch, who is interested in visual representation (and whose paper on the topic appears in this volume), has collaborated with art historian Samuel Edgerton on analyzing the pictures astronomers create (Lynch and Edgerton 1988). Steve Woolgar became the project manager of an industrial computer development firm in order better to understand the process of technology construction. Diana Forsythe (1992, 1993), a cultural anthropologist by training, has worked for many years in collaboration with computer scientists building medical expert systems, both critiquing the notion of expertise and acting as a designer. Because the field is so interdisciplinary, the term *science studies* often replaces the disciplinary-specific label, such as *sociology of science.*

HISTORICAL REVIEW

In the United States, most sociology of science before the late 1970s was dominated by the work of Robert K. Merton and his associates (see, for example, Merton 1973; Zuckerman 1979) and by a group of researchers conducting bibliographic citation analysis (from which work came the concept of "invisible colleges"; see Crane 1972; Mullins 1973). While there had been much criticism of functionalist sociology on a number of fronts, particularly from symbolic interactionism and Marxism, the sociology of science received scant attention. Symbolic interactionists, for example, had studied work and occupations, medicine, deviance, gender, the family, urban life, and education, critiquing functionalism in each of these areas. Yet they had produced only a few scattered monographs and articles on science (for example, Glaser 1964; Strauss and Rainwater 1962; Marcson 1960; Becker and Carper 1956; Bucher 1962) and had undertaken no large research programs in this area.

Meanwhile, in Europe in the early to mid-1970s,[2] a group of researchers began a series of studies to demonstrate, contra Merton, that science was not "disinterested, communistic, and universal." Many of them had also been deeply influenced by Kuhn's (1970) *Structure of Scientific Revolutions*, a book that had questioned the cumulative nature of science and raised the issue of the incomparability of scientific viewpoints or paradigms. They were concerned to show that science was not neutral, that the outcomes and content of science as well as access to it as a profession were determined by structural commitments, political positions, and other institutional considerations. MacKenzie's (1981) work on the interrelationships of statistics and eugenics is a good example of these efforts. These researchers were also concerned to demonstrate the constructed nature of science and its view of nature. Thus, they were strongly antipositivist. Some of this work was done at the University of Edinburgh, and the "interests" model became especially associated with the "Edinburgh School" (see, for example, Barnes 1977). Other important centers with overlapping approaches were in Paris and Amsterdam.

In 1979, Bruno Latour and Steve Woolgar published *Laboratory Life*, probably the book from the field that is most well known outside of science studies. The book was an ethnographic study of a scientific laboratory, and its purpose was to document the creation of a scientific fact. Using a variety of techniques from anthropology, semiotics, and ethnomethodology, they traced the birth of a biological fact in the context of lab work. They concentrated on a process they called "deletion of modalities," a progressive stripping away of contextual information about production, with the end result being a fact bare of its own biographical information. The book was an immediate success and was one of the factors helping spawn a series of laboratory studies and descriptions of act-making, often ethnomethodological in approach.

The combination of fieldwork and antipositivism was familiar to American symbolic interactionists, who welcomed the chance to apply these techniques to science and to learn from colleagues in Europe. A number of collaborations ensued among researchers in America, England, France, and the Netherlands pursuing these viewpoints. (See Fujimura et al. 1987; Clarke and Gerson 1992; Star 1992b; Clarke 1990; 1991 for reviews of this work.) Among our common interests and beliefs was the necessity of "opening up the black box" in order to demystify science and technology; that is, to analyze the process of production as well as the product. Methodological directives for those of us working in the interactionist tradition were familiar: Understand the language and meanings of your respondents, link them with institutional patterns and commitments, and, as Everett Hughes once said, remember that "it could have been otherwise." Many of our colleagues in Europe held similar views, albeit from very different traditions: Do not accept the current constructed environment as the only possibility; try to understand the processes of inscription, construction, and persuasion entailed in producing any narrative, text, or artifact; try to understand these processes over a long period of time (some of this work is represented in Law 1986a; Bijker et al. 1987; Callon et al. 1986).[3]

There were and are many other groups throughout the world studying science or technology: policy makers, historians, analysts of the impact of technology (particularly computing and automation), and the number is growing rapidly. Another important development began as programs of social science research gelled into Science, Technology, and Society programs at a number of technical institutions and regular universities. New undergraduate and graduate STS programs began to spring up, both within traditional departments and as interdisciplinary programs. Early STS programs represented an amalgam of interests: ethics and values in science and engineering, studies of social impacts of technology, and history of science and technology. They were often an academic home for science criticism, that is, studies that demonstrated scientific bias (racism, sexism, classism) or danger resulting from scientific and technological research and development (nuclear and toxic wastes, recombinant DNA, technological disasters). Criticism of the sacrosanct institution of science and explication of the constructed nature of nature have remained core problems in science studies, and there is currently lively debate about the role of activism in the field, as Restivo and Croissant indicate in their paper for this volume.

QUESTIONS OF ORGANIZATIONAL SCALE

Questions of organizational scale have always plagued (or some might say, graced) social science. Is social change individual or aggregate? How can we understand the relationship between social facts and individual experience?

These questions appear in science as it is interlaced with presumptions about the nature of scientific inquiry. If researchers accept that nature is simply "out there" waiting to be discovered, they may append to that belief the idea that "anyone can do it, geniuses better than the rest of us." There is nothing that logically ties these two together, but much of the received mythology about science involves great men (*sic*), great moments, great labs, and great accidents of Nature revealing herself. This combination of individualism, positivism, and elitism conspires to confuse the question of the appropriate level of organizational scale at which to conduct inquiry. So, the secondary literature on science is littered with psychologism, reified "societies" that act in mysterious ways on believers, and participant histories that claim exclusive centrality for powerful, rich institutions and people.

Against this trend is a lively debate, partially represented in the pages of this book, about the right unit of analysis for studying science. In escaping from the nasty things mentioned in the previous paragraph, sociologists and anthropologists of science have invented, borrowed, or transformed units of analysis from other parts of the discipline or from science itself: bandwagons, social movements, political economy and large-scale work organization, units of action and activity that cross human/nonhuman boundaries, the taken-for-granted truth about Nature that reflects old and widespread conventions (and superstitions).

How is the little black box of the computer, the test tube, or the door-closer joined with phenomena at larger scales of organization? This is a fundamental question about science and technology, but it appears whenever one questions the nature of local social arrangements as articulated with those at a distance or with considerably more power and purview. All of the articles herein attempt to answer this question ecologically and propose several modes for doing so.

At the largest scale of organization, questions are raised here about the utility and role of science or technology in maintaining or changing the *status quo*. This is asked not simply in terms of technological determinism, but in terms of larger scale issues, a central one being: Can there be a revolutionary science/technology in the absence of revolutionary social change in other spheres? To the extent that one believes in the interpenetration of spheres and science as a social institution of its historical time and place, the answer must be no. This puts the question of political commitment squarely at the center of science studies. For one thing, it is difficult to escape examining oneself as a scientist while engaged in studying scientists full time. Truly revolutionary science or technology thus means full-scale revolution. The sociology of science might allow us better to understand what that might mean.

METHODOLOGICAL ISSUES

Interwoven with questions of scale and politics are questions of method. Scientists are very challenging respondents. For one thing, all scientists share with us concerns about reliability and validity of data, robustness of findings, and the meaning of those findings. I never met a scientist who had not thought about the issues raised in this introduction. As a group of respondents, scientists are particularly difficult and rewarding because they have often thought rigorously about the issues we are investigating, and about which we are ourselves uneasy (Woolgar, this volume). So the work of our respondents blends with our own. The meaning of participant observation in this case can begin radically to change.

There are several kinds of work to be juggled in doing research in STS, each of them methodologically challenging. First, there is the map and language that the scientists themselves use in their work. Second, there is our mapping of the work practices and organization. Third, we create maps of the communications between domains. Fourth, a complex "nested" map is generated that shows who answers to whom, and why. It is at this level that questions of unit of analysis, or scope, often show up in force. Reconciling the different maps is a nontrivial methodological problem, again common across many domains of social science and political life.

WHY I AM NOT A NAZI: REALISM AND RELATIVISM IN SCIENCE AND TECHNOLOGY STUDIES

One of the curious things about being in the STS field is that one is immediately plunged into philosophical debates about realism and relativism. Briefly, realism is the position that "there really is a there out there, and it's true in some absolute sense." Relativism holds that truths are relative to a place, time, or person (often a historical situation or geographic/cultural location). All researchers in science studies have had the experience of being challenged about the "underlying truth" of science: What about the scientific method? What about truth? What about the laws of gravity?

I have been involved in science and technology studies for about fifteen years, first as a science critic, then as a historical sociologist and ethnographer. I have given over one hundred talks on various aspects of the sociology of science and technology. In almost every presentation, I have been asked some version of what I now call the "there there" question: But are you saying it's *all* socially constructed? Doesn't that mean anything could be true? Isn't there anything out there? Are you saying that scientists are making it all up? Are you saying germs don't really make you sick, or gravity doesn't really make things fall down?

It is indicative of the central place of science in mainstream Western belief systems that *merely to imply* that the acquisition of scientific knowledge is work, not revelation, seems to involve the kind of radical idealism (if not radical autism) alluded to above. But this is not necessarily the case.

To say, as Hughes did about social order, that "it could have been otherwise" is not to say that it *is*. And to say that the conditions of nature or science are the result of collective enterprise that includes humans and nonhumans is not to imply that the merest whim on the part of an individual could overturn them. Rather, as social scientists, let's ask: Under what conditions do such questions about reality routinely get raised?

First, the term *socially constructed* is a reformist term, inserted into titles in sociology/anthropology of knowledge and science. Its initial purpose was to demonstrate that the reports of science that had been stripped of production history were missing important historical and situated accounts; second, to restore accounts of the actual work and its organization to those reports. Furthermore, if one takes "society" as the scientific problem, then the image of a society "out there" structuring an experience that is then entered into the canon of research doesn't make any sense either.

I call the idea that cumulative collective action is flimsy the "mere society" argument. It is paralleled by simplistic perceptions about, for example, socialization and gender. The argument goes something like this: "So, she's been socialized as a girl. Well, let's just let her into the corridors of power and de-socialize her, and then everything will be ok." Such a statement depends on a trivial and reified conception of both socialization and gender. Whatever bundle of actions, past and present, we might think of as "socialization" here is far more complex and durable than most of us realized in the early days of feminism. Similarly, the notion of "institutionalized racism" has been crucial in understanding that racism is not simply a matter of people not being nice to each other, nor necessarily to be found in a single set of micro-interactions—rather, it is a web of racist discourse and practices that extends through and informs all human practice—and cannot be simply transcended (hooks 1990). The durable bundle of actions and experience that comprise "science" has a similar sturdy complexity. This complexity does not defy its ontological status as "created," however. The constructivist or relativist schools in science studies (and I will not explicate the subtle differences between them here) have often been accused of flimsiness or mentalism on grounds that deeply confuse epistemology (how do you know what you know) with ontology (how are you what you are).

Scholars in science studies have disagreed about this issue and will continue to do so for some time to come. Yet a thread runs throughout the work of the groups represented in this issue: Let's replace the either/or dichotomy of constructed versus real with more useful concepts. Concepts such as workplace ecology, *irréductions* (Latour 1987), sociological imagination, networks and translations, and boundary objects (Star and Griesemer 1989) are important here. Wimsatt's (1980) concept of "robustness" has similarly been an important replacement for more restrictive concepts of reliability and validity. He, borrowing from biologist Richard Levins, defines it as "the intersection of independent lies," or more sociologically, the durability of collective action despite the fragility of any one instance.

During the 1980s there were scores of articles and books addressing this class of questions in science and technology studies. They have important links as well with earlier work in other parts of sociology and anthropology. For example, the debate in the 1960s about labeling deviance asked whether some things aren't *really* sick (or unnatural). The sociology of art has been concerned with the question of whether some things aren't *really* just beautiful (in a timeless or transcendental fashion).[4] The enduring concern with ethnocentrism in anthropology has recently exploded in debates about the place of the anthropologist and whether the knowledge constructed by anthropology is rightly seen as a jointly created fiction. In sociology and anthropology of medicine, the debate occurs as a question about whether one can differentiate physiological disease from "illness behavior"—aren't some things *really* just germs?

But the analytic trick in each of these cases is to raise the concept of "really" to the status of rigorous, reflexive inquiry and ask: *Under what conditions does the question get raised?*

One of the difficult things about trying to analyze an institution as central as science is that one challenges the received views of things for audiences and respondents. In giving talks that defend the above position, I have sometimes been called a Nazi, or parallels have been drawn between the social construction of science and Nazi science. It took me a while to figure out what people were talking about in these accusations, since being a Nazi is anathema to me.

If one takes the point of view that fascism requires a kind of situation ethics and requires that one redefine the situation according to opportunism or a kind of distorted view of science and nature, then any attempt to make relative any situations (especially natural or scientific ones) becomes morally threatening. This is so because one antidote to fascist ideology is to affirm an overriding value in human life, a universal value that cannot be distorted by the monstrosities informed by local, parochial ideologies of racism and genocide. Ethicists often base their arguments on this presumption. The worst thing for an ethicist is to hear arguments that plead "special circumstances"—the name of the game is finding good universals.[5] Yet this criticism of relativists as Nazis shows another kind of confusion, which again relies on a separation of the social and the natural and a separation between the conditions of production and the product. If the relative ontological status of a phenomenon is inextricably embedded in the conditions of production, then it's not a question of an analyst legitimating genocide or situation ethics. Rather, the question on a meta-level becomes: How can we make a revolution that will be ontologically and epistemologically pluralist yet morally responsible? Can we be both pluralist and constructivist, hold strong values and leave room for sovereign constructions of viewpoints? These are not new questions, either; both the French and American Revolutions were fueled by them. I would claim that there is stronger evidence for Nazism arising from ignorance of the conditions of production of knowledge than from exploring the relative configurations of these conditions in different times and places; more oppression from the appeal to absolute natural law than from negotiations about findings. While I'm not implying here that science studies is the best weapon against totalitarianism, the fact that this question arises so frequently in so many different contexts is to me indicative of the fact that science has been such an inviolate institution, certainly in academia.

CURRENT INTELLECTUAL DEVELOPMENTS IN STS

Taking on science as a social construction grew beyond either interest explanations or laboratory ethnographies by the end of the 1980s. Science and technology studies (STS) has over the past several years worked hard at two central intellectual currents, both of which are at the core of an ecological analysis of science (or perhaps, in some sense prior to it). The first is the establishment of science as materially based (see especially Clarke and Fujimura 1992a and 1992b; Haraway 1989; Clarke, this volume; Lynch, this volume); the second is science as a form of practice (see especially Star 1989a; Pickering 1992).

It is remarkable for how long accounts of science (in history, philosophy, and sociology/anthropology of science) neglected to notice that much of the activity we call science consists of people manipulating materials , including specimens, media and cultures, breeding colonies, and display items. This material culture of science is important not just as another form of exoticism, but for the ways in which it is constituent of scientific findings and constraining of the ways we perceive scientific meaning.

OF HUMANS AND NON-HUMANS

One of the issues that appears in different ways in the papers in this volume is the issue of "where to draw the line" in analyzing science and technology. Traditional studies usually drew the line at the edge of the black box, whatever it might be: the computer, the laboratory, the closed scientific work group. The argument in this volume is that science studies in the past have left out some of the most important actors, the "nonhuman" ones. Many of the new sociologists of science are engaged in a kind of democratization of this analysis, as the papers here demonstrate. If one adopts an ecological position, then one should include all elements of the ecosphere: bugs, germs, computers, wires, animal colonies, and buildings, as well as scientists, administrators, and clients or consumers (see Clarke and Fujimura 1992b and Latour 1987 for an analysis of this). The advantages of such an analysis are that the increased heterogeneity accounts for more of the phenomena observed; one does not draw an arbitrary line between organism and environment, one can empirically "track" lines of action without stopping at species, mechanical or linguistic boundaries, and especially without invoking a reified conception of society.

On the other hand, this kind of analysis presents some serious ethical problems—on both sides (Singleton and Michael 1993). For many years feminists, radical ecologists, and pantheists have recommended a kind of analysis that does not exclude anything from the natural world. The exclusion of animals, the biological environment, and other parts of the natural context has been one of the major sources of alienation under patriarchy, late capitalism, or religions that are antinature (Griffin 1978; Merchant 1980; Harding 1991). Restoring the natural world to the research context would be an ethical and political advance. On yet another hand, the papers by Kling and Iacono and that by Star are written from within a research context in which it is not humans who have been privileged at the expense of nonhumans, but vice versa. It is computers and automation that have occupied a privileged position vis-à-vis human beings, often because of the inadequate social analysis held by computer movement advocates. An ethical social problems position in this case most likely involves checking the power of nonhumans and their advocates, and seeing that humans understand it contextually, not democratizing the nonhuman position. Thus where Latour is concerned to restore ecology from one side, Kling and Iacono and Star are concerned to restore it from another.

What are the moral values invoked by such analyses? I think that there are no simple answers. The dividing lines should not really be between advocates of humans and advocates of nonhumans, but between ecologists and reductionists. In furthering the cause of an ecologically responsible, socially and philosophically sophisticated analysis of science and technology, we need to confront head-on questions of scale, of boundary drawing, and of mystifying science and technology, *as well as* questions of race, sex, and class. To do that, recursively and reflexively, we need an ecological approach.

A recent collection of papers in the sociology of science highlighted this debate in the field (Pickering 1992). Collins and Yearley's paper in that volume accuses Latour and Callon of playing "epistemological chicken" in the interests of advancing the nonhuman analysis at the expense of the human. Callon and Latour respond with a defense of their position, claiming it as a heuristic analytic device that pushes the boundaries of science studies beyond reified sociological categories. Fujimura's paper in the volume, taking a symbolic interactionist/pragmatist and feminist perspective, rejoins with the claim that neither side has comprehended the human stakes involved, and that when the debate is phrased as humanists versus poststructuralists, once again concerns of all women and men of color, as well as other minorities, are ignored.

The debate between the British and the French, on the one hand, and Fujimura's claim that from an American pragmatist perspective the issues are misframed are important for the ecological analyses presented here. If we take ecological to mean treating a situation (an organization or a country or interactions and actions) in its entirety looking for relationships, and eschewing either reductionist analyses or those that draw false boundaries between organism and environment, then indeed the human/nonhuman question is reframed. The axes within the ecological space are four:

1. continuity versus discontinuity
2. pluralism versus elitism
3. work practice versus reified theory
4. relativity versus absolutism

On the left-hand, radical side go continuity, pluralism, and relative ecologies of work practice; the reactionary side is discontinuity (or divides great and small), elitism (or pretensions to a single voice), reified theory (or deletions of the work in representations of it), and absolutism (or "there really is a little bit of determinism").

A central fight within American sociology, and subsequently within sociology of science as discussed above, has been against functionalism, that school of thought which sees a closed-world, top-down, organismlike social order that draws its imperative from an imputed physiology-writ-large. The sociological field in America is mined with "hot spots" that come from the scars of these historical battles. From the pragmatist side are the words *consensus, boundary maintaining, natural*, and *obvious.*

The fights between the British and the French resonate along these axes in three ways. First, the relativism of both schools satisfies the pragmatist concern with pluralism. They both imply that there are frames of meaning, definitions of situations, different perspectives based on experience. Due to the long history of fighting that pseudosingular voice, such pluralism became the salient relativist dimension for pragmatists/interactionists in STS. The fact that it also has profound resonance along the axis of nature/society, people/things, and so on doesn't carry as much historical weight, for the reason that pragmatists never believed in that divide in the first place. John Dewey and Arthur Bentley, for example, spent their entire careers fighting these notions in analytic philosophy; they appear as similar fights in the work of symbolic interactionist sociologists influenced by them, such as Howard Becker and Anselm Strauss. So the work of Collins and Pinch (1982) on the different valuations placed on parapsychology and psychology resonates with that commitment to pluralism, too, seeming to restore the voice of the underdog to scientific debates and balance it out—to even out the "hierarchy of credibility" discussed above.

The French actor-network theories, and their emphasis on the inclusion of nonhumans (see Latour, this volume), find a match in the pragmatist concerns about continuity and process. Because the mandate of the pragmatist research program since the 1920s has been to "follow the actors," it is not surprising that there have developed strong ties between French researchers and American symbolic interactionists.

A central tenet of the pragmatist work in STS has been to think of scientists as people who are doing a certain kind of work.

Among other things, science is a job. It's a very interesting one, because it turns out that even *calling* it a job invokes the wrath of American functionalists, most philosophers, many deans and administrators, and most computer scientists. Simultaneously, this vision of science as work invokes the appreciation and support of many historians.

As a symbolic interactionist, I agree with Callon and Latour and Lynch and Woolgar that new methods that will lead us across traditionally accepted boundaries are crucial, and those that will help manage the "rich confusion" of things and people are absolutely critical for science studies at this time. Because of my pragmatist concerns with work, I would add work itself to the rich confusion, in the form of activity, practice, and/or work organization. I would also like to emphasize a neglected dimension in the worlds of STS and science/technology: the great divide between formal and empirical.

Work, Formalisms, and Divides. One of the most confusing actants in this complex ecology is the work of scientists and technologists who create formalisms,[6] including those working with information technologies. The impact of STS in these spheres is not so much limited by concerns with relativism/realism—indeed, here Latour's early point about scientists not being naive realists is absolutely true. But many scientists I've known *are* naive formalists, especially those in information technology and computer science. One thing about computing technology is that it allows one, paradoxically, to use a very concrete thing to manipulate representations that are quite formal. The great divide that computer science itself then produces is between the formal and the empirical—this is reproduced many times across the sciences, including social sciences. On the one side are formalists who believe that computers are embodied mathematical theories—theories come to life. (I do not exaggerate here; if anything, I am understating the case.) On the other side are engineers who argue that *only* making things (e.g., machines, programs, new speed records) really counts for technological advance. And on a third (much, much smaller) side are a few brave souls who argue (mostly for reasons of safety or ethics) that empirical studies must join together inseparably with formal models. They are as yet few in number, and have suffered enormous academic stigmatization for their stance. Some details of the debate in computer science itself can be found in Newell and Card (1985, 1987) and Carroll and Campbell (1986).

These pieces and Fetzer (1988) give a sense of the vituperative flavor of the debate. Star (1992b) reviews the work of people in several disciplines working in this "third force" (see also Star, in press b).

The formal/empirical canyon is a complex and compelling great divide (see Bowker et al. 1993 for a discussion of the issues), one that is only beginning to receive attention in STS. And here are the stakes. A formalist would argue that when building air traffic control systems, human fallibility and bias is such that we are virtually killing people to rely on it. The computations are so many and so intricately interconnected that only a machine can be smart enough (statistically, and there's the rub) to do them fast enough before the (now much more complex) airplanes fall out of the sky. The empiricist argues that all such mathematical systems are unprovably fallible (using strong formal allies like Gödel and Schrodinger), too big or too dangerous to test, and that we'd better stop the reliance on formal testing or we will all (literally) be blown sky high by Star Wars or a series of high-tech accidents.

A significant divide indeed, since it captures all life forms in its technological threads. . . .

The formal/empirical divide is also richly represented in social sciences. On the one side, in American social science especially, there are formalist fundamentalists who believe that life "really is" a mathematical model, and that empirical data are incidental at best to its representation. Only quantitative truth matters. On the other are empiricist fundamentalists who sneer at numbers, algorithms, or other sorts of formal models.[7] Star's and Woolgar's papers in this volume explore ways to refuse this great divide of formal versus empirical in computer science, psychology, and in social theory more generally.

As both a scientist and a citizen, I have a great stake in closing this divide. I do believe that the stakes are as high as the empiricists in computer science claim, although I don't think they are as centralized as many of them envision. Rather, I think the consequences of maintaining the formal/empirical divide are highly distributed, and reside as much in forms of bureaucracy, education, and exchange as in bombs and air traffic control systems.

The Status of Matter and the Absolute

I have on occasion taught Latour's *Science in Action* (1987) to scientists. The book's central tenet is that "nature" is nowhere to be found apart from the web of work and inquiry constituting the relations of science. To my initial surprise, the class discussions often became theological in nature. It seems my students who are in the sciences aren't afraid to use words like *God* or *soul*. However, among social scientists such discussions seem to be completely taboo. It's easier to talk about sex or excrement or almost anything than to talk about one's deepest spiritual or metaphysical beliefs. (I suppose in the United States at least it's because religious fundamentalists conjure up images of antiscience, antifeminist fanatics.)

There are a set of questions in STS of science that resemble metaphysical and theological questions throughout the ages. Beyond the questions of humans and nonhumans, and of formal and empirical knowledge, many of the deeper issues in the debate seem to reflect divergent opinions about the status of matter itself. The questions go something like this: A couple of years ago I gave a talk at UCLA about my work in collaborating with computer scientists in artificial intelligence. I talked among other things about the primacy of distributed (cross-personal, organizational, or community) cognition. Harry Collins was also there, and as "devil's advocate" asked me a question: "Agreeing that cognition is social, isn't there a limit to that? How do you explain, for example, that you could wake up in the morning speaking English, go alone into a locked room, and come out at night still speaking English? Doesn't that imply that there's some cognition only in the head?" At the time, I didn't have much of an answer for him, but in thinking through the human/nonhuman debate for this volume, I will venture a bit of translation.

First of all, when speaking of the brain in that fashion, one is implicitly speaking about matter, about physicality. Traditionally, people have had a difficult time modeling or speaking of "brain" and "thought" without invoking one of the original great divides, that between mind and matter. If we want to refuse that great divide, we must thus carefully examine how we think about matter itself. This is perhaps the microarchitecture of an ecological analysis. If we are to go beyond the current debates, STS researchers must confront this basic dichotomy and not allow in "a little bit of determinism" here and "a little bit of realism" there.

Think of matter as composed of arrangements of space and time—some very, very fast, as with light, some very, very slow, as with rocks. In an Einsteinian/quantum mechanics fashion, this matter has no absolute speed or rhythm. Rather, its rhythm and speed derive from its context. The rearranging of space-time configurations is a constant, never-stopping process, although some speeds are too slow for us to perceive as anything but stopped.

Another way in which this rearrangement works is as a relative location. Analytically, it is extremely useful to think of human beings as *locations* in space-time. We are relatively localized for many bodily functions and for some kinds of tasks we perform alone. But for many other kinds of tasks we are highly distributed—remembering, for example (Middleton and Edwards 1990). So much of our memory is in other people, libraries, and our homes. But we are used to rather carelessly localizing what we mean by a person as bounded by one's skin. Pragmatist philosopher Arthur Bentley cautions against the philosophical contradictions this brings about in his brilliant essay, "The Human Skin: Philosophy's Last Line of Defense" (1975 [1954]). The skin may be a boundary, but it also can be seen as a borderland, a living entity, and as part of the system of person-environment. Where the skin is, indeed, under some conditions becomes a very interesting question. But as an unthinking, linelike division between inside and outside, where "self" is on the inside, it makes no sense philosophically. Parts of our selves extend beyond the skin in every imaginable way, convenient as it is to bound ourselves that way in conversational shorthand. Our memories are in families and libraries as well as inside our skins; our perceptions are extended and fragmented by technologies of every sort.

All the matter in our body can be thought of this way, including the brain. In 1896, John Dewey wrote a critique of reflex psychology that still stands today (1981 [1896]). He noted that the common image of psychology was that a stimulus would happen, "go in" to the brain, stop there, be processed, and something would come back out. This was complete nonsense, said Dewey. It doesn't "go in" through nothingness . . .there is an event that changes the air, interacts with skin, with nerves. It is continuous, and there is never a time when it "stops." The arc is a convenient notation for a dualist, reductionist psychology, however, and makes certain things amenable to quantification.

I reiterate Dewey's critique with respect to cognition and the individual, and recommend it to researchers in STS. Learning English is a series of continuous events, of changes, rearrangements in the space-time of your body. Once the process gets going it keeps on going, given constant interactions with other people and all kinds of humans and nonhumans in the world. I don't know enough about death to know whether or in exactly what forms it might keep going afterwards, except that the ongoing actions we leave embedded in the world constitute one such action; for example, the books we write may be read after our deaths.

So the alone person in the room speaking English before and after is one case of a time of aloneness (a typically very short time, otherwise it would be solitary confinement, the ultimate penalty in most cultures). The ecological image for the aloneness of that learning is analogous to holding your breath—you still need oxygen, but you take your lungs "out of play," or put them on hold, for a moment. So the alone person is aside in the sense of not being together with others. Aloneness seen this way is not a vindication for mentalism or for the primacy of the individual; it's just a special case of relocating.[8]

There are historical and contemporary neurophysiologists who view the brain this way, as well as the brain-body-environment. The images have a wide range. Some have seen the brain and cognitive functioning as a "re-entrant, emergent process," where that which is sensed keeps circulating in the brain forever. Others find that we can't process sound without hearing what is before and after it—perception is entirely relative with respect to context. Once something is perceived, the action of perception continues indefinitely, changing and being changed by other events near it, sometimes resonating, sometimes clotting up or clumping up, sometimes fading into background noise.

To think this way we must vastly complexify the way we think and talk about matter. The brain is not a lump of meat with a few electric channels strung though it. The body/brain of any one person is a location of dense rearrangements, nested in like others. When we use the shorthand "individual" or "individual cognition," we are thus only pointing to a *density*.

Thus, in speaking of aggregates of peopled, material ecologies (including in them things, built environments, the natural world), we have a basis of resolution of the realism/relativism dichotomy and of the formal/empirical divide. An ecological analysis refuses to ground beliefs, including scientific beliefs, in something outside of this webbing location.

Moral Implications. What are the moral implications of this view? Scientists certainly don't hesitate to address such concerns in bringing up these issues, and neither should we in STS. Sociolo-