Second Edition

Philosophy, Rhetoric, and the End of Knowledge

A New Beginning for Science and Technology Studies

Steve Fuller • James H. Collier

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Acknowledgments

The first edition of this book was written while I was teaching in the Graduate Program in Science and Technology Studies at Virginia Tech, which remains the largest and most successful program of its kind in the United States and perhaps the world. It was in this context that I met Jim Collier, whose own interests inspired the pedagogical side of the first edition and whose presence is more explicitly felt in this one. He is also responsible for persuading Lawrence Erlbaum Associates to publish the new edition, for which I am very grateful.

The last ten years has seen a shift of geographical focus in my career from the United States to the United Kingdom. Britain has proved a very fruitful gateway for encountering the full spectrum of world opinion concerning the issues raised in these pages. I have applied, extended, and tested the arguments in this book in multiple settings, though most recently the Department of Management, Politics and Philosophy at the Copenhagen Business School has provided the impetus for making the editorial revisions needed for making it more useable in the classroom. Among those who over the last ten years have contributed most to a sympathetic yet critical appreciation of this work, I would like to thank Thomas Basbøll, José Antonio Lopez Cerezo, Christine Isager, Bill Keith, Joan Leach, Peter Plöger, Sujatha Raman, and Francis Remedios. Special thanks also for the unswerving moral support of my partner, Stephanie Lawler.

This book continues to be dedicated to William Lynch, formerly a student at Virginia Tech and now a tenured associate professor in the Interdisciplinary Studies Program at Wayne State University, Detroit. In my original dedication, I said that he was the sort of person whom I could imagine someday writing a book like this one. His first book, *Solomon's Child* (Stanford University Press, 2001), which I recommend to readers of these pages, serves to illustrate one of the fundamental theses of this book—that theorizing is a political practice. In particular, Lynch shows that the discourse of method in the Scientific Revolution in 17th-century England set normative boundaries around the community of inquirers, even though it did little to constrain day-to-day scientific practice. It is just this macro-structural character of scientific rhetoric—what used to be called its 'ideological' function—that deserves further critical attention from those inclined to sustained historical and sociological work.

—Steve Fuller, Coventry, UK

My deepest thanks go to Steve Fuller for beginning a conversation, during those heady days at Price House, which continues to fire my imagination. My great fortune is to work with Steve and those participating in the vast project of social epistemology.

To the students who participated in my rhetoric of science seminars at the University of Massachusetts and Virginia Tech, your voices echo in these pages. I want to thank my colleagues and friends in English and STS at Virginia Tech for their boundless support. Our privilege is to work with Lawrence Erlbaum Associates—we take great pride in having this book in your care. For their patience, concern, and clarity I thank Susan Barker, Art Lizza, and, especially, Sarah Wahlert. Linda Bathgate gives this project heart. I appreciate her encouragement and creativity and I am deeply grateful to have the chance now, and in the future, to work with her. Finally, to Monique. We begin.

—James Collier, Blacksburg, VA

Introduction 2003:
The More Things Remain the Same, the More They Change

For the past 15 years, *social epistemology* has been a project aimed at fostering closer cooperation between humanists and social scientists in the emerging interdisciplinary complex known as Science and Technology Studies (STS). STS has the potential of not only redrawing disciplinary boundaries within the academy, but ultimately, and more importantly, of making the academy more open to the rest of society. The trick is that STS practitioners employ methods that enable them to fathom both the "inner workings" and the "outer character" of science without having to be expert in the fields they study. The success of such a practice bodes well for extending science's sphere of accountability, presumably toward a greater democratization of the scientific decision-making process. These concerns are also shared by the assemblage of people who travel under the rubric of *rhetoric of science* and who teach oral and written skills in settings that range from general education to technical communication (Fuller 2001b). The success of *Philosophy, Rhetoric, and the End of Knowledge (PREK)*, then, should be measured in terms of its ability to persuade philosophers, theoretical humanists and social scientists, STS practitioners, and rhetoricians of science to see each other as engaged in a common enterprise.

By that yardstick, the first edition of *PREK* may be judged only a modest success, although it attracted considerable critical attention on publication in early 1993, including a symposium in the December 1995 issue of *Philosophy of the Social Sciences* and a major extended discussion in a volume devoted to interdisciplinarity published by the College Board, the firm that administers the entrance examinations most widely used in US universities (Newell 1998). Nevertheless, that much work remains to achieve the book's original promise is reflected in its new subtitle: *A New Beginning for Science and Technology Studies.*

Much has happened in the interim to shift the context of the book's argument. These issues are discussed before reviewing the book's contents. Despite the passage of time, the basic message remains the same: Theorizing is a politically significant practice. Recognized as political, one sees theorizing as having consequences beyond its intended audience. In this sense philosophy has, historically, transformed how nonphilosophers think and act in the world. Philosophers prefer not to acknowledge this aspect of their discipline because doing so would affirm the role of philosophy's classic opponent, rhetoric, as part of its own repertoire. However, STS has drawn renewed attention to this rhetorical dimension by focusing on the constitutive, or "constructed," character of reality. As a result, theorizing is understood as a kind of practice. Whether practitioners of STS—or the rhetoric of science, for that matter—have done the most they could with this insight is far from evident, as should become clear in what follows.

PREK's argument was originally advanced to counter what I call STS's "High Church" tendency to become a version of the thing it studies. "High Church" STS tends to be interested in the special epistemic status that science enjoys vis-à-vis other forms of knowledge. In coming to understand how science organizes itself internally and projects itself externally, STS began mimicking those very processes to acquire academic respectability and expert authority. In contrast, "Low Church" STS focuses more on the problems that science has caused and solved in modern society. From the Low Church standpoint, STS was preoccupied with proliferating jargon, establishing self-contained citation networks, and solidifying a canon. As yet another elite subject in the making, STS was losing sight of the most important reason for its pursuit—the patent contradiction that science is a universal form of knowledge, yet its production and distribution remains in the hands of an elite.

Both the High Church and Low Church sects of STS like to trace their origins to the 1960s. Whereas the High Church points to Thomas Kuhn's *The Structure of Scientific Revolutions* (1970) as the watershed STS text, the Low Church portrays STS as a response to the disturbing symbiosis that developed between scientific research and the military establishment during the Vietnam War (Fuller 2000b: chap 8). Moreover, the two sects interpret STS's "radicalism" rather differently. High Church radicalism heads toward "reflexivity." Reflexivity is an inward turn whereby STS practitioners apply to their own work the same principles that have enabled them to deconstruct the epistemic authority of the scientists they study. As a result, STS research is revealed to offer no overarching lessons about the nature of science, but rather specific points that vary significantly across contexts in which STS might be practiced. High Church radicalism tends to undercut Low Church radicalism—which is basically a version of the "emancipatory" politics associated with Western socialist parties. Here the STS practitioner invokes her own initially privileged "standpoint" on science. This position's emancipatory capacity is tested by the extent to which science can be made available to the entire citizenry (Harding 1986, 1991). Thus, science is put squarely in the service of humanity, perhaps even to the point of "downsizing" science so that more people can participate in its conduct and evaluation.

PREK was meant to provide a kind of High Church defense for the Low Church position. At the time, *PREK*'s rhetorical strategy seemed sensible. There was a general need to provide High Church defenses of Low Church perspectives. Much Low Church writing in STS is devoted to chronicling the insidious ways in which science, technology, and society intertwine. The authors of these books and articles tend to have excellent instincts about what matters if only because—unlike most people working in STS today—their livelihoods do not depend on agendas set by clients and funders. However, their work tends to be neither quite respectable in academic terms, nor quite recognizable as a public voice.

This state of affairs leaves open the task of *institutionalizing* a consistently critical stance toward taken-for-granted forms of epistemic authority. In their rather different ways, positivism and Marxism did just that in the 20th century, and social epistemology may do something like that in the coming century. The positivists and Marxists came to realize—not immediately to be sure—that the university provided the most hospitable site for institutionalized criticism, or "tenured radicalism" as some conservative American commentators, following the lead of Roger Kimball, like to put it. However, at the start of the 1990s, universities were still locked into a vision of knowledge production based on the inward-looking logic of disciplines. Consequently, *PREK*'s rhetoric was one of "opening up" the universities to extramural considerations that would serve to shake up ossified disciplinary structures. In this context, *PREK* offered STS insights into the conventional, and hence changeable, character of disciplinary boundaries.

However, in the last dozen years, much has happened to both the university and STS. If anything, universities are now *too open* to extramural forces, as disciplinary boundaries are now periodically rearranged by academic CEOs (a.k.a. "Presidents," "Rectors," and "Vice-Chancellors") and CKOs ("Chief Knowledge Officers") merely to reflect changing market conditions for the university's "knowledge products" (a.k.a. diplomas and patents). Seemingly, the university had previously come to identify itself so closely with its disciplinary structure that, once disciplines became moveable feasts, the university lost its sense of autonomy and direction. In short, the university simply became an awkward but biddable multi-purpose service provider with no ends other than to please the paymaster. This development has been especially pronounced in Europe, where science policy gurus now speak of "the new production of knowledge," in which universities are thrown into an open market that forces them to compete against such nonacademic entities as science parks, think tanks, corporate training centers, and online degree programs (Gibbons et al. 1994).

Much of the blame for the university's institutional implosion is traceable to the decline of guaranteed public expenditure. In the brave new world of neo-liberalism, universities must provide explicit justification for their continued existence. But let us recall that the welfare state was largely responsible for creating the expectation that universities could address every public policy need from improved health care to enhanced worker skills. In the United States especially, this expectation is a legacy of the crucial role played by academics in bringing World War II to a successful resolution through the atomic bomb project. For the next half century, physics research became the intellectual front line of national defense, and the other disciplines, as was possible, followed in line. An Americanized—some would say, vulgarized—version of logical positivism provided ideological cover for this movement. In this context Alvin Gouldner's (1970) resonant phrase, the "welfare-warfare state," acquired its meaning. However, the end of the cold war brought about a privatization of the welfarist justification for academic knowledge production. Instead of one long-term collective threat (i.e. nuclear annihilation) that justified higher taxes, the prevention of several relatively short-term individualized threats (i.e. diseases and ailments) now justifies higher insurance premiums. Accordingly, the locus of research funding has shifted from a virtual state monopoly on physics to the more dispersed corporate sponsorship of the biomedical sciences.

STS has proved adaptive to the new knowledge production regime. As the scientistic wing of postmodernism, STS has replaced Americanized positivism as the ideology of choice in many science policy circles. Policymakers increasingly renounce the old hierarchical "linear" models of scientists dictating knowledge use in favor of an image of "heterogeneous networks" of scientific and nonscientific interests. It would seem, then, that STS has become more rhetorically sensitized. If so, STS has managed this feat without paying much attention to the rhetoric of science as a field. This benign neglect has been largely reciprocated as rhetoricians contend with their own demons.

In particular, rhetoric is a field whose value is most naturally proven in the classroom through the transformation of people's attitudes and actions. However, the increasingly sharp separation of teaching and research in university culture means that, more than other academic practitioners, rhetoricians have had to live a schizoid existence. Rhetoricians teach students to speak and write more publicly while simultaneously trying to demonstrate that their own knowledge cannot be reduced to that of, say, a sociologist, historian, or literary critic. The very appeals to jargon and authority that rhetoricians routinely criticize in a student's rhetorical practice turn out to be their own weapons against interdisciplinary interlopers on the research frontier. Moreover, the situation is complicated by academia's shifting market environment. In effect, many denizens of rhetoric and communication departments today would prefer to be at the research frontier—of perhaps some other discipline—than in the classroom teaching rhetoric.

Alongside its sibling in the medieval trivium, philosophy (formerly dialectic), rhetoric best exemplifies the unity of teaching and research that remains the official ideal—and institutional hallmark—of the university today. In particular, research innovations are tested in classroom practice. As teachers demonstrate new twists on familiar argumentative themes, students acquire the means for expanding their prospects of effective self-expression and public intervention. Especially in the United States, students reciprocate through voluntary alumni contributions that enable the university to empower successive generations of students. In this context, education is about enabling the person to engage in the project of self and societal improvement, what Wilhelm von Humboldt reinvented at the dawn of the 19th century as *Bildung*. Education, in this instance, is *not* about learning how to identify reliable authorities to whom one "offloads" (or "delegates," to use Bruno Latour's euphemism) epistemic judgment and personal responsibility. Recall the risk element of Kant's original motto for the Enlightenment that so influenced Humboldt: *aude sapere*—'Dare to know!' A desirable consequence of education is that students are no longer uncritically dependent on the authority of the family, church, and perhaps even the state as a source of protection that elicits their "trust" (a notion that is given a rhetorical dressing down in chapter 8).

Proving the mettle of research through education goes back to the very foundations of university life in the Middle Ages. The Masters argued the position advanced here. The Doctors argued for a more specialized, deferential, and referential approach to the unification of teaching and research. The flavor of the original dispute is captured by the patron saints of the Masters—the ever skeptical William of Ockham—and the Doctors—the ever dogmatic Thomas Aquinas. However, from the mid-19th century onward, the balance of power in university has tilted from the Masters to the Doctors, as evidenced in the proliferation of academic specialties and the expansion of doctoral training, often as part of job certification. The overall effect has turned the universities into engines of expertise. Adapting a distinction drawn in democratic theory (e.g. Held 1987), we may speak of a transition from the *proletarianization* to the *plebiscitarianization* of knowledge production: from *prolescience* to *plebiscience*. Social epistemology aims to reverse this tendency, returning the legacy of the universities to the Masters. The distinction between prolescience and plebiscience has serious implications for science policy more generally.

Plebiscience argues that there should be only as much public involvement in knowledge production as will allow the process to flow smoothly. Normal science policy approximates plebiscientism in that the public normally ends up being involved in decisions about scientific research only when that research has potentially adverse consequences for a particular community. The scope of public involvement is restricted to the affected community. Otherwise the default public attitude is deference to established scientific authority. Social epistemology as practiced by most analytic philosophers tends to justify this practice (e.g. Kitcher 1993; Goldman 1999; cf. Fuller 1996).

Prolescience reverses the priorities. It argues that knowledge production should proceed only insofar as public involvement is possible. In a prolescientific state, research agendas and funding requests would have to be justified to a board of nonexperts, not simply to a panel of scientific peers. Although critics typically read it as a veiled form of anti-science, more instructive would be to regard prolescience as an implicit challenge to plebiscience's elitist assumptions. In economic terms, this elitism appears in plebiscience's strong distinction between the production (*by* experts) and the distribution (*to* nonexperts) of knowledge. This distinction is embodied by the mutation of representative democracy known as *corporatism.*

The corporatist reverses the democratic impulse by making the people beholden to their representative—in this case, an expert scientist. Corporatists suppose that as the world becomes a more complex place, people *ought* to lose interest in managing more of their lives—in fact, in all but the most locally effective aspects of their lives. The extent to which the corporatists have successfully cultivated this ethos may be seen in how "abstract" or "remote" people come to regard, say, the workings of foreign policy or scientific research vis-à-vis their own daily concerns. Yet the felt abstractness or remoteness of these activities, which the corporatist promotes as grounds for rule by experts, does not necessarily reflect any causal detachment from everyday life. After all the price and availability of consumer goods at home could easily be affected by either a breakdown in international relations or a scientific redefinition of product safety standards. What *is* detached from everyday life, however, is a rhetoric for talking about the causal connections. Hence, only a limited potential exists for a variety of constituencies to realize the stake they have in the conduct of affairs taking place outside their own neighborhoods.

Two possible diagnoses arise of the deficiency brought on by such rhetorical detachment—one aimed at the lay public and the other at experts. One diagnosis is inspired by Piaget's child development experiments: The public simply have no opportunity to make the sorts of decisions that would force them to appreciate the complexity of the human condition—and hence break out of the simplistic schema within which they normally make political judgments (Rosenberg 1988). The other diagnosis concludes that experts are not provided sufficient opportunity to account for themselves in ways that would force them to reduce the complexity of their own cognitive situation. STS offers this diagnosis. For example, scientists can modulate their speech and writing patterns depending on whether they need to justify themselves to an audience of like-minded researchers or to a committee of scientifically illiterate members of Congress. Probably little of scientific importance is lost in the translation since, if need be, the scientists can descend from abstract formulae to simple drawings to explain a point. The scientists may *prefer* to concentrate the expression of their knowledge claims in dense jargon rather than diffuse it through a cognitively permeable ensemble of words, pictures, artifacts, and ambience. But that guild privilege is one we can ill afford scientists to enjoy. Thus, every time an STS researcher unravels science's multiple rhetorics, she strikes a blow for prolescience by demonstrating that much of what would otherwise be considered "external" to science quickly becomes "internal" once scientists need to answer to a wider audience.

STS and the rhetoric of science have proceeded largely independently of each other, yet social epistemology retains close ties to both fields by stressing the overlap in their agendas. In this respect, *PREK* remains a vision unrealized but very much realizable. Before outlining the perspective that this book brings to a long overdue merger of interests, let us recount the most relevant developments of the last decade. A good way into this matter is by looking at what has happened to the two people who most influenced my own early thinking about STS, especially in terms of the links that the field might forge with rhetoric: Bruno Latour and Steve Woolgar (Latour & Woolgar 1986), both of whom remain formidable presences on the intellectual scene.

In 1993, two years after its appearance in French, Latour published his most widely read work in English, *We Have Never Been Modern* (Latour 1993). By Latour's own account, this work was his bid at becoming a "made for export" French intellectual. He succeeded. The book argues that "modernity" is the collective hallucination of self-styled "Enlightenment" thinkers who have, in practice, done little more than try to suppress those who disagreed with them. From this perspective, postmodernism mistakenly presupposes that modernity existed in the first place. Instead, Latour argues, we should be "amodern," which is to lose any sense of guilt or longing for the modernist hallucination. The book is written in the abstract yet provocative style of French intellectuals. Yet Latour invoked his authority as an empirical researcher—indeed, an "anthropologist"—to argue that science, supposedly the epitome of modernity, neither is nor ever has been modern. The argument's conclusion and style appealed to Francophiles in cultural studies and more mainstream American humanists like Richard Rorty and Clifford Geertz, who had independent grounds for believing Latour's claims. Latour's popularity peaked soon thereafter. He appeared to win a permanent appointment to Princeton's Institute for Advanced Studies. However, at the last minute, the appointment was vetoed by the Institute's physicists, who objected to Latour's disrespectful, perhaps even uninformed, treatment of their work. This event turned out to be a signature moment in the ongoing "Science Wars."

The Science Wars publicly pit professional scientists and STS scholars in ways they had never experienced in more strictly academic media. That the Science Wars exist at all testifies to both the rhetorical strength and weakness of STS. STS currently enjoys a public profile it did not have in 1993. All of this status is deserved and much of it earned. In challenging taken-for-granted conceptions of science with historical and sociological studies of scientific practice, STS has rightly led to a wholesale reconsideration of exactly why we value science so highly. Moreover, such a review could not have happened at a more opportune time since the end of the cold war has fostered wide-ranging inquiries into public expenditure, including spending on science.

Despite its sensitivity to the social contexts of scientific knowledge production, STS has difficulty applying this awareness reflexively. Consequently, the field's recent history is marked by a series of public relations disasters—notoriously the so-called Sokal Hoax of 1996 (Sokal & Bricmont 1998). The "hoax" was that a physicist managed to get a leading cultural studies journal to publish a politically correct but scientifically nonsensical article by leaning on the authority of various French *penseurs*, not least Latour, as well as their American emulators. In hindsight, the cleverest thing STS defenders could have done was to have stuck to their constructivist guns and deny Alan Sokal ultimate authority over the content of his text. After all, a corollary of the view that all knowledge is socially constructed is that the individual is no longer sovereign over her text. Indeed this idea is perhaps the clearest theoretical link between, on the one hand, poststructuralist thinkers like Michel Foucault and Jacques Derrida and, on the other, STS scholars like Latour and the authors of this text (Fuller 2000c).

Yet STS defenders recoiled in the face of ridicule. They backpedaled, denying any affiliation whatsoever with recent French thought—Latour excepted, of course. Philosophically speaking, this move amounted to a retreat to the microrealism of disciplinary expertise. Sophistic arguments followed to the effect that scientists practice science but do not necessarily understand what it is that they practice. In a widely watched debate between Latour and Sokal at the London School of Economics in July 1998, Latour drew two telling analogies of the relationship between STS and science. In the first instance, he likened the relationship to economics vis-à-vis business; in the second, he likened the relationship to a physician vis-à-vis a patient. Neither analogy was likely to persuade scientists that STS was not a threat to the legitimacy of their inquiries.

As the dust settles on this and other skirmishes in the Science Wars, STS supporters seem to have dropped Latourian arrogance in favor of a more timid retreat behind disciplinary boundaries. Here STS stakes its claim to epistemic authority on the all-purpose fudge word "discourse", whose meaning can be expanded (to cover all social practices) or contracted (to cover only words) as suits the speech situation (e.g. Guillory 2002).

I should stress that I do not share Sokal's doubts about Latour's STS competence. Rather, every time I see Latour shake his head in disbelief at the hostility generated by his statements, I question his *rhetorical* competence and, in particular, his obliviousness to the constitutive function of rhetoric. Apparently, Latour cannot assert STS's autonomy as a form of inquiry without implying the field's superiority to what it studies. This incapacity is rhetorically lethal in a democratic public forum. Not surprisingly, Latour's stock is highest in fields that are already inclined to have a demystified view of scientific knowledge, such as business studies and cultural studies. The transit between these two fields is more fluid than one might like to think.

This last comment brings us to an equally, although perhaps less obviously, lethal rhetorical tendency in STS. The tendency, one all too familiar from rhetoric's chequered past, involves rendering oneself as responsive and adaptive as possible. In this guise, STS is less an arrogant knowledge producer than a user-friendly service provider. Here we enter the world of the newly appointed Professor of Marketing at the Oxford Business School, Steve Woolgar, formerly ethnomethodologist *extraordinaire* of laboratory and computer life.

Since the first edition of *PREK*, Woolgar has not added appreciably to his body of written work. Instead he has become one of Britain's most successful academic entrepreneurs, recently bringing to fruition a £3.5 ($5.5) million research program on the "virtual society" for the UK's main social science funding agency. This program, concerned with cyberspace's transformation of civil society, involved researchers from a quarter of the UK's universities. Woolgar's skill at coordinating and publicizing this innovative initiative has led to his membership on policymaking boards in both London and Brussels devoted to the social integration of new information technologies. Wondrous to behold is the metamorphosis of a paper-shuffling academic into a master of the PowerPoint presentation, capable of distilling social constructivist insights into actionable bullet points.

On the surface, Woolgar "talks turkey" to the public and policymakers as advised in chapter 8 of *PREK*. But on closer inspection, all is not well. I had naively presumed that the rhetorically aspiring academic would start with a substantive position toward which she would try to draw her nonacademic audience. I had not envisaged that the academic would be simply satisfied with persuading the audience that she had done a good job and should do more of the same in the future. (New jobs welcomed!) Woolgar has become Britain's most engaging spokesperson for a cautious but thorough "informatization" of civil society—a curious fate for an intellectual radical. Aside from Luddites on the extreme right and left, no one could be offended by the words nowadays coming from Woolgar's lips. To be sure, governments always need respected academics to stimulate research in new areas of public policy. But I was surprised to find Woolgar so obliging—especially without having demanded that the government be moved from *its* initial position. Perhaps, then, Woolgar is less spinner than spun. At the very least, his fate underscores the need for the rhetorician—of science or otherwise—to conceptualize the "context of reception" in a way that prevents the standard of rhetorical success from dissolving into the reflected glow of a satisfied customer.

However, one would be mistaken to conclude that Woolgar somehow "betrayed" his earlier radical self. Rather, the social constructivist philosophy shared by classical rhetoric, STS, and social epistemology is, strictly speaking, open ended about its own normative implications. For example, both Latour and Woolgar have always been hostile to what Woolgar calls "positionism." Positionism is the tendency to epitomize a person's activity in terms of a finite set of fixed beliefs that are either true or false regardless of the person's action contexts. (I have been accused of doing just this: see Woolgar 1991b.) This constructivist tenet serves as an antidote to analytic philosophy's tendency to stereotype and otherwise misinterpret large swathes of discourse in a ham-fisted attempt to enforce logical rigor. However, the constructivist failure to recognize positions makes it difficult to determine when and whether someone—including the rhetor—has shifted from one belief to another. Thus, one fails to assume responsibility for a normative perspective, which is necessary for bringing closure to a situation that is open to many possible interpretations and follow-up actions.

A good example is the career of Woolgar's favorite trope, "configuring the user." This expression refers ironically to the customization of computer software to fit the needs of its likely users. In a famous study that launched him on the path to a chair in marketing, Woolgar (1991a) showed that, in fact, software engineers design what they can and then try to persuade potential users that this product is just what they need. Such a strategy has mixed results, of course, which makes for interesting reportage and a general recommendation that engineers incorporate potential users early in the software design process so as to increase the likelihood of user uptake. (Henceforth the careful rhetor will look at the fine print whenever matters of "participatory design" are invoked.) Good advice—except that Woolgar keeps open the explanation for any increased user uptake: Is it due to an objectively improved fit between product and need or the mere fact that users now have a personal stake in the software's success? Moreover, as a good social constructivist, Woolgar does not see much of a choice here. The very idea is that the users' well-defined "needs" were never demonstrated in the first place, although they served as a "noble lie" necessary to motivate the engineers' productivity.

Generally speaking, rhetoricians are familiar with the land of smoke and mirrors, although perhaps not with the region where double negatives always turn up positive. Woolgar has proved especially persuasive for two reasons that deserve note. First, he has pushed the social constructivist argument to the extreme, all the while being inoffensively disrespectful of disciplinary expertise. Latour tried to do the same, but his efforts only did half the job—resulting in the appearance of offensive disrespect toward scientists. Woolgar has an advantage over his French colleague—a mere philosopher by training—in having received a first-class honors degree in engineering from Cambridge. As a disciplinary apostate, Woolgar can target the constructivist argument in a way that will ring true even to those who might otherwise be hostile.

Second, Woolgar steers clear of any discussion of larger structural factors beyond the interaction among social agents that may shape the course of software design. While reflecting an especially purist microview of social constructivism, Woolgar's studied antistructuralism also obscures any awareness of the power relations that might obtain between engineers and users. After all, if users do not like the software, the most they can do is not purchase it, which may or may not adversely affect the engineers' livelihoods. In any case, the users are usually in no position to provide a viable alternative that would satisfy their own needs. That producers and consumers remain so strongly differentiated reinforces the sort of asymmetry on which power feeds.

Of course Woolgar's rhetorical advantage is served by not engaging in power-talk. Such talk invites charges of user *co-optation*, which, in turn, conjures up thoughts of guilt or blame on the part of the more successful software producers—a very unpopular idea in our neo-liberal times. Instead Woolgar appeals to a methodological principle called "analytic skepticism," which amounts to assuming, for research purposes, that all agents are equally powerful. This principle helps register the words and deeds by seemingly minor agents that might otherwise go undetected. But analytic skepticism does not notice the things that agents fail to do or say because they believe it would not meet with a favorable response.

I do not mean to suggest that Woolgar has somehow masterminded a conspiracy against software users. At most he provides ideological cover for common neo-liberal practices that economists often justify in more explicit terms. For example, one of Woolgar's cleverest arguments is a version of the invisible hand. On this argument, engineers generate flaws in software design not out of overt technical incompetence, but out of tacit social competence. These "flaws" really constitute a means of efficiently dividing the labor of software design between themselves and their potential users. (The main efficiency, of course, accrues to the engineers as the users do their share of the work unpaid or only after they have purchased a defective software package.) In effect, the users fill in the details of the overall design plan. Michael Perelman (1991) coined the term *metapublic goods* to capture the phenomena of Internet-based usergroups that form to discuss problems and solutions relating to software implementation. Often producer representatives lurk on these usergroups and incorporate the findings into the next generation of software. In part, Perelman drew attention to metapublic goods to show that, even in a highly privatized political economy, the idea of public good would always be reinvented as individuals realize that it is in their own long-term interest to freely share certain hard-won forms of knowledge. But when we consider who exactly is positioned to convert this freely shared knowledge into concrete benefits, are we likely to regard the relationship between producers and consumers as symbiotic or parasitic?

If the careers of Latour and Woolgar are morality tales for the aspiring rhetorician of science, what lessons should be drawn? The clearest one is not to become captured by the current rhetorical context by unreflectively continuing a practice that had proved effective in a previous context. In a sense, Latour and Woolgar do not sound much different from a dozen years ago. Now, however, their audiences and their exigencies are different, and unsurprisingly so too are the consequences of their discourse—and, perhaps, even what one thinks Latour and Woolgar had really been about for all these years. Moreover, their predicament is shared. The interdisciplinary fields surrounding cultural studies have suffered a similar fate. These fields were typically born of university expansion in the 1960s and 1970s when an influx of new constituencies and funding enabled the establishment of fields dedicated to questioning the assumptions of traditional disciplinary formations that remained unaffected by these larger societal changes. By the 1980s and 1990s, cultural studies continued to thrive on its anti-establishment line, but its import had changed. The field now appeared to be in the business of dismantling not only disciplinary boundaries, but the autonomy of academia altogether. Consumership had replaced citizenship, entrepreneurship had replaced emancipation, and so forth—as the university privatized its mission. Social epistemology stands against this tendency, whereby self-styled radical academics have unwittingly ceded control of the context of their knowledge production to the market.

My first formal exposure to rhetoric came from a major argumentation theorist who helped deepen the debate over the resolution of expert disagreement in a democratic forum, Charles Arthur Willard (1983, 1996). He and others reached back into classical rhetoric for *topoi* or argumentative frameworks that commonly arise in the legitimation of scientific claims (e.g., Prelli 1989; Gross 1990). At the same time, programs in technical writing evolved from their humble origins as required "composition" courses in English departments to the site of some of the most promising research on the reading and writing conventions in the academy and the liberal professions. Charles Bazerman (1988) has been perhaps the leading rhetorician of science from this background, and the co-author of this edition of *PREK*, James Collier, is an emerging leader (a good history is Russell 1991). Nevertheless, the growth of a field called "rhetoric of science" has not been completely welcomed within rhetoric itself. Some rhetoricians worry that the field is little more than a safe haven for interdisciplinary interlopers who then stretch the meaning of rhetoric beyond recognition (Gross & Keith 1996).

For the last 15 years, the most visible U.S. group to identify openly with the rhetoric of science is the Project on the Rhetoric of Inquiry

(POROI) at the University of Iowa, which also houses one of America's leading rhetoric departments. From POROI has come such landmarks as Donald McCloskey's *The Rhetoric of Economics* (1985) and the anthology *The Rhetoric of the Human Sciences* (Nelson, Megill, and McCloskey 1987). These works abundantly illustrate how distinguished humanists and social scientists use the resources of rhetoric to stem the tide of disciplinary fragmentation and the academy's growing irrelevance to public debate. In the United Kingdom, also dating from the mid-1980s, a group of sociologists and psychologists at the University of Durham have promoted similar themes in conferences, books, and especially the journal, *History of the Human Sciences*. (My own move to the United Kingdom, a year after *PREK*'s publication, was facilitated by this group.) Professional rhetoricians have also increasingly adopted the rhetoric of inquiry agenda, some even refashioning concepts developed in the first edition of *PREK* (e.g. Taylor 1996; Ceccarelli 2001).

The second edition of *PREK* constitutes both a significant abridgement and extension of the first edition. In terms of abridgement, this edition removes unnecessary digressions and gratuitous references. Moreover, much of the prose has been edited to make it more "readerly" (e.g. shorter sentences and clearer internal references). These editorial features are due to the new co-author, James Collier, who has had considerable experience using the first edition of *PREK* in writing-based rhetoric courses. (Through mutual agreement, Fuller and Collier have continued the first edition's practice of addressing the reader in the first person singular.) As that edition of *PREK* was going to press, Collier was writing his own technical writing textbook, *Scientific and Technical Communication: Theory, Practice, and Policy*, the prospective contents of which appeared in an appendix to *PREK*. Since that time, this book has been published (Collier 1997). It remains the only textbook that addresses—at the level of both theory and practice—the multiple registers in which scientific communication occurs today. Supplementing the original Collier-inspired appendix, Collier has supplied discussion questions for each of the chapters of this edition of *PREK*.

In hindsight, it should also be acknowledged that Collier was among the first to recognize the potential of STS to turn into a bag of rhetorical tricks available to the highest bidder. In my STS seminars at Virginia Tech in the early 1990s, Collier was steadfast in calling for a "humanist" alternative to the rather value-neutral and perhaps even amoral attitude that STS seemed to have toward science. While received at the time as ardent and perhaps even old-fashioned, Collier's call, as the fates of Latour and Woolgar clearly suggest, is increasingly pertinent as STS becomes more central to the construction of our world. Here the social epistemologist continues to tread on what remains a "no man's land" of interdisciplinary interaction.

The social epistemologist needs, of course, to establish credibility with both academics and policymakers. This problem is quintessentially one of rhetoric, especially of cultivating *ethos*. (Not surprisingly, the journal *Social Epistemology* is now edited by Joan Leach at the Graduate Program in Rhetoric of Science at the University of Pittsburgh.) Specifically, the social epistemologist must overcome the classical stereotypes of *both* the philosopher (as Platonist) and the rhetorician (as Sophist). The stereotypical philosopher invokes norms as an excuse for distancing herself from the people, who (so says the philosopher) willfully fail to meet her lofty standards. The stereotypical rhetorician abandons norms for gimmicks that can secure short-term success for her client (often in willful disregard of more long term and less tangible benefits). The social epistemologist's way out of these stereotypes is to realize that the normative is constitutively rhetorical. To wit, *no prescription can have force if the people for whom it is intended have neither the ability nor the desire to follow it.* This point implies two *principles of epistemic justice* (à la Rawls 1972) that I propose as procedural constraints on normative transactions. I call these *humility* and *reusability* (which are discussed in more detail in chapter 8).

The turn to political philosophy here is quite deliberate. Philosophy's public service is to promote Enlightenment. This idea first reached self-consciousness in the 18th century, when the efficiency of the capitalist mode of production freed up enough people's time from the material necessities of life that a relatively widespread discussion of societal ends could be conducted on a sustained basis: Where are we going? Should we be heading there? If so, how should we get there? Who should be doing what in the meanwhile? Many 20th century theorists have questioned the Enlightenment's emphasis on managed talk over directed action. Nevertheless, the project has been unique in examining tradition for the sake of transforming it, rather than simply continuing it (Wuthnow 1989: pt. 2). The most inspiring case in point is the U.S. Constitution, my best example of rhetorically effective theorizing in the Enlightenment spirit, whose full realization requires the participation of all the members of a society.

The U.S. Constitution is sometimes described as the only successful instance of "philosophically designed order," in marked contrast to the failed instances that make up the entire history of totalitarian politics. (Has there ever been a form of totalitarianism that was *not* philosophically inspired?) However, the highlighted turn of phrase misleadingly leaves the impression that the U.S. Constitution involved the "application" or "implementation" of a particular philosophical theory. In fact, I would claim that the U.S. Constitution is itself an example of philosophical theorizing fully actualized (or "rendered self-conscious," as Hegel might say). For the U.S. Constitution does exactly what every philosophical theory—especially the ones that have gone by the name of "metaphysics"—has aspired to do. It provides a procedural language for articulating a variety of distinct perspectives on the world. The worth of such a theory is measured by the transformations of perspective that it enables: Are the perspectives simply given the opportunity to pursue what they have already identified as their own interests, or are they constrained to take into account the interests of others in such a way that they reach positions better able to address the standing hopes and fears of the day?

To put the point about the U.S. Constitution as an insight of social epistemology, philosophical theories are diffuse social movements. If law and politics actualize philosophical theories, then metaphysics and epistemology, respectively, as commonly understood (i.e. as the study of reality and its modes of access) are what result when such theorizing *fails* to be actualized. The power of the great philosophical theories of the 20th century—Marxism, pragmatism, logical positivism, existentialism, and structuralism—lay not in the truth of their specific doctrines. Their power resided in the ability of their procedural languages—what is often disparagingly called their "jargons"—to get people from quite different walks of life to engage in projects of mutual interest. Such collaboration was made possible by the several registers in which each of these languages could be articulated. Thus, to restrict logical positivism to a handful of Euro-American academic philosophers adept in formal logic and conversant with cutting-edge scientific research would be to ignore logical positivism's more lasting significance as a social movement. Here we need to look to the constituencies for works like A. J. Ayer's *Language, Truth, and Logic*, I. A. Richards' *Practical Criticism*, Count Korzybski's *Science and Sanity*, S. I. Hayakawa's *Language, Thought, and Action*, Stuart Chase's *The Tyranny of Words*, and even Samuel Beckett's *Watt*. Each of these works extended representation in the "Positivist Constitution" to such Low Church outposts as psychiatry, political science, education, communication studies, literary criticism, and—dare I say—the general reading public.

In our own time, Jürgen Habermas has singularly excelled at theorizing in a way that not only draws into his discourse variously interested intellectuals but also intervenes in the public affairs of his native Germany and, increasingly, the European Union. He is now the leading philosophical advocate of a European Constitution, modeled partly on the U.S. Constitution. But as Habermas (1987) has rightly observed, the biggest threat to rhetorically effective theorizing in the late 20th century has been *postmodernism.* The threat comes from the refusal to believe in the possibility of the sort of constitution that I have been describing (e.g. Lyotard 1983)—a form of talk that sublimates without entirely eliminating the deep divergences that exist in contemporary society. More so than Habermas, social epistemology accepts the facts that inspire postmodernists but not their skeptical normative conclusions.

In response to the skeptical postmodernist, I would ask whether a constitution really requires a meeting of minds or simply a confluence of behaviors. Following a convergence of opinion within both analytic and continental philosophy (e.g. Quine 1960; Derrida 1976), I believe that only a philosophical conceit backed by a dubious mental ontology makes agreement on meanings, values, and beliefs a necessary condition for coordinated action. Instead, parties simply need to realize that they must serve the interests of others as a means of serving their own. That is, their diverse perspectives are causally entangled in a common fate. Much thinking about public policy reifies zero-sum gamesmanship. This belief illicitly presumes that opposing interests require opposing courses of action that eventuate in one side's succeeding at the other's expense. Such thinking is compelling only if one imagines that parties are fixed in their positions—a situation that will obtain only if the parties do not communicate with one another. But communication does not necessarily breed consensus. Rather, communication may cause all concerned to change their positions such that their still quite different goals can be pursued in harmony and perhaps even to the benefit of others who are not directly involved. In any case, in the long term, both sides to a dispute will either win or lose together.

The traditional strategy for instilling this sense of mutually implicated inquiry has been to engage in a *rhetoric of truth*. In this instance, inquirers are led to believe (usually with the help of a philosophical theory) that they are *already* heading in a common direction, fixated on a common end, and that all subsequent discussion should be devoted to finding the most efficient means toward that end. The historical persuasiveness of this strategy is revealed in the traditional definition of the subject matter of epistemology and the philosophy of science as "methodology" (a search for means) rather than as "axiology" (a search for ends). In contrast, the *rhetoric of interpenetrability* is my attempt to develop a rhetoric that does not, in the name of "truth," preempt the articulation of significant disagreements over the ends of inquiry. I deploy this rhetoric in four cases of interdisciplinary renegotiation in which I have participated. Much of this book reports on my practice as a theorist moving within the academy, as well as between the academy and the rest of society.

Part I of this book lays out the basic position of the book: The field of Science and Technology Studies has the potential to be an emancipatory practice given its dual mission of dissolving disciplinary boundaries and democratizing knowledge production. However, a properly renovated sense of philosophy and rhetoric is needed for the normative project of STS to get off the ground. After locating the roots of STS 19th century concerns about the proliferation of rival epistemic authorities, Chapter 1 outlines the major contemporary STS orientations and discusses why normative questions have been generally given the silent treatment. An account of rhetoric is then given that is designed to empower the STS practitioner with an empirically responsive normative sensibility. The account is based on the idea that norms are prescribed to compensate for already existing tendencies to reach some mutually desirable goal. Finally, the standpoint and scope of my brand of social epistemology is introduced. My position involves a "shallow" conception of science. The authoritative character of science is located not in an esoteric set of skills or a special understanding of reality, but in the appeals to its form of knowledge that *others* feel they must make to legitimate their own activities. In this way, rhetoric goes to the very heart of science.

Chapter 2 argues that the desultory character of most interdisciplinary research and the lack of cross-disciplinary epistemic standards are really two sides of the same problem. The scent of banality accompanying calls to interdisciplinary scholarship arises from a failure to take to heart the (merely) conventional character of the differences separating academic disciplines, as well as between the academy and society at large. This point is repeatedly driven home by STS research. It implies that interdisciplinary exchanges have the potential to significantly transform the work that disciplines do, especially by constructing new epistemic standards to which several disciplines agree to hold themselves accountable. However, a "knowledge policy" initiative of this sort requires a special rhetoric called *interpenetrative*. I present several pressure points for interpenetration in the academy. At the same time, I distance this rhetoric from both a blandly tolerant humanism and a maniacally technocratic enthusiasm.

Part II characterizes four cases of interdisciplinary interpenetration in which I have participated, mainly with regard to rhetorical strategies available and used, as well as their socio-epistemic implications. Common to the four types—incorporation, reflexion, sublimation, excavation—is the suggestion that many, if not most, of the "philosophically deep" problems generated by the sciences are the function of unreflective, often downright bad, communication habits. Entrenchment thus is mistaken for profundity. But this finding does not mean that these problems can be easily resolved. Still, Part II provides a fairly comprehensive sense of the state of play in the epistemological debates that currently dominate the social and cognitive sciences, as well as much of the humanities. The status of STS as a player in this game is the subject of Chapter 6.

Part III is meant to show that the problems generated by the sciences have a deep political and economic character that cannot be dealt with apart from all the other issues involved in governing a polity. Self-image and aspirations aside, science is *not* autonomous in practice.

Chapter 7 elaborates the sensibility that social epistemology brings to knowledge policy—namely, that the knowledge system may have problems even if nobody is complaining. Indeed the institutional inertia that governs most science policy is the biggest problem. After showing how both independent and advocacy journalism obscure this problem, I suggest strategies for constructing normative considerations in a policy setting. Finally, I consider objections that "knowledge policy" would have to be Machiavellian to succeed.

Chapter 8 moves from the systemic to the political, suggesting a continuity between philosophy's classic normative mission and "knowledge politics." Basically, philosophers are in the business of questioning standards and achievements that are normally found exemplary. But practically speaking, the union of Big Science and Big Democracy currently provides no public forum for conducting the politics of knowledge. This problem raises the distinct possibility that "science" and "democracy" have outgrown each other. None of the old normative models has much purchase on the sorts of activities that pass under those names today. Inspired by work in economic sociology and mass media law, I propose a principle of *epistemic fungibility* to cut Big Science down to a democratically manageable size. The chapter concludes by considering the rhetorical indifference with which academics conduct their professional lives, which prevents them from both appreciating the political character of their own work and preventing policymakers from using that work in the most appropriate manner. This chapter contains an appendix that discusses the negotiating style of the social epistemologist as interdisciplinary mediator.

Part IV tackles the two main foes of the knowledge policymaker: The *relativist* (in many guises) and the *antitheorist* (in the person of literary critic Stanley Fish). These two foes are weakest where they advertise themselves as strongest: the relativist operates with an obsolete conception of society, while the antitheorist has a rather unrhetorical, positivistic conception of theory. Offered in place of these inadequacies are, respectively, some conceptions of society compatible with social epistemology and a conception of *presumption* as a legal or scientific norm (an embedded theory, if you will) that counteracts a community's acknowledged worst tendencies. The book ends with a utopian postscript that conveys the difference that the position conveyed in this book would make to the way we think about knowledge in the world. An appendix provides some templates for various pedagogical contexts in which *PREK* might figure as a textbook.

Finally, to readers initially skeptical of this enterprise, please keep in mind that if the pursuit of knowledge policy or the satisfaction of normative impulses seems inherently authoritarian, that is only because not enough people are doing it. In *The Open Society and Its Enemies* (1950), Karl Popper first complained about the "transcendental" viewpoint of Marxists and Freudians who thought it better to meet an objection with a meta-level diagnosis of the objector's (faulty) state of mind than with a straightforward counterargument. In a sense, postmodernists who are reluctant to engage in the normative enterprise that follows in these pages have drawn a perverse lesson from Popper's complaint. After all, a theoretical language is not born transcendental, but it can be unwittingly elevated to that status if the audience feels that the theory must be either accepted whole cloth or rejected in its entirety. True believers do the former, postmodernists the latter. Either way, transcendence is rhetorically accomplished and the open society remains an unrealized possibility.

PART I

THE PLAYERS AND THE POSITION

1

The Players: STS, Rhetoric, and Social Epistemology

HPS AS THE PREHISTORY OF STS

Most 19th-century theorists of science are classed today as "philosophers," although virtually all had scientific training and a historical orientation. British theorists were concerned with the popular reception of science and the role of scientific reasoning in democratic decision-making processes. French theorists were concerned with science and technology as extensions of the state and instruments of social progress. German theorists were preoccupied with the division of academic labor in the emerging structure of the research university (Ben-David 1984: Chaps. 5-7). Ultimately, however, the cognitive exigencies of the modern world dictated the uses to which these theorists would put science and its history. For the most part, the uses have been highly "rhetorical." These theorists sought ways to express scientific claims that would move appropriately educated audiences to support emergent scientific institutions for cognitive authority over their competitors—religion, craft guilds, folk wisdom, and explicitly pseudo- and antiscientific movements.

The task was neither easy nor evenhanded. Science's strongest suit was its claim to derive knowledge by experimental observation. Still the preferred rhetorical strategy—the enumeration of "demarcation criteria" that science could alone meet—effectively inclined the public not to scrutinize, but rather to trust the scientists' observational powers based on verbal accounts that enabled them to "virtually witness" what scientists had done (Ezrahi 1990: Chap. 3).

The first self-proclaimed "positivist," Auguste Comte, initiated the task of demarcating science from nonscience. Comte sought to identify theories worthy of further pursuit without having to precommit significant intellectual and material resources. To ensure the economic viability of this presorting process, philosophers tried to read epistemic merit off the surface features of theories that one might find in a student's textbook. This process supposed that a theory's verbal and mathematical presentation would indicate its likelihood in pushing back the frontiers of knowledge. By the time logical positivism caught the philosophical imagination in the 1930s, accepted thought was that scientific theories should wear their logical structures and operational definitions on their rhetorical sleeves.

The history of science was used in an equally rhetorical fashion, unashamedly Whiggish by current standards. By 1840, the Cambridge geologist and cleric William Whewell both had coined the term scientist and opened the field called "History and Philosophy of Science" (HPS). HPS explicitly sought what was best to believe about the past to construct a desirable future. This project entailed a twofold strategy of: (1) eliciting principles of epistemic growth that could be transferred across disciplines—and, potentially, made the possession of all inquirers; and (2) favoring the study of certain revolutionary periods in which the process of major epistemic change was evident.

In the case of (1), keep in mind that 19th-century physics was regarded as a discipline that had largely run its course and whose methodological vitality was thus better placed in the more exciting developing proto-sciences of life, mind, and society. This assumption explains, in the case of (2), the bias toward focusing on great showdowns over theory choice and agenda setting at the expense of studying the workaday methods of the most advanced sciences of the day.

The sense of "normative" that I pursue under the rubric of *social epistemology* returns to the 19th-century idea of philosophers intervening to improve the course of knowledge production. Nineteenth-century philosophical interventions ran the gamut of prescriptive activities. Whewell advised Faraday and Darwin on the conception and interpretation of their theories. Comte and John Stuart Mill laid down the steps by which the fledgling social and psychological disciplines might become truly scientific. Ernst Mach used the history of physics as a critical wedge in contemporary debates by recovering dissents to which the Newtonian paradigm had failed to respond adequately. (Einstein later credited Mach's critical appeal to history with having prodded his own thinking in a relativistic direction. Paul Feyerabend also made himself the master of this form of history.) Pierre Duhem normalized science's relations with a traditional cultural authority like the Roman Catholic Church by stressing the partial continuity between science and religion (e.g. in the medieval origins of modern physical concepts) despite the ultimately different ends of their inquiries (instrumental success vs. explanatory truth). For his part, John Herschel normalized science's relations with the emerging reading public of Victorian Britain by portraying scientific reasoning as an extension and formalization of common sense. John Dewey's influence in schools of education enabled him to play largely the same role in early 20th-century America.

In retrospect, the most distinctive normative contribution of these theorists of science was by isolating a lingua franca, a procedural language that would enable the sciences to develop toward greater methodological unity and, hence, greater public accountability. Positivism is still the term normally used to capture this project in both its 19th- and 20th-century forms (Fuller 2001a). The project of social epistemology is sympathetic with positivism's instinctive question: *How do we cope with an increasingly diversified social and cognitive order?*

However, the possibilities that we pursue in response to this question are mediated by recent developments in STS, which have veered considerably off the course of philosophical positivism. But before suggesting where positivism went wrong, it is important to point out that not all recent philosophers of science have relinquished the robust normative perspective of the previous century's theorists. In this regard, Karl Popper and Paul Feyerabend are precursors of social epistemology.

Popper and Feyerabend intervened in the shadows of policy forums where research is initially stimulated and ultimately evaluated. They stressed that science needs to be evaluated in terms more of consequences than of conception. Also in their writings is the theme that, given the increasing access to resources that science commands, research has become—if it hadn't been already—both an investment opportunity and a public trust. Research needs to be acted on as such. To put the point in the signature Popperian way, science must be supported as an "open society" that will serve as a model for all of society. Social epistemology embraces the spirit of this enterprise (Fuller 2003a).

The progressive 19th-century mind supposed that if science gave us the most comprehensive grasp of the world, then the most comprehensive grasp of science could be gotten by studying science scientifically. However, the political economist Vilfredo Pareto gave this line of reasoning a particular spin. His idea was not so much to study the actual practice of science by scientific means (as STS would eventually do), but rather to treat scientific practice as if it were like the world represented by our best scientific theories. Pareto saw scientific practice as an idealized mechanics closed under a system of rational principles operating on the inputs of nature, but frequently subject to extraneous influences. Thus was canonized the "internal–external" distinction in the historiography of science (Fuller 2000b: Chap. 7). Internalists tried to deliver on Hume's promise to provide a "mental mechanics" that paralleled Newton's physical mechanics. More generally, *science was taken to have the qualities of the things that science studies*. This "homeopathic" theorizing bears an uncanny resemblance to the idea of the individual as the microcosm of nature or of the species. The resemblance was further strengthened in 20th-century philosophy of science. Not only was science seen as reproducing the structure of the world it represented, but as potentially transpiring in the mind of a single individual—namely, the philosopher of science. Comte anticipated this microinternalism by justifying his hierarchy of the sciences in terms of its enabling him to reenact the history of science in his mind. In our own time, this "rational reconstructionist" position has been represented by a host of positivist (Reichenbach), Popperian (Lakatos), and historicist (Shapere) philosophers of science.

In 1962, Thomas Kuhn unwittingly began to undo HPS. His major breakthrough, in *The Structure of Scientific Revolutions*, was to account for the history of science as internally driven without concluding that it was being driven anywhere in particular. A veteran instructor in the Harvard general education program, Kuhn reminded his readers that the memorableness of the sequence of episodes in internalist histories of science, canonized in science textbooks, served as the vehicles by which the "normal science" of a paradigm is transmitted. However, the specific episodes in the sequence varied from paradigm to paradigm, thereby relativizing any conclusions about "progress" and the "ends of knowledge" that internalists might want to draw from the ordering.

Kuhn's blow to philosophers of science is hard to exaggerate (Fuller 2000b). Some (mistakenly, I believe) have even taken his book to mark the revenge of the humanists against the positivists. Given Kuhn's sequence of paradigm–anomalies–crisis–revolution–new paradigm, cyclical history would seem to have finally made a major inroad into the last bastion of linear progress, science. Although few philosophers officially conceded any ground to Kuhn, increasingly fewer defend scientific progress in substantive terms (i.e. terms scientists themselves would recognize). Rival conceptions of "verisimilitude" and "increased empirical adequacy" are contested on such purely formal grounds. Even if agreement were reached on one of these notions, philosophers would still be in no position to evaluate, let alone improve on, the degree of progress enjoyed by current research programs. This debate has a scholastic cast as philosophers retreat from explicit historical appeals to quasi-transcendental arguments about the "nature" of science: How would science be possible at all without a certain conception of progress? Questions of this sort were wisely passed over by Kuhn in silence.

With increasing internalization, HPS has developed a more restricted normative sensibility. HPS currently seems to be conducted more in the spirit of a schoolmaster giving marks than a policymaker trying to improve the conduct of inquiry. Philosophers of science know that it was good to choose Copernicus over Ptolemy by Galileo's day, and that it would have been better to have made the choice sooner. But they have precious little to say about what line of research we ought to pursue *now*. One wonders what HPS practitioners would say if they realized just how close their current research places them to literary criticism and art connoisseurship—two disciplines whose practices have become increasingly alienated from their putative objects of evaluation. Contrary to 19th-century hopes, critics' judgments typically do not feedback into the creation of better art or even better publics for the reception of art. What is produced, instead, is a self-sustaining body of scholarly literature. Any positive impact of critics on the course of art in this century has been fortuitous, much like the impact of philosophy on science's current course.

THE TURN TO SOCIOLOGY AND STS

Legend has it that Kuhn's overall impact on the academy has been more liberating than inhibiting. While Kuhn betrayed little knowledge of sociology in *Structure*, his own example suggested to sociologists (especially Barnes 1982) the possibility of explaining most of what was interesting about science without having to make reference to such philosophical categories as "truth," "objectivity," "rationality," or even "method." These categories had traditionally led sociologists to enforce a double standard in the way they studied science vis-à-vis the way they studied other social practices. Indeed this double standard is operative in the work of the founder of the sociology of knowledge, Karl Mannheim (especially 1936) and his distinguished American successors Robert Merton (especially 1973) and Joseph Ben-David (1984). To varying degrees, these early sociologists unwittingly diminished the public accountability of science—if not contributed to its outright mystification—by refusing to scrutinize science by its own principles. Not studying science scientifically meant that sociologists typically drew conclusions about science based on the authoritative testimony of the great philosophers and scientists, or anecdotal evidence from great episodes in the history of science. Since such prescientific sources of knowledge would not have been tolerated in the study of other social phenomena, why should methodological standards be lowered for what is supposedly society's premier cognitive institution?

Inspired by Kuhn's work, the first school in STS was founded in the 1970s. The Strong Programme in the Sociology of Scientific Knowledge (Barnes 1974; Bloor 1976), or the "Edinburgh School," rejected the double standard in the sociological study of science by laying down what I dub:

> *The Fundamental Mandate of STS*. Science should be studied as one would study any other social phenomenon, which is to say scientifically (and not by relying uncritically on authoritative testimony, anecdotal evidence, and the like).

Surprisingly, few of the most prominent STSers are actually trained as sociologists (Fuller 2000b: Chap. 7). Nevertheless, they can all be broadly identified as "sociological" in the sense of denying an "internal" history of science distinguished in its categories and methods from the history of the rest of society. Despite the mix of methods that these researchers have used to study science, analogies from, allusions to, and even actual instances of ethnographic practice enjoy epistemic privilege in the field. This bias enables STS researchers to "observe on site" the divergence between the words and deeds of scientists. These findings, absent an explicit normative stance, have resulted in the much ballyhooed "relativism" of STS research. (Fuller 1992a, and Traweek 1992 offer alternative views of the strengths and weaknesses of this tendency.)

The target in this sociological dressing down of science is not the scientists. Generally, scientists are portrayed in STS research as modest toilers who make the most of difficult situations in which expectations are high but resources are often embarrassingly low. Rather, the real foes are philosophers and those inclined to act on their prescriptions. Positivist philosophers have fostered these unwarranted expectations by making it seem as though science works by a "method" that manifests a "rationality" quite unlike anything else that society could offer. This sentiment continues to be found in popular accounts of science, which speak the language of hypothesis generation, theory testing, and falsifiability—words that sound right *only* if one is speaking of science. In that regard, the demarcationist rhetoric practiced by Comte and his successors proved all too effective. For when one actually steps into the labs and the other workplaces where science is done, a variety of quite ordinary, often inconsistent, activities that could be said to fall under these fine rubrics are observed. Thus, we arrive at the normative crossroads facing STS: *How should STS conduct itself in light of what it learns about science?*

This question may be subject to various elaborations. For example, should STS advise the public to abandon its faith in science? Should STS scrutinize science more but expect less (or vice versa)? Or rather should STS let the scientists go about their business and simply put an end to all this mystifying talk about "method" and "rationality"? Moreover, STS must decide whether its own practices should be changed. This concern is the *reflexive dimension* of the normative question, which in the history of philosophy has been most strongly associated with the Hegelian tradition. In the case of STS, we might wonder: If science is, indeed, the product of sociohistorical contingency, how is it that only now (and here) do we come to learn this, and how should this knowledge be allowed to affect our subsequent practice? The answers to this important question have been far from uniform. Some argue for minimal effect, an epistemic "business-as-usual" attitude, whereby the STSer pursues her inquiries alongside those of the sciences they study (e.g. Collins 1985 and the more orthodox ethnographers). Others suggest that STS should purge this newfound contingency from its own practice and become more scientific than the scientists themselves (e.g. probably the original intent of the Strong Programme). Still others argue that STS should incorporate contingency into the content of its own findings so as to lend a more partisan and political flavor to its research (e.g. roughly speaking, my own and other critical-theoretic approaches). Finally, still others recommend that STS adopt a self-deconstructive style of writing that reveals the contingent character of distinguishing "factual" from "fictional" accounts of science (e.g. Woolgar 1988a and the radical ethnographers).

The Achilles heel of STS has been a reluctance to *argue* about the relative merits of these reflexive postures. Instead STS researchers have tended to resolve these matters silently in their practice (although Pickering

1992 makes a promising start at engagement). The problem, of course, is that *silence* leaves perilously open the question of *what is the point of closely studying the knowledge system* (Fuller 1988a: Chap. 6)? Social epistemology offers a forum for such normative considerations in to bring STS closer to both the most abstract and the most concrete of students of the knowledge enterprise—epistemologists, science policy analysts, and critical social theorists.

Perhaps the best way to begin to identify the desired forum is by distinguishing two general attitudes toward science that can be found among STS practitioners. The first attitude, *Deep Science*, is that current training ensures that scientists know what they are doing and should continue doing without the misguided commentary of philosophers and other outside scrutinizers. The second attitude, *Shallow Science*, is that STS practitioners take their own success in penetrating the inner workings of science to imply that nonspecialists should have more of a say about which science is done and how.

We can think of the two attitudes as providing alternative answers to the question: *Where does one find knowledge in society?* The Deep Science inquirer locates knowledge in the skills that scientists display in their workplaces, which are taken to be intimately connected with the things they produce and which are then "applied," for better or worse, throughout society. This approach is similar to the way we ordinarily think about science. However, the Shallow Science inquirer makes no such distinction between knowledge and its applications. Knowledge is seen as distributed across the network of authority and credibility with which a particular piece of scientific work—especially a text—is associated. Thus, whereas the Deep Scientist (i.e. the scientist studied by the Deep Science inquirer) has knowledge in virtue of her unique powers of mind and body, the Shallow Scientist has knowledge in virtue of others' letting her exercise discretion. As will become increasingly clear, my brand of social epistemology is linked to Shallow Science.

Deep Science is a largely nonverbal craft, or "tacit knowledge," that requires acculturation into long-standing disciplinary traditions and is best studied by a detailed phenomenology of scientific practice. Opposed to this image is that of Shallow Science, a largely verbal craft that consists of the ability to negotiate the science–society boundary to one's own advantage in a variety of settings. Shallow Science is studied by deconstructing the seamless rhetoric of scientists to reveal the clutter of activities—the positivist's "context of discovery"—that such rhetoric masks. Typical students of Deep Science include historians of experiment who follow Michael Polanyi (1957, 1969) in devaluing the role of theorizing—and the use of language, more generally—in everyday scientific practice (e.g. Gooding et al. 1989). Students of Shallow Science include most social constructivists, discourse analysts, and actor-network theorists.

Despite my sympathies with the Shallow Science camp, to whom I assign the generic label *constructivist* throughout this book, I depart from many of its members in believing that a robustly normative approach to science is compatible with, and facilitated by, the Shallow Science perspective. For, in their own inimitable attempts to isolate one all-purpose methodological trick, philosophers of science originated the Shallow Science perspective to enable nonscientists such as themselves to hold science accountable for its activities. In this way, the classical philosophical focus on the context of *justification* has metamorphosed into a sociological interest in science's mode of *legitimation.* By contrast, students of Deep Science tend to be purely descriptive in their aspirations, tacitly presuming that science works well as long as the scientists do not complain (cf. Fuller 1992a). Is it any surprise, then, that Deep Science tends to be concentrated in labs, whereas Shallow Science is spread diffusely across society?

The Deep and Shallow images define polar attitudes toward the cognitive powers of the individual scientist. At the Deep end is the idea that scientists are especially well suited, by virtue of their training, to represent the nature of reality. The practices of scientists, however disparate their origins, have fused into a "form of life" with its own natural integrity. At the Shallow end is the idea that scientists are no better suited than laypeople to represent reality. This idea is rarely appreciated not only because scientists share with laypeople basic limitations in their ability to scrutinize their own practices, but because the epistemic cost of admitting the fallibility of scientific judgment is especially dear: How would engineering be possible if the judgments of physicists were not well grounded? Yet it is precisely this easy relation between science and technology that the Shallow Science perspective has endeavored to challenge (e.g. Bijker et al. 1987).

The basic problem with Deep Science is that its conception of the social is unbecoming to anyone who wishes to hold science accountable to someone other than the scientists themselves. Deep Science provincializes society into jurisdictions of "local knowledge," the authority of which is meant to be taken on trust regardless of the potential consequences for those outside a given jurisdiction. On this basis, most partisans of Deep Science claim to be relativists. Indeed, generally speaking, being a relativist is easy if you presume that your utterances affect only intended audiences in your community. However, if you believe that language enhances, diminishes, or reverses existing social orders when appropriated outside the original context of utterance, then the well-defined jurisdictions of the relativist will be impossible to maintain. The methodology of actor-network theory, which tracks the alignment of interests—both scientific and nonscientific—that have a stake in the fate of a piece of research (Callon, Law, and Rip 1986, as popularized in Latour 1987), makes this point quite vividly.

Someone with a Shallow Science perspective clearly refuses to take a term like tacit knowledge at face value. Rather than presuming that the term has a positive referent—namely, a scientist's unarticulated craft ability—the Shallow Science perspective treats appeals to the tacit dimension as rhetorical indicators of when one should stop asking scientists to account for their activities. A fascinating social history could be told about the shifting boundary between the "tacit" and the "articulate" in accounts of science. Such a history would search for the sorts of things that scientists and their epistemological mouthpieces have identified as the "proper objects" of intuition or immediate experience, which as such can be transmitted only by personal contact. (I would guess that the more items contained in a society's inventory of the tacit dimension, the more successful the scientists were at staving off the bureaucrats.)

From the Shallow Science perspective, Deep Science historians treat tacit knowledge somewhat naively by drawing a spurious distinction between the transience of explicit formal theories and the persistence of tacit laboratory practices. As the Shallow Science partisan sees it, this distinction may be due less to an absolute difference between theory and practice than to a difference between a practice legitimated by verbal means and a practice legitimated by nonverbal means. A practice that passes muster by saying (or measuring) certain things can be subjected to a finer-grained level of analysis—and hence of criticism and directed change—than a practice that requires simply that it appear (to the relevant audience) to be proceeding smoothly. Although the tacit practice may vary historically—just as the verbal practice—those variations would be harder to detect, let alone lead to improvements. Admittedly, matters quickly become complicated once one recalls that uttering the right words at the right time is routinely treated as a kind of silencing practice (or a "display of competence," as the Polanyites would say) that absolves the speaker from any further scrutiny of her position.

How can Deep Science be brought around to the normative perspective of Shallow Science? Simply put, Deep Science must "thicken" its conception of language use. Instead of the Deep Science partisan's sense of language as a pale abstraction of an ineffably rich world, the Shallow Science partisan presents language as a construction that sharply focuses an otherwise indeterminate reality. The thickener is rhetoric. If I may be allowed the philosophical indulgence of reconstructing history for my own purposes, the first stage in the thickening process takes us back to the Sophist Protagoras' invention of language as something that could be standardized and controlled, specifically, by shifting the paradigm of usage from sincere speech to grammatical writing (Billig 1987: Chap. 3). This shift from an aurally to a visually based communicative medium—or "externalization"—was accompanied by a creation of scarce conditions for access to this medium, a sure sign of language's materiality (cf. McLuhan 1962, 1964). Thus, people were shown to have differential access to communicative skills, the remediation of which required training in the verbal arts of rhetoric and dialectic. The final step that Protagoras took was to charge for his services, thereby converting a scarce resource into a marketable good. This last move enabled Socrates to launch one of the earliest attacks on the capitalist spirit. After all, as Socrates portrayed it, the Sophists were proposing to alienate the client from his soul and then reacquaint him with it at a cost. The Sophists failed to meet the Socratic challenge because the ease with which they flaunted their dialectical prowess, in both serious and playful settings, served to undermine the idea that the good they were peddling was truly scarce.

Plato then pushed Socrates offstage and concluded that right-minded speech was not scarce at all and, indeed, was universally available. However, certain people, the ones whose activities the Sophists fostered, unjustifiably tried to restrict access to such speech by eloquence, obfuscation, and threats. Plato's step here undid the thickening of language that Protagoras had begun. Had the thickening process continued, the Sophists would have supplemented their *embodiment* speech in grammar with an account of grammar's *embeddedness* in the material context of utterance. As the Sophists' conception of language became thicker, rhetoric would have yielded to the sociology of knowledge and political economy. Similar conclusions have been drawn by cognitive scientists, sociologists (cf. Shrager and Langley 1990: 15-19; Block 1990: Chap. 3), and rhetoricians (cf. McGee and Lyne 1987).

If Deep Science is wedded to a "thin" conception of language (as a kind of transparent representation of the world) and Shallow Science is wedded to a "thick" conception of language (as one fortified with rhetoric), the natural question to ask is how does one thicken the thin? Let me suggest here two translation strategies that capture the moments of embodying and embedding language. The idea behind the two strategies is that to thicken language is to give it spatiotemporal bearings. The boundaries of language so thickened constitute an "economy"—the metaphysical notion that not everything that is possible can be realized in the same time and place, and therefore every realization involves a trade-off of one set of possibilities against another. Embodiment and embeddedness address, respectively, the temporal and spatial dimensions of the thickening process. Thus, using "speech" to refer to a unit of discursive action, we have the following definitions:

> *Embodiment* (*Temporalization*): Language is embodied insofar as the goal of speech is manifested in the manner in which the speaker conducts herself during the time that she is speaking.

> *Embeddedness* (*Spatialization*): Language is embedded insofar as a speech is treated not as an instance of a universally attributable type, which everyone in the speech community possesses to the same extent, but rather as part of an object the possession of which is finitely distributed among the speech community's members.

In illustrating embodiment, consider the sorts of activities that are said to be done "for their own sake" or as "ends in themselves." Such Kantian talk signals that the consequences of pursuing these activities will not figure in their evaluation. Not surprisingly, Kantian talk is most effective when the activities in question have undetectable or diffuse consequences, as knowledge production is typically said to have. As we become more accustomed to planning our epistemic practices and monitoring their social consequences, this Kantian talk will lose currency. However, the so-called ultimate ends—such as peace, survival, happiness, and (yes) even *truth*—refer not to radical value choices for which no justification can be given, but rather to constraints on the manner in which other instrumentally justifiable ends are pursued. Thus, happiness in life is achieved not by reaching a certain endpoint, but by acquiring a certain attitude as one pursues other ends. A related point applies to the pursuit of truth. "Serious inquirers" comport themselves in a way that, over time, reinforces in others the idea that they have caught the scent of the truth. Admittedly, there is considerable disagreement over the exact identity of the relevant traits (e.g. how respectful of tradition?), but few doubt that there are such traits. Verbal attitudes that are incongruous with one's avowed aim do not wear well over time and are likely to be dismissed as inauthentic "mere rhetoric," failing to manifest "methodological rigor."

If, in terms of our metaphysical economy, embodiment is a measure of "return on investment" (i.e. whether my manner tends to diminish or enhance the audience's sense of my purpose), embeddedness is a measure of "currency flow;" what Michel Foucault (1975) called the "rarity" of an utterance. Embeddedness is tied to the social epistemologist's problem of determining what gives knowledge its "value," a point to which I return at the end of this chapter. The basic idea is that whenever someone speaks effectively, either she increases the effectiveness of what is said by decreasing the ability of others to follow suit or she decreases the effectiveness of what is said by increasing the ability of others to follow suit. Thus, the "currency" of what is said is either strengthened through restriction or weakened through inflation (cf. Klapp 1991). Magicians, for example, have for centuries passed down their lore through a highly guarded process of apprenticeship. This process ensures restricted access to the lore, which is integral to the "success" of magic on lay audiences. However, once a professional magician like the Amazing Randi breaks rank and divulges the secrets of his craft, magic loses much of its effect, devalued to simply another performing art or form of entertainment. (Would something similar happen if a band of Nobel Prize laureates publicly endorsed the Shallow Science perspective, admitting how it perfectly explained their own careers as scientists?) A related strategy, which is prominent in Chapter 5, is to destabilize the power relations embedded in restricting the applicability of value terms, for example, applying "rationality" or "intelligence" exclusively applying to human beings and then only to certain human beings in certain settings. By developing semantic conventions (metaphoric extensions, if you will) for applying, say, "rationality" to nonhuman entities or atypical humans, we make it harder to take politically significant action on the basis of that term. In this way, "rationality" is neutralized as a source of power.

Having now begun to lay some of my rhetorical cards on the table, I had best confront the most vexed player in this field, *rhetoric*.

RHETORIC: THE THEORY BEHIND THE PRACTICE

Using the word *rhetoric* is hardly the most rhetorically effective way to refer to anything, let alone something that might be properly called "rhetoric"! Whether the friends or foes of rhetoric are to blame is unclear. Rhetoric's friends tend to overemphasize the community-building functions of well-chosen language, often harboring some fairly nostalgic (if not downright mythical) views about the degree of common ground that is achievable or desirable between people. Desirability may be questioned insofar as communities where people are always *pleased* to listen to each other probably will learn little from whatever is said. Where in such communities is rhetoric's potential for *re*configuring the ways in which people relate to each other and to the world? For their part, rhetoric's foes have got that part of the story right. At the same time, however, their stress on the demystifying, divisive, and otherwise debasing character of rhetoric presupposes a trumped up (if not downright paranoid) view of rhetoric's pervasive and corrosive powers. Are all adept rhetors such sinister sirens? (Only your advertising agent knows for sure!) What, then, could we want to preserve from these vexed conceptions of rhetoric?

Rhetoric's place in my approach is to help overcome the antinomies that plague current STS thinking today, which have inhibited the development of the field toward social epistemology. These antinomies largely result from STSs having decisively discredited certain philosophical conceptions of science without leaving anything in their place. For openers, consider the following, very basic antinomy:

> (T+) Philosophers have claimed that language stands apart from the natural order it passively represents. Language thus functions as a "mirror of nature."
>
> (T-) STSers have shown that language is part of the natural order, with just as much capacity to move and be moved as anything else. Indeed language is much of the stuff out of which "nature" is actually constructed.

Swords appear to be crossed over the nature of language. But if we follow the long line of Western thinkers from Aristotle to Habermas who believe that linguistic ability is the mark of the human, then the antinomy may be seen more profitably as covertly expressing the dispute between determinism (T+) and free will (T-). Operating between these two extremes, rhetoric offers a sphere of "freedom within limits," an expression that harkens back to Kant and Hegel. "Freedom within limits" involves a distinction between rational freedom, which entails limits, and irrational freedom, which provides no limits. Rhetoric is the exercise of rational freedom. I can act rationally, in the sense of deliberating over alternatives, only if my options are limited and thereby surveyable. The truly free being, God, always sets limits. However, the rest of us make do in limited situations not of our own creation. This idea is what rhetoricians have traditionally called *exigence*, the feature of the world that brings forth the occasion for rhetorical invention (Bitzer 1968). Now the horizon of this inquiry can be broadened to include the conditions under which exigences are reproduced time and again—why it seems that we have only a limited set of options for dealing with certain recurrent situations (cf. McGee and Lyne 1987). A study of conventions would be grounded in an analysis of the power structure that maintains them. Understood as a systematic enterprise, STS is largely oriented toward this goal. The social epistemologist enters the picture to locate exigences that enable the destabilization of this power structure.

The reader is perhaps beginning to see rhetoric's place. To reinforce this perspective, let us consider rhetoric's role in resolving related antinomies. I have marked rhetoric's resolution as (T'):

> (T+) Philosophers have claimed that rational language use conceptually presupposes that a discourse could be understood by any other language user, regardless of her particular interests.
>
> (T-) STSers have shown that rational language use is relative to the standards of particular linguistic communities, whose differing interests may render their discourses mutually incomprehensible.
>
> (T') Rhetoricians have ways to help disparately interested parties overcome their language differences to join a common cause.

Here, *a priori* normative claims to a universal audience are met with *a posteriori* empirical claims to incommensurable worldviews, only to be resolved by *a posteriori* normative claims to what, in the next chapter, I call *interpenetrable* discourses. Another version of this antinomy—considering rational and irrational freedom—highlights the distinctiveness in how the rhetorician begins her inquiry:

> (T+) Philosophers erase the past and begin from scratch, much as God would have ideally designed the universe: first things first. This move

enables the philosopher to operate with maximum freedom, constrained only by the principles that she has already laid down.

(T-) STSers begin *in medias res* on the same ontological plane as the people they study, constrained only by whatever the people under study have let constrain their own practices.

(T') Rhetoricians also begin *in medias res*, but then design strategies for transforming recognized exigencies into normatively acceptable action.

The importance of this last antinomy for demarcating rhetoric from other disciplines cannot be overestimated. For example, philosophers typically propose normative theories of action that satisfy their colleagues but rarely the people (say, actual scientists) whose actions would be judged or governed by those theories. The same may be said of the models of rationality proposed by neoclassical economists. Consequently, as Laymon (1991) and others have observed, these theories are idealizations without being approximations of the phenomena they model. In other words, as such theories are supplemented with more realistic assumptions—about, say, human psychology, sociology, and the decision-making environment—their ability either to predict or to prescribe behavior does not improve accordingly. Rather, if the theory is to work at all, the normative theorist must supply *un*realistic auxiliary assumptions about human beings (the path of fictionalism), blame reality for its failure to conform (the path of moralism), or try to force reality into the mold of the theory (the path of coercion).

While not denying the occasional efficacy of these approaches, the rhetorician would argue that the normative project may be pursued more effectively. A more effective approach would factor in more realistic assumptions about the intended audience at the outset by respecting the fact that people are not blank slates at the beginning of normative inquiry waiting for the pronouncements of philosophers to give their lives direction. Rather, people in search of guidance come with certain concerns, habits of mind, and situations in which they are prepared to act. Any normative proposal must therefore take the form of advice that *complements* this state of affairs. Such advice must function as a "heuristic" that strategically compensates for biases and processing limitations that already exist in the target knowledge system. In terms more familiar to rhetoricians, norms are proposed in the spirit of shifting the burden of proof in a direction that enables more fruitful arguments to be made.

Among the various branches of philosophy, the rhetorician would find more kindred spirits in ethics than in epistemology. Traditionally, the standard of knowledge presupposed by epistemologists has been omniscience. Opinions that thrive on anything less—no matter how methodologically scrupulous they may be—are susceptible to the illusions of Descartes' Evil Demon. As a result, when epistemic norms are proposed, relatively little attention is paid to whether they would actually improve the conduct of inquiry if they were in place. Instead one is told that inquiry would improve in an ideal setting. Unfortunately, given the ever-present possibility of the Demon, the real world of inquiry is an unlikely setting. By contrast, ethicists do not typically aim to provide a set of moral principles that would always enable its adherents to resist the temptations of Satan. On the contrary, a point is often made of saying that ethics would not be needed if there were "moral saints" because no advice would be needed on how to *improve* one's conduct.

Ethics presupposes moral imperfection but also its corrigibility. Whereas epistemologists have only recently turned to cognitive science to grasp the psychological backdrop against which epistemic norms operate, moral psychology has been an integral part of ethical inquiry from Plato and Aristotle onward. Moral principles, such as Kantianism and utilitarianism, have been proposed in the spirit of disciplining or mitigating features of "human nature" that are already present when the ethicist begins her inquiry (Baier 1985). The exact consequences that these principles have for conduct will depend on the conception of human nature that the audience brings to the ethical forum. A utilitarian confident in her understanding of the world will take "the greatest good for the greatest number" as an injunction to engage in projects of deferred gratification that promise big long-term payoffs. A more skeptical utilitarian will interpret the slogan as a call for incremental policy and reversible decisions. Similarly, a confident Kantian will be relentless in her dutifulness, ignoring consequences completely. A less confident adherent to the categorical imperative will harbor a guilty conscience as she wonders whether she is, indeed, steadfast in her duty.

The closest epistemology gets to this spirit is Popper's falsification principle, which was designed to counteract our predisposition toward finding evidence that supports our own opinions. Popper (1959) repeatedly complained that by setting a superhuman standard for knowledge, epistemologists fostered two sorts of overreactions, either of which was sufficient to undercut any motivation for doing science. On the one hand, those who were confident in their fundamental beliefs wanted everyone to share them. On the other hand, those skeptical of their beliefs did not leave open the prospect that another set of beliefs might mitigate their skepticism. From the reaction of philosophers and STSers to falsification, one might conclude that Popper had a rhetorical sensibility (cf. Orr 1990). Consider the following three opinions on the viability of falsificationism, which correspond to the philosophers' (T+), STSers' (T-), and Popper's own (T'):

> (T+) Since it is easy to find counterinstances to any hypothesis, strict adherence to falsificationism would not allow hypotheses enough time to be developed before being tested.

> (T-) People are psychologically ill disposed to falsificationism, which explains why the principle has been rarely applied—despite claims to the contrary by philosophers and scientists alike.
>
> (T') Although it is easy to find counterinstances to any hypothesis, precisely *because* people are psychologically ill disposed to falsificationism, advising scientists to apply the principle will issue in an optimal turnover of hypotheses. The scientists' native resistance to falsificationism will cause them to fortify their hypotheses from attack so that only developed versions will ever be decisively falsified.

Arch-rationalist that he was, Popper would probably be the last to want to identify his approach with that of the rhetorician's. Unlike most philosophical pieces of advice, however, his is of the sort that might actually lead to better results the *closer* one moved toward a realistic understanding of human beings (cf. Gorman et al. 1984). By contrast, consider a formula such as the ever-popular Bayes theorem. It is a mathematical equation that determines the most plausible of a set of rival hypotheses by comparing their probabilities before and after a test has been run. The idea behind Bayes theorem, what philosophers after Peirce call "abductive" reasoning, is impeccable (Salmon 1967). Yet this precise guide to scientific reasoning fares poorly when addressed to human beings, whose computational powers are severely strained very quickly even when they are well disposed to using formal methods (cf. Faust 1985; Cherniak 1986). In an entirely serious vein, then, Glymour (1987) argued that such formal models of rationality are really suited to computer androids. Rhetorically speaking, the positivists who developed and promoted these models had a radically mistaken sense of audience. They failed to realize that their proposals could make sense only to machines that had yet to be invented! The history of formal reasoning as a philosophical institution prior to the computer revolution testifies to this point. With the exception of elementary logic exercises and cutting-edge logic research, formal models have functioned less as tools for the actual conduct of reasoning and more as yardsticks or templates for the evaluation of informally expressed arguments (cf. Toulmin 1958).

A historically salient feature that explains both rhetoric's virtues and ambivalent place in the academy is its self-image as primarily a practice, from which a body of doctrine may ex post facto be derived and taught. The pecking order implied here is quite the reverse of the one normally found in the academy. Conventional academic disciplines tend to regard practice—with more or less contempt—as an application of theory-driven research. But rhetoricians have been inclined to see matters the other way around, with academically certified knowledge being the ultimate safe haven for the failed practitioner. Those whose theories of rhetoric are confined to the classroom never meet the test of the marketplace: Those who can't do, teach. Rhetoric's epistemic prejudices make it the cousin of liberal professions, such as law, medicine, and engineering. I would argue that rhetoricians make good models for how STS practitioners should conduct themselves given their understanding of the nature of knowledge production.

Like practitioners of the liberal professions, rhetoricians are alive to the fact that the classroom and the textbook represent a limited range of communicative possibilities. Rhetoricians are expert in constructing the occasions and sites that call for certain forms of argument and persuasion. The kindred professional strategy is to create a universally felt "need" to see a doctor or lawyer when various personal and social exigencies arise. STSers need to craft such a need by addressing the ongoing problem of epistemic economy: the questions that arise from the production, distribution, and consumption of knowledge in society. However, as it stands, STS practitioners share with other academics a rather unimaginative sense of how to make use of their space and time. Where are the attempts to mix media, engage different audiences at different registers? Perhaps academics interested in STS should be taught not only public address (as I have required in my own seminars: see the Appendix to this book), but also the performing arts and architecture to refine their spatiotemporal sensibilities (cf. Soja 1988, who represents a school of "postmodern geographers" who urge this point in all seriousness). Continuing to write the same sorts of articles and books to the same audiences is not enough even if one asserts the fact–fiction distinction is being "blurred" or "crossed." If the communicative environment remains largely unchanged, these "new literary forms," as they are sometimes called (e.g. Clifford and Marcus 1986; Woolgar 1988a; Ashmore 1989), will simply have poured old wine into new caskets—the thin conception of language, yet again, whereby *only* the words have changed but not the social relations in which they are embedded.

ENTER THE SOCIAL EPISTEMOLOGIST

My version of social epistemology begins by reading the findings of STS research through a Shallow Science perspective. This generates three presumptions that inform the strategies and positions adopted in this book. In particular, they motivate the alliance between rhetoric and STS that I wish to forge, as well as encapsulate the issues raised up to this point:

> *The Dialectical Presumption*: The scientific study of science will probably serve to alter the conduct of science in the long run insofar as science has reached its current state largely in the absence of such reflexive scrutiny.

> *The Conventionality Presumption*: Research methodologies and disciplinary differences continue to be maintained only because no concerted effort

is made to change them—not because they are underwritten by the laws of reason or nature.

The Democratic Presumption: The fact that science can be studied scientifically by people who are not credentialed in the science they study suggests that science can be scrutinized and evaluated by an appropriately informed lay public.

These presumptions, in turn, generate certain semantic consequences that have been implicit in my past work, but which I now make explicit so readers are not misled by what follows. These consequences consist of the following collapsed binaries: *Reasons = Causes; Natural = Social; Public = Policymaker.*

Reasons = Causes: This follows in the wake of the Dialectical Presumption. Both supporters and critics of science typically capitalize on the distinction between these two terms to quite opposite effects. Supporters use it to ground the difference between an autonomously driven knowledge enterprise (governed by "reasons") and one driven by external social factors (swayed by "causes"). Critics use this distinction to separate the ideology that scientists invoke to legitimate their activities (mere "reasons") from the true account of why they do what they do (real "causes"). The possibility of drawing this distinction—and the internal/external histories of science that it breeds—diminishes as scientists come to justify their activities in the sorts of terms that best explain them. That a distinction between reasons and causes continues to exist is a measure of the extent to which knowledge generated by STS has yet to feed back into the conduct of the inquiry (cf. Fuller 1988a: Appendix B, 1989a: Chap. 1).

Natural = Social: This follows in the wake of the Conventionality Presumption. I typically mean "science" in the generic, German sense of *Wissenschaft*, a systematic body of knowledge closed under a canonical set of methods and a technical vocabulary. Discipline is the best one-word English translation. Unless otherwise indicated, discipline refers indifferently to the natural and the social (human) sciences. I am not simply pitching my claims at a level of abstraction where such a distinction no longer makes a difference (certainly, that would accord with the "epistemological" character of social epistemology). More important, from the STS perspective, the natural sciences consist of certain strategies for mobilizing societal resources. Indeed as becomes clear in my discussion of the rhetoric of science policy, natural scientific research indirectly tests hypotheses about social organization and political economy. The success or failure of those strategies and hypotheses determines the longevity of a given science—and much else of societal import.

> *Public = Policymaker*. This follows in the wake of the Democratic Presumption. If the promise of STS is delivered and the workings of science can be understood by nonexperts, then each person currently identified as a "knowledge policymaker"—a government bureaucrat, say—will have the status of *primus inter pares*: someone whose role as policymaker is potentially interchangeable with that of any other concerned and informed citizen. This projected state of affairs will be brought about not by everyone acquiring formal training in all the sciences, but by scientists learning to account for their activities to larger audiences, which, in turn, enables everyone to assume a stake in the outcome of research. A high-priority item for social epistemology, then, is the design of rhetorics for channeling policy-relevant discussions in which everyone potentially can participate.

The larger context in which social epistemology is situated is the profound ambivalence that Western philosophers have had toward the equation of knowledge and power. Admittedly, this ambivalence has become increasingly obscured in the 20th century as epistemology (including philosophy of science) and ethics (including social and political philosophy) have evolved into separate specialties, especially in the Anglo-American analytic tradition. However, the problem is easily recovered once we see the Western tradition as having been fixated on the problems of *producing knowledge* but *distributing power*. Consequently, epistemology has tended to concentrate on practices with the highest levels of epistemic productivity ("science") regardless of their access to society at large. Ethics has focused on schemes for equitable distribution without considering the costs of (re)producing the institutions needed for implementing those schemes. Thus, social epistemology is born with an "essential tension" (Roth 1991): how to balance Machiavellian and democratic impulses?

The Machiavellian impulse is toward maximizing the production of knowledge and power, even if the means of production are concentrated in an elite cadre of "epistemocrats." By "epistemocrats" I mean those whose superior knowledge of people (and what is good for them) enables them to mask their own interest in bringing the world into alignment with their normative model. The ultimate source here is Plato. In the Aristotelian *phronesis* approach to politics, rulers are no smarter than the ruled except in their ability to represent several constituencies at once. The Platonic *episteme* approach involves the ruler in strategic overclarification and illusion to guide the populace toward a normatively acceptable end. As I show in Chapter 4, economists have been especially skillful in converting "purity" to "power" in this manner (Proctor 1991). By contrast, the democratic impulse aims to maximize the distribution of knowledge and power, even if this serves to undermine the autonomy and integrity of current scientific practices. Democratic modes of persuasion are entirely open faced: If I can't justify my knowledge claims to you, then you have no reason to believe them.

Social epistemology's relevance to rhetoric and argumentation lies in its stress on the integral role that communication, both its facilitation and impedance, plays in contemporary thinking about knowledge and power. The most distinctive contributors to social epistemology in our time—Karl Popper, Thomas Kuhn, Michel Foucault, and Jürgen Habermas—can be best understood in terms of the type of communication they take to be realizable in today's world. A useful way to configure these four disparate thinkers is in terms of the following chain of ideas: Free access to the communicative process breeds increased accountability, which in turn forces aspiring authorities to couch their claims to knowledge in terms that can be understood by the largest number of people. By leveling terminology, we convey the idea that we all live in the same world. Any apparent differences in the access we have to that world are attributed to epistemic artifice—"ideology," if you will—which typically masquerade as ontological differences or "incommensurable worlds." These world differences restrict the number of eligible critics of one's claims to the class of people known as "experts" or "natives." In *Social Epistemology*, I argued that this chain of ideas implies that communication breakdown is the leading cause of cultural difference, the diachronic version of which is conceptual change (Fuller 1988a: xiii).

Popper's "open society" account of knowledge production articulates the positive relation between cognitive democracy and one world suggested in the previous scenario. Kuhn's "paradigm" picture of the scientific enterprise asserts the negative relation between cognitive authoritarianism and a plurality of discrete worlds. However, I see both Kuhn and Popper as talking mainly about the implications of opening or closing discourse for one's own pursuits. In contrast, Foucault and Habermas are more concerned with the implications that these possibilities have for what *others* do. Foucault teaches that the power associated with claims to superior knowledge accrues to those who can suppress alternative voices or, in Kuhnian terms, consign others to worlds incommensurable with one's own. In the case of scientific authority, this suppression is best studied in terms of the presumptions that aspiring revolutionaries need to overturn before being granted a complete hearing (Fuller 1988a: Chap. 4). Habermas, however, wants each inquirer to submit her claims to a series of validity checks that exert a measure of self-restraint. These checks give others a chance to stake their own claims. If Foucault is an other-directed Kuhn, Habermas is an other-directed Popper—at least from the social epistemologist's vantage point. The result is shown in Figure 1.1. The particular philosophical lesson about the knowledge–power nexus that social epistemology teaches from this configuration of Foucault, Kuhn, Habermas, and Popper is that *knowledge differences become reality differences when it becomes impossible to communicate across those differences.*

Knowledge Politics / **Implications For**	COGNITIVE DEMOCRACY (Leveled Playing Field)	COGNITIVE AUTHORITARIANISM (Multiple Jurisdictions)
SELF-INTEREST	Popper's Open Society	Kuhn's Paradigms
TREATMENT OF OTHERS	Habermas' Ideal Speech Situation	Foucault's Suppressed Voices

FIG. 1.1 Social epistemology's universe of discourse.

As a positive research program, social epistemology proposes inquiries into the maintenance of the sort of institutional inertia that has made social epistemology's three presumptions (dialectical, conventionality, democratic) radical rather than commonplace. Why don't research priorities change more often and more radically? Why do problems arise in certain contexts and not others? Why is there more competition for resources within a discipline than between disciplines? A sensitivity to latent incommensurabilities turns out to *help*, not hinder, this sort of critical knowledge policy. Armed with the tools of the STS trade, the social epistemologist can isolate the quite heterogeneous set of interest groups that derive enough benefits, in their own distinctive ways, from the status quo that they have little incentive to change. The strategy, then, would be to periodically restructure the environments in which researchers compete for resources. The terms of this restructuring may be quite subtle (such as providing incentives to reanalyze data gathered by earlier researchers). Less subtly, researchers may be put in direct competition with one another where they previously were not. Moreover, researchers may be required to incorporate the interests of another discipline, including that discipline's practitioners, to receive adequate funding. Finally, researchers may be forced to account for their findings not only to their own discipline's practitioners, but also to the practitioners of other disciplines and maybe even the lay public.

While a long-term goal, I see this last step as essential to social epistemology's project of locating the *value* of knowledge (Fuller 1988a: Chap. 11). The value of knowledge has been discussed in the philosophy of science in one of two ways, mirroring a dichotomy already present in theories of value available in economics (cf. Mirowski 1989). On the one hand, there is a kind of *labor* theory of epistemic value. This theory locates the value of knowledge in the difficulty or improbability of extracting knowledge from the world. Knowledge itself is natural stuff (brains, books, etc.) that has been substantially transformed by a scientist's labor. There are High Church and Low Church prototypes for this view. The High Church evokes Francis Bacon's view of clever experiments as the means by which humans overcome their own ignorance and nature's resistance in yielding its secrets. The Low Church evokes diligence, testing one's mettle, and "hard thought" as educational virtues.

On the other hand, there is a kind of *utility* theory of epistemic value. This theory points to the capacity of knowledge for organizing a wide variety of phenomena, which can in turn be used to realize a wide variety of ends. Knowledge is a field of rival means–ends relations (or if–then statements) that pull the scientist in different directions to different degrees. On the High Church side lies Newtonian mechanics as a model of parsimonious explanatory theory for all the sciences; hence, the ultimate means to every scientist's ends. On the Low Church side lie the consumer technologies that enable large numbers of people to satisfy their wants with ease. As Joseph Agassi (1985) observed in another context, these two classical views—the labor theory associated with basic research and the utility theory with applied research—are fundamentally opposed. These views coexist only as a result of a hard-won exchange forged in the academy. Basic researchers exchanged some of their prestige and allowed applied researchers to work alongside them. In return for a piece of the applied researcher's credentialing process, basic researchers were assured a steady stream of students for the pure sciences. This grafting of labor onto utility theories of epistemic value is reflected in every curriculum that requires engineers to study branches of physics and mathematics or that requires medical practitioners to study branches of biology and chemistry. In many cases, the study of basic research diverts from, if not outright impedes, the mastery of the relevant applied techniques.

As against both the labor and utility views, I propose that the value of knowledge lies in the ability of its possessor to influence the subsequent course of its production. Thus, the physicist's knowledge of physics is worth more than, say, a popularized account of quantum mechanics not because of its inherent profundity or its ability to ease the lives of the physicist and others, but because of the relative ease with which the trained physicist can intervene in the production of physical knowledge. The most obvious advantage of my view is that it brings under one rubric the epistemic idea of *demonstration* and the political idea of *empowerment*. Consequently, competence is judged in terms of an appropriate alteration of the tradition rather than a simple reenactment of it.

The ability to influence the course of knowledge production also calls into question the value of being a mere possessor, or "consumer," of knowledge, which affects how one thinks about the ends of education (Fuller 2002b: Chap. 2). Epistemic value, then, is gauged not only in terms of certain products, but more important in terms of certain *productive capacities* that are ideally distributed through the knowledge system. In that case, education can serve to devalue the currency of knowledge if students come to "understand," say, the nature of scientific research or democratic government without being provided the opportunity to affect the course of these institutions. Feminists have been especially sensitive to this point. To wit, women more quickly gained access to seats in college classrooms than to places at the lecture podium (cf. Hartman and Messer-Davidow 1991). In the first half of this century, courses in "civics" in American public schools aimed to address this problem by instructing students on the political mechanisms at their disposal. Nothing comparable has yet been done for science education. At best schools produce "pure consumers" of science who regard scientific research and its technological extensions as being as normal and unchallengeable as any of their own daily activities. Education of this sort, for all its distribution of facts and figures, is akin to indulging in a high-calorie diet without vigorous physical exercise: The citizenry's epistemic energy is converted to an acquiescent adiposity!

By helping to reconfigure the variables of knowledge production, the social epistemologist can ensure that disciplinary boundaries do not solidify into "natural kinds" and that the scientific community does not acquire rigidly defined class interests. Such reconfigurations will go a long way toward keeping the channels of communication open between sectors of society that seem increasingly susceptible to incommensurability. Indeed this strategy would even alter the character of the knowledge produced, including perhaps what we take something to be when we call it "knowledge." In all this, social epistemology needs to be a thoroughly rhetorical enterprise. Consider the two different contexts of persuasion that are implicated in the prior discussion. First, there is the need to motivate scientists to restructure their research agendas in light of more general concerns about the ends that their knowledge serves. Second, there is the need to motivate the public to see their fate as tied to the support of one or another research program. As long as a set of norms, and the rhetorical transactions underlying them, remain in force unexamined, they will fail to receive the explicit consent of the governed: inertial producers matched with inert consumers. Thus, the social epistemologist recognizes the essentially rhetorical character of normative action, to wit:

> *A necessary (although not sufficient) condition for the appropriateness of a norm is that the people to whom the norm would apply find it in their interest to abide by the norm.*

The standpoint of *interpenetrative interdisciplinarity* will consider who these people are and what their interests might be.

THOUGHT QUESTIONS

☙ Is a "science of science"—an empirical approach to studying and evaluating the means by which scientific knowledge is produced—possible? If so, on what grounds could one, who is not a practitioner of science, offer normative evaluations regarding how scientific knowledge is produced? If not, are scientists themselves ultimately responsible for how knowledge is organized, used, and diffused in society?

☙ The approach of 19th-century European theorists to science and history is termed rhetorical insofar as their goal was to help fashion the reception of scientific claims over rival claims made by competing groups and institutions. Who are these theorists? Considering specific examples, do you accept the premise that a given theorist, through rhetoric, sought to secure and was instrumental in achieving a particular historical outcome? What strategies and arguments are employed to secure the epistemic authority of science? Is the persuasive effect of these arguments explanatory in accounting for the epistemic ascendancy of science? Do the strategies employed by 19th-century theorists have any modern rhetorical currency? Why or why not?

☙ Fuller's account of the aims of 19th-century theorists, with regard to science and history, provides the groundwork for his claim that Thomas Kuhn, in the *Structure of Scientific Revolutions*, began to "undo" the project of history and philosophy of science. Do you agree with the historical reasoning supporting this claim? Please explain your answer. In part, Fuller suggests, the didactic aim of *Structure* was to prepare members of society to become connoisseurs of science. Broadly, what place has the judgment of critics—whether of art, literature or science—had on the conduct on a given activity? How can we explain the relative effectiveness of critics and criticism rhetorically?

☙ What are the differences between STS and the project of social epistemology? Can the "major contributors" to STS be considered to be social epistemologists? Are the major contributors in STS "rhetorical" in the same way as 19th-century European theorists?

☙ Fuller maintains that STS requires a "reflexive posture" in which science studies practitioners address, in open forums, the way in which their practice should be pursued in light of empirical research and findings on the conduct of science and technology. Do you agree? What are the advantages and disadvantages of such a reflexive posture? Can you provide examples of other disciplines that are reflexive?

☙ Does Fuller's distinction between Deep Science and Shallow Science strike you as true or efficacious? What implications would adopting a Deep

Science or Shallow Science perspective have on studying science scientifically? On studying science rhetorically? How might Deep Science or Shallow Science perspectives be tailored to provide a more or less normative approach to the study of science? What is the role of rhetoric in shaping Deep Science or Shallow Science approaches to science?

☙ Rhetoricians, Fuller suggests, have the tools that allow for language differences, intrinsic to given circumstances, to be resolved among interlocutors and to be changed into "normatively acceptable action." Initially, how does the rhetorician determine what passes for actions scientists (or other practitioners) should take? How might the disciplinary or professional backgrounds of interlocutors determine what passes for the actions one should take? On what basis would a rhetorician resolve the difference in normative orientations among practitioners from different fields, disciplines, or professions? For rhetoricians to act effectively as "disciplinary diplomats," would the participants in a negotiation need to agree to "thick" conception of language? Why or why not?

☙ What are the means at the rhetoricians' disposal to resolve the linguistic differences among parties interested in negotiating disciplinary disputes? How can rhetoricians bring reticent parties to the table? Citing a specific example, what might be the aim of negotiating a given disciplinary dispute? That is, what might a normative proposal from a rhetorician to given practitioners look like? What are examples of normative criteria governing scientific practice that have been proposed by practitioners outside of the field? Can you think of examples of normative criteria proposed by scientists to govern the practice of an outside field?

☙ What are the similarities and differences (if any) between a social epistemologist and a rhetorician? Are the presumptions that inform the practice of the social epistemologist compatible with the presumptions that inform the practice of the rhetorician?

☙ What are the differences between the High Church and Low Church branches of Science and Technology Studies? How is knowledge valued and expressed, rhetorically, in each branch? What is held as the value of knowledge on the social epistemologist's view? Do you agree with Fuller's conception of knowledge as "currency"? Why or why not? If we view knowledge as a commodity, what difficulties do you foresee in determining its value in the marketplace of academic disciplines and in professions? Does Fuller suggest that knowledge of language and communication has a permanent value in all epistemic transactions? How so?

☙ How does the social epistemologist help to "reconfigure the variables of knowledge production"? What role does the rhetorician help to "reconfigure the variables of knowledge production"? What would the process of determining norms regulating the production of knowledge look like? Can you give examples of people, professions, or disciplines that have solicited or that may solicit help in establishing norms governing knowledge production?

2

The Position: Interdisciplinarity as Interpenetration

THE TERMS OF THE ARGUMENT

I understand *interdisciplinarity* as both a fact and as an ideology. Certain sorts of problems—increasingly those of general public interest—are not adequately addressed by the resources of particular disciplines. Rather, these problems require that practitioners of several such disciplines organize themselves in novel settings and adopt new ways of regarding their work and coworkers. As a simple fact, interdisciplinarity responds to the failure of expertise to live up to its own hype. Assessing the overall significance of this fact, however, can easily acquire an ideological character. As an ideologue of interdisciplinarity I believe that, unchecked, academic disciplines follow trajectories that increasingly isolate themselves from one another and from the most interesting intellectual and social issues of our time. The problem is only masked by dignifying such a trajectory with the label "progress." Thus, I want to move away from the common idea that interdisciplinary pursuits draw their strength from building on the methods and findings of established fields. My goal is to present models of interdisciplinary research that call into question the differences between the disciplines involved, and thereby serve as forums for the renegotiation of disciplinary boundaries. This goal is perhaps the most vital epistemological function for rhetoric to perform in the academy, the need for which has become clear only with the emergence of STS.

An interesting, and probably unintended, consequence of the increasing disciplinization of knowledge is that the problem of interdisciplinarity is drawn closer to the general problem of *knowledge policy*—the role of knowledge production in a democratic society (Fuller 1988a: Appendix C). As disciplines become more specialized, each disciplinary practitioner, or "expert," is reduced to lay status on an expanding range of issues. Specialization serves to heighten the incommensurability among the ends that the different disciplines set for themselves. In turn, experts' abilities to coordinate their activities in ways that benefit more than just their respective disciplinary constituencies diminish. The increasingly strategic roles that deans, provosts, and other transdepartmental university administrators play in shaping the future of departments testify to the tendency of assimilating the problem of interdisciplinary negotiation to the problem of knowledge policy.

A complementary trend is the erosion of the distinction between academic and nonacademic contexts of research. Currently, corporations subsidize not only academic research, but also often pay for the university buildings in which the research occurs. Either through government initiatives, venture capitalism, or the lure of the mass media, the nonacademic public is potentially capable of diverting any narrowly focused disciplinary trajectories. Social epistemology's contribution to these tendencies, one might say, is to make such initiatives intellectually respectable. The key is to cultivate the *rhetoric of interpenetrability*. Although the technofeminist Donna Haraway (1989) revived the idea behind interpenetration (to produce "cyborgs," techno-organisms that interpenetrate the nature–culture distinction), the term probably still carries enough of the old Marxist baggage to merit unpacking.

" The interpenetration of opposites," also known as "the unity and conflict of opposites," is one of the three laws of dialectics identified by Friedrich Engels in his 1883 work on the philosophy of science, *The Dialectics of Nature*, now a staple of orthodox Marxism. Put metaphysically, Engles' idea is that stability of form—the property that philosophers have traditionally associated with a thing's identity—inheres in parts whose tendencies to move in opposing directions have been temporarily suppressed. Marx applies the interpenetration of opposites in the concept of *structural contradiction*. This idea purports to explain the lack of class conflict between the workers and the bourgeoisie by holding that the workers unwittingly buy into capitalist ideology and, hence, fail to identify themselves as a class with interests opposed to those of the bourgeoisie. The Italian humanist Marxist Antonio Gramsci popularized the term hegemony to capture the resulting ideological harmony, which leads workers to blame themselves for their lowly status. However, armed with the Marxist critique of political economy, the workers can raise this latent contradiction to the level of explicit class warfare. Once the workers identify exclusively with each other, they are in a position to destroy the stability of the capitalist system. Now consider a rhetorical example that makes the same point. Philosophers since Plato have supposed that communication involves speaker and audience partaking of a common, reliable form of thought. Rhetoricians have taken the interpenetrative view that any apparent meeting of minds is really an instance of strategically suppressed disagreement that enables an audience to move temporarily in a common direction.

An unlikely place for strategically suppressed dissent to apply, yet where it applies with a vengeance, is in the *history of tolerance*. First, there is what might be called *passive tolerance*, the ultimate target of sophisticated forms of censorship, yet still unrecognized by philosophers as a legitimate epistemological phenomenon. In the 1950s Carl Hovland and his Yale associates (1965) captured passive tolerance experimentally as "the sleeper effect": Subjects become better disposed to a message after repeated exposure over time even if they were originally ill-disposed because of the source of the message. Thus, even conservatives may start to express sympathy for a liberal's proposed social program once they hear about it enough and forget its liberal origins. At least the burden of proof starts to shift in their minds so that now they might want to hear arguments for why the program should *not* be funded. In a democracy whose mass media are dedicated to the equal-time doctrine, managing this form of tolerance is a rhetorical and epistemological challenge. Given that the proliferation of messages serves only to increase the amount of passive tolerance, the trick is to "activate" tolerance without thwarting it.

Active tolerance aims, in theory, to empower groups by channeling their attention toward one another. In practice, active tolerance often turns out to be a version of "my enemy's enemy is my friend": Otherwise squabbling factions agree to cease hostilities to fend off a still greater and mutual foe. John Locke's *Letter on Toleration* of 1689, which influenced the establishment of religious tolerance in the American colonies, defined the common enemy as an ominous band of "atheists" who had no place in a Christian commonwealth. The logic of interpenetration can work in this environment if the threat posed by the foe forces the factions beyond mere peaceful coexistence to active cooperation in combating the foe (cf. Serres 1982, on the strategy of removing a "parasite"). Once the foe has been removed and all the factions are able to go their own way, they will have been substantially transformed as a result of their collaboration.

The rhetoric of interpenetrability aims to recast disciplinary boundaries as artificial barriers to the transaction of knowledge claims. Such boundaries are necessary evils that become more evil the more they are perceived as necessary. I urge a rhetoric that shows the ways in which one discipline takes for granted a position contradicting, challenging, or in some way overlapping a position taken by another discipline. As a dialectical device, interpenetrability goes against the grain of the current academic division of labor, which typically gives the impression that issues resolved in one discipline leave untouched the fate of cognate issues in other disciplines. For example, one might think that psychologists' laboratory findings have no necessary bearing on the psychological makeup of the sort of ordinary "situated" reasoners that historians and other humanists study. No mutual challenge is posed by the juxtaposition of laboratory cognizers and historical cognizers. Hence, any interaction between the two types will be purely a matter of the inquirer's discretion. In this context advocates of interdisciplinarity, especially the cultural anthropologist Clifford Geertz (1983), have traditionally spoken of social scientific theories as "interpretive frameworks" that can be applied and discarded as the inquirer sees fit, but never strictly tested.

In stressing applicability over testability Geertz and other interdisciplinarians were reacting, perhaps overreacting, to positivist academic rhetoric. The aim of Popper's falsificationist methodology had been to *eliminate* false hypotheses. The finality of such eliminationist rhetoric made one close follower of Popper, Imre Lakatos, squirm over the possibility of preemptively squashing fledgling research programs. Ultimately, another of Popper's famous students, Paul Feyerabend, espoused the anarchistic doctrine of letting a thousand flowers bloom. Even as a simple fact about the history of science, eliminationism is hard to justify. Once articulated, theories, for better or worse, tend to linger and periodically reemerge in ways that make half-life an apt unit of analysis.

Unfortunately, the explicitly nonconfrontational strategy of Geertz and his cohort plays in the worst way to the exigencies of our cognitive condition. There is little need to belabor the point that, for any field, more theories are generated than can ever be given a proper hearing. How then does one decide on which theories to attend to and which to ignore? Testability conditions of the sort Popper offered under the rubric of falsifiability constitute one possible strategy. For example, a theory may challenge enough of the current orthodoxy that the orthodoxy would be significantly overturned if the theory were corroborated. This theory is one that Popper might test. However, if inquirers are allowed complete discretion on how they import theories into their research, they will likely capitalize on their initial conceptions as much as possible and ignore—not test—the theories that implicitly challenge those conceptions. In the long term, the nonconfrontational approach would probably lead to the withering away of subversive theories that could be accommodated into standing research programs only with great difficulty. My point here is that, unless otherwise prevented, inquirers will diverge in ways, mostly involving the elaboration of incommensurable technical discourses, that will make critical engagement increasingly difficult.

Much of the sting of Popper's rhetoric could be avoided if testing were seen more in the spirit of a Hegelian *Aufhebung*—the incorporation and elimination of opposites in a more inclusive formulation. Concretely, I suggest that when disciplines (or their proper parts, such as theories or methods) interpenetrate, the "test" is a mutual one that transforms all parties concerned. One cannot, however, simply test a discipline against the standards of its epistemic superior or, even, evaluate both disciplines in terms of some neutral repository of cognitive criteria (as might be provided by a philosopher of science). Rather, *the two disciplines are evaluated by criteria that are themselves brought into being only in the act of interpenetration*. These criteria will undoubtedly draw on the settlements reached in earlier interdisciplinary disputes. Still the exact precedent that they set will depend on the analogies that the current disputants negotiate between these prior exchanges and their own.

THE PERILS OF PLURALISM

Although the three presumptions that social epistemology takes from STS—the dialectical, the conventional, and the democratic—make me a natural enemy of "traditionalists" in the academy (e.g. Bloom 1987), my comments in the last section are meant to throw down the gauntlet to many of the so-called *pluralists* (e.g. Booth 1979) who normally oppose the traditionalists. Despite their vocal support of interdisciplinary research, pluralists assume that practitioners of different disciplines, left to their own devices and absent any overarching institutional constraint, will spontaneously criticize one another in the course of borrowing facts and ideas for their own purposes. If Popper's "Open Society" were indeed a by-product of such a pluralistic academic environment, the social epistemologist would not need to cultivate interventionist impulses. However, I believe that criticism requires special external incentives. Otherwise each discipline will politely till its own fields, every now and then quietly pilfering a fruit from its neighbor's garden but never suggesting that the tree should be replanted in a more mutually convenient location. My view here rests on the observation that criticism flourishes in the academy—insofar as it does—only within the confines of disciplinary boundaries (say, in journal referee reports) and erupts into symbolic violence when it spans such boundaries. Given this state of affairs, the "tolerance" revered by pluralists turns out to be the consolation prize for those who are unwilling to face their differences.

In terms of the idea of active tolerance raised previously, there are two directions in which a tolerant community may go at this point. On the one hand, it may take advantage of the opportunity provided by realizing that "my enemy's enemy" is really "my friend" and foster an interpenetrative intellectual environment. On the other hand, the community may foster just the reverse perhaps out of fear that voiced disagreements would allow the enemy to reappear. As the "tolerant" Christian commonwealth holds, interdenominational strife is Satan's calling card. In a more secular vein, commonly in the history of academic politics, rival schools of thought ceased fire whenever a more powerful "third party," usually a government agency, was in a position to discredit the knowledge produced and gain advantage over the feuding parties. For example, Proctor (1991: Chap. 8) argued that sociologists in early 20th-century Germany became preoccupied with appearing as "value-neutral" inquirers when it became clear that an assortment of conflicting normative programs were being advanced on the basis of scholarly research. By suppressing these deep disagreements, the sociologists believed (with mixed results) that they could counter government suspicions that the classroom had become the breeding ground for alternative ideologies, and thereby salvage the "autonomy" of their inquiries. (Furner 1975 offers the American analogue to this story.) From the standpoint of social epistemology, a better strategy would have been for the sociologists to argue openly about what normative programs they wanted their research to legitimate and to enroll various government agencies as allies in the ensuing debate. So doing would dissipate whatever leverage the state could exercise in its official capacity as "external," "neutral," and, most important, *united.*

Tolerance works homeopathically: In small doses, it provides the initial opportunity for airing differences of opinion, which will hopefully lead to an engagement of those differences. However, in large doses, tolerance replaces engagement with provincialism and produces Robert Frost's policy of "good fences make good neighbors," and the veiled sense of mutual contempt that it implies. The unconditional protection of individual expression not only fails to contribute to the kind of collaborative inquiry that sustains the growth of knowledge, but also fails to foster healthy social relations among inquirers. In particular, individual expression instills an ethic of *learning for oneself* at the expense of *learning from others*. This ethic accounts for interdisciplinarians' tendency to become "disciplines unto themselves"—increasingly fragmented sects unwittingly proliferating old insights in new jargons that are often more alienating than those of the disciplines from which they escaped.

My complaint here is that interdisciplinary fields mutate *without replacing some already existing fields*. Interdisciplinary fields merely amplify, not resolve, the level of babble in the academy. Given the exigencies of our epistemic situation, pluralists hardly help matters by magnanimously asserting that anyone can enter the epistemic arena who is willing to abide by a few procedural rules of argument that enable rival perspectives to remain intact and mutually respectful at the end of the day. (After all, isn't the security of this outcome what separates the interdisciplinary environment of the academy from the rough-and-tumble world of politics?) In practice, this gesture amounts to one of the following equally unsavory possibilities:

1. Everybody gets a little less attention paid to her own claims to make room for the newcomer.

2. The newcomer starts to adopt the disciplinary perspective of the dominant discussants, and consequently is seen as not adding to the level of academic babble.

3. Given that the newcomer starts late in the discussion, her claims never really make it to the center of attention.

Newcomers, of course, fear that (3) is the inevitable outcome, although the path of cooptation presented in (2) does not inspire confidence either. As a result, newcomers have been known to force themselves on the discussion by attempting to "deconstruct" the dominant discussants calling into question the extent to which the discussants are really so different from one another, especially in a world where there are still many other voices yet to be heard. Aren't they all *men*? Aren't they all *white*? Aren't they all *bourgeois*? Aren't they all *normal scientists*? The suggestion is that if the discussants are "really" all the same, they can easily make room for the genuine difference in perspective offered by the newcomers. Clearly, the deconstructive newcomers are trying to totalize or subsume all who have come before them, which gives their discourse a decidedly *theoretical* cast.

Critics of untrammeled tolerance and pluralism have observed that pluralists become extremely uncomfortable in the face of this theoretical cast of mind regardless of whether the source of the theory is Marxism, feminism, or positivism, for that matter. (Kindred suspicions have surrounded "synthetic" works in history, which, while not especially theoretical, nevertheless juxtapose pieces of scholarship in ways other than what their authors originally intended; cf. Proctor 1991: Chap. 6). After all, the deconstructors have turned the pluralist's procedural rules into topics in their own right. No longer neutral givens, the rules themselves now become the bone of contention. Rules appear to foster a spurious sense of diversity that, in fact, excludes the most challenging alternatives. I return to this topic under the rubric of "knowledge politics" in Chapter 8.

Pluralist forms of interdisciplinarity reinforce the differences between disciplines by altering the *products* of research while leaving intact research *procedures*. A good piece of interdisciplinary research is supposed to abide by the local standards of all the disciplines referenced. This standard exists despite the fact that most disciplines are born of methodological innovations that, in turn, reflect deep philosophical dissatisfaction with existing methods. Given such a historical backdrop, research simply combining the methods of several disciplines—say, a study of attitude change that wedded historical narrative to phenomenological reports to factor analysis—would hardly constitute an improvement on the rigorous deployment of just one of the methods. Thinking that combining disciplinary methods would automatically constitute an improvement is to commit the *fallacy of eclecticism*—the belief that many partial methods add up to a complete picture of the phenomenon studied (rather than simply to a microcosm of cross-disciplinary struggles to colonize the phenomenon). The fallacy is often undetected. Interdisciplinarians deftly contain the reach of any one method so as to harmonize it with other methods that together "triangulate" around the author's preferred account of the phenomenon. Readers, of course, are free to infer that one method was brought in to compensate for the inadequacies of another, but the nature and potential scope of the inadequacies are passed over by the author in tactful silence.

Triangulation is regarded in a favorable light in the social science methods literature (e.g. Denzin 1970; Webb et al. 1981). Here triangulation appears as a means to ensure that the inherently partial and reductive nature of a given research tool does not obscure the underlying complex reality that the researcher is trying to capture. Not surprisingly, discussions of triangulation focus on the need for multiple methods to achieve a balanced picture of reality—not on the more basic fact that the biases introduced by divergent methods persistently reemerge across virtually all research contexts. Triangulation, then, defers an airing of these differences to another day or, perhaps, another forum, such as the philosophy of social science, where the results of deliberations are less likely to be felt by research practitioners. (For this reason, ethnomethodologists have been especially insistent on letting these metascientific concerns interrupt and shape their research practices; cf. Button 1991: especially Chaps. 5-6).

Another sort of triangulation is prominent among humanists who attempt to "blur genres," in Clifford Geertz's (1980) memorable phrase. Geertz (especially 1973) is among the most masterful of these eclectics. A discussion ostensibly devoted to understanding the practices of some non-Western culture will draw on a variety of Western interpretive frameworks that sit well together just as long as they do not sit for too long. For example, an allusion to the plot of a Shakespearean tragedy may be juxtaposed with Max Weber's concept of rationalization to make sense of something that happens routinely in Southeast Asia. The juxtaposition is vivid in the way a classical rhetorician would have it—namely, as a novel combination of familiar tropes. In fact the brilliance of the novelty may cause the reader to forget that it is meant to illuminate how a non-Western culture actually is, rather than how a Western culture might possibly be. But most important, Geertz's eclecticism caters, perhaps unwittingly, to what the structural Marxist Louis Althusser (1989) astutely called the *spontaneous philosophy of the scientists.* By this Althusser meant the tendency for an inquirer to understand her own practice in terms of her discipline's standing with respect to other disciplines, which is usually as part of a sensitive and closely monitored balance of power. Goldenberg's (1989) survey of scientists' attitudes toward science—to be discussed at the end of this chapter—illustrates nicely the way in which the philosophical self-images of the various sciences reinforce one another. Of special interest here is the fact that this reinforcement takes place *regardless* of whether the sciences in question respect or loathe one another. In both cases, interdisciplinary differences are merely affirmed without being resolved. To follow Althusser, merely affirming differences disarms the critical impulse that has traditionally enabled the discipline of philosophy—and now social epistemology—to force the sciences to see the deep problems that arise, in part, from the fact that they treat each other as "separate but equal."

In catering to readers' interests, an eclectic author would want the mere juxtaposition of methods to establish seemingly common epistemological ground. After all, if you accept the validity of any of the methods used in an eclectic study, you can incorporate the study into your own research. Such a study is thus very "user-friendly" to the normal scientist. By contrast, revolutionary theorists have refused to ignore the problematic status of common epistemological ground. Their answers have typically involved an interpenetration that leaves the constitutive methods or disciplines permanently transformed. New presumptions are instituted for the threshold of epistemic adequacy, which in practice means that new people with new training are needed for the evaluation of knowledge claims.

Consider these uncontroversial cases of successful revolutionary theorizing. After Newton's *Principia Mathematica*, astronomy could no longer just yield accurate predictions, but also had to be physically realizable. After Darwin's *Origin of Species*, no account of life could dispense with either the "nature" or the "nurture" side of the issue. After Marx's *Capital*, no study of the material forces of production would be complete without a study of the social relations of production. This point was rhetorically conceded even by Marx's opponents who then started designating their asocial (i.e. "neoclassical") economics a "formal" science. After Freud's *Interpretation of Dreams*, any psychology based primarily on conscious introspection would be dismissed as at least naive (and at most spurious, à la behaviorism's response to cognitivism).

INTERPENETRATION'S INTERLOPERS

Equipped with her rhetorical skills, the social epistemologist can facilitate revolutionary theorizing in our epistemic institutions. Normally, classical epistemologists and philosophers of science evaluate revolutionary theories in terms of explanatory adequacy. But the social epistemologist wants to unearth the implicit principles by which the revolutionary theorist managed to translate the concerns of several fields into an overarching program of research. In the days of logical positivism, this project would have been seen as involving the design of the "metalanguage" which enables the revolutionary theory to subsume disparate data domains. The social epistemologist, however, regards translation as a bottom–up affair. The concerns of different disciplines are first brought to bear on a particular case—be it historical, experimental, hypothetical, or anecdotal—and then bootstrapped up to higher levels of conceptual synthesis. In that case, the relevant linguistic model is borrowed not from metamathematics, set theory, and symbolic logic, but from the evolution of a trade language, or pidgin, into a community's first language or creole. Over time a creole may become a full-fledged, grammatically independent language. The positivists did not err in thinking that there could be global principles of knowledge production. Rather, they erred in thinking that those principles could be legislated *a priori* from the top–down rather than inferred inductively as inquirers pool their epistemic resources to reconstitute their world.

Let me distance what I have in mind from a related idea, the *trading zone*, most closely associated with the historian of 20th-century physics Peter Galison (1997) and economist Deirdre (née Donald) McCloskey (1991). McCloskey offers the most succinct formulation of the idea, one that goes back to Adam Smith's *Wealth of Nations*. As a society becomes larger and more complex, people realize that they cannot produce everything they need. Consequently, each person specializes in producing a particular good that will attract a large number of customers who will, in exchange, offer goods that the person needs. Thus, one specializes in order to trade. McCloskey believes that this principle applies just as much to the knowledge enterprise as it does to any other market-based activity.

Galison's version of the trading zone draws more directly from the emergence of pidgins mentioned previously. His account has the virtue of being grounded in a highly informed analysis of the terms in which collaborative research has been done in Big Science-style physics. For example, determining the viability of the early nuclear bombs required a way to pool the expertise of pure and applied mathematicians, physicists, industrial chemists, fluid dynamicists, and meteorologists. The pidgin that evolved from this joint effort was the Monte Carlo. The Monte Carlo is a special random number generator designed to simulate stochastic processes too complex to calculate, such as the processes involved in estimating the decay rate of various subatomic particles. Currently, the Monte Carlo is a body of research in its own right, to which practitioners of many disciplines contribute, now long detached from its early nuclear origins. Two questions arise about the models that McCloskey and Galison propose:

1. Are they really the same? In other words, is Galison's history of the Monte Carlo trade language properly seen as a zone for "trading" in McCloskey's strict economic sense?

2. To what extent does the trading-zone idea capture what is or ought to be the case about the way the knowledge enterprise works?

The short answer to (1) is no. McCloskey is talking about an activity in which the goods do not change their identities as they change hands. The anticipated outcome of McCloskey's trading zone is that each person ends up with a greater number and variety of goods than when she began. The process is essentially one of redistribution, not transformation. In contrast, Galison's trading zone is closer to the idea of interpenetration. The Monte Carlo simulation is not just that, say, applied mathematicians learn something about industrial chemistry that they did not previously know. Rather, the interaction produces a knowledge product to which neither had access previously. The Monte Carlo simulation, then, is an emergent property of a network of interdisciplinary transactions. Yet McCloskey's idea perhaps captures the eclecticism of the human sciences in the postmodern era, which, to answer (2), calls its desirability into question.

Interestingly, another economist, Kenneth Boulding (1968: 145-47), already offered some considerations that explain why "Specialize in order to trade!" is not likely to become a norm of today's knowledge enterprises—although it perhaps should be. Boulding points out that to enforce Smith's imperative in the sciences, one would need two institutions. One institution would be functionally equivalent to a common currency (e.g. a methodological standard that enabled the practitioner of any discipline to judge the validity, reliability, and scope of a given knowledge claim). The other would be a kind of advertising agency (e.g. brokers whose job it would be to persuade the practitioners of different disciplines of the mutual relevance of each other's work). Short of these two institutions, the value of knowledge products would continue to accrue by producers' hoarding them (i.e. exerting tight control over their appropriate use) and making it difficult for new producers to enter their markets.

Galison's trading zone entails problems from the standpoint of interpenetration promoted here. He shows how a concrete project in a specific place and time can generate a domain of inquiry whose abstractness enables it to be pursued subsequently in a wide variety of disciplinary contexts. In this way, Galison partly overcomes a limitation in McCloskey's trading zone. He also shows that the trade can have consequences—that is, costs and benefits—that go beyond the producers directly involved in a transaction. But Galison does not consider the *long-term* consequences of pursuing a particular trade language. Not only does a pidgin tend to evolve into an independent language, as in Galison's own Monte Carlo example, but it also tends to do so at the expense of at least one of the languages from which it is composed. Either that or one of the source languages reabsorbs the developed pidgin in a process of "decreolization." In any case, no practical way to arrest language change exists short of segregating entire populations (cf. Aitchison 1981: especially pt. 4).

This empirical point about the evolution of pidgins may carry some normative payoff. The mere invention of new languages does not clarify the knowledge enterprise if old ones are not being displaced concurrently. Because we are ultimately talking about scientists whose energies are distributed over a finite amount of space and time, cartographic metaphors for knowledge prove appropriate. You cannot carve out a new duchy without taking land away from neighboring realms—even if the populations of these realms are steadily growing. The strategy of interpenetrability that I support is, ultimately, a program for rearranging disciplinary boundaries. This strategy presumes that creativity results from moving boundaries around as a result of constructive border engagements.

The social epistemologist imagines the texts of, say, Marx or Freud as such border engagements, the conduct of cross-disciplinary communication by proxy. They implicitly represent the costs and benefits that members of the respective disciplines would incur from the revolutionary interpenetration proposed by the theorist. For example, in the case of *Capital*, the social epistemologist asks what an economist would have to gain by seeing commodity exchange as the means by which money is pursued rather than vice versa, as the classical political economists maintained. Under what circumstances would it be worth the cost? Such questions are answered by examining how the acceptance of Marx's viewpoint would enhance or restrict the economist's jurisdiction vis-à-vis other professional knowledge producers and the lay public. Specifically, we would have to look for audiences that, at the outset, took the judgment of economists seriously (for whatever reason); Marx's potential for affecting those audiences (i.e. his access to the relevant means of communication); and the probable consequences of audiences acting on Marx's proposal. Configuring *Capital*'s audience would undoubtedly do much to facilitate understanding the reception and evolution of Marxism. In this instance, however, the social epistemologist's larger goal is to capture the generalizability of the judgments that Marx made about translating distinct bodies of knowledge into a common framework: What was his strategy for removing interdisciplinary barriers? How did he decide when a key concept in political economy was really bad metaphysics in disguise, and hence replaceable by some suitably Hegelized variant? How did he decide when a Hegelian abstraction failed to touch base with the conception of material reality put forth in classical political economy? Is there anything we can learn from Marx's decisions for future interdisciplinary interpenetrations? So often we marvel at the panoramic sweep of revolutionary thought when in fact we would learn more about revolutionary thinking by examining what was left on the cutting-room floor.

The practice of the social epistemologist differs from that of mainstream hermeneuticians and literary critics in emphasizing the *transferability* of Marx's implicit principles to other potentially revolutionary interdisciplinary settings. However, none of these possibilities can be realized without experimental intervention. One possibility is the writing of new texts that will forge new audiences, whose members will establish the new terms for negotiation, which will convert current differences into strategies for productive collaboration. The dialectical, conventionality, and democratic presumptions that social epistemology derives from STS are meant to render explicit what revolutionary theorists have tacitly supposed about the nature of the knowledge enterprise.

THE PRESSURE POINTS FOR INTERPENETRATION

The kind of pressure point I want is the unit that best epitomizes the Conventionality Presumption. A survey of the various sociological units in which the knowledge enterprise can be analyzed reveals that the most conventional are academic disciplines. Disciplines correspond more exactly to technical languages and university departments than to sets of skills or even distinct subject matters. For example, some skills are common to several disciplines, and other skills may be combined across disciplines with potentially fruitful results. However, the institutional character of disciplinary differences encourages inquirers to forgo these points of contact and to concentrate, instead, on meeting local standards of evaluation. This focus, in turn, perpetuates the misapprehension that disciplines carve up a primary reality, a domain of objects, whereas interdisciplinary research carves up something more derivative. Indeed sometimes in the effort to shore up their autonomy, disciplines will retreat to their signature topics, which are highly stylized (or idealized) versions of the phenomena they purport to study. When political science, for example, wants to demonstrate that it is a science, practitioners retreat from the programmatic aspirations of wanting to explain life in the *polis* and point to the track record of empirical studies on voting behavior, as if the full complexity of political life could be constructed from a concatenation of such studies (J. Nelson 1987). If special steps are not taken to stem this tide of gaining more control over less reality, the situation will not likely remedy itself (Fuller 1988a: Chap. 12). On this basis, we can specify two sets of tensions—*spatial* and *temporal*—that make disciplines especially good pressure points for interpenetration.

In terms of the spatial tension, disciplines are defined by two forces—*the university* and *the profession*—that are largely at odds with one another, although much of the conflict remains at the implicit level of structural contradiction. A university occupies a set of buildings and grounds in (more or less) one place and each discipline a department in that place. The limits of university expansion are dictated by a budget, from which each department draws and to which each contributes. The idea of "budget" should be understood liberally here to include not only operating funds, but also course assignments and space allocation (cf. Stinchcombe 1990: Chap. 9). Of course universities expand, but the interests of particular departments are always subserved to that of the whole. The brutest way of making this point is to recall the overhead costs that researchers receiving government grants must turn over to their universities for general operating purposes. Yet, in more subtle ways, the particularity of departments comes out in how curricular responsibilities are distributed among disciplines in different universities. The intellectual rigor or epistemic merit of a discipline may count for little in determining the corresponding department's fate in the realm of university politics.

Moving from the university department to the professional association, we see that an association has indefinite horizons that stretch across the globe and determine the networks within which practitioners do and share their work. Such an association is more readily identified with technical languages and their ever-expanding publication outlets than with fixed ratios of money, courses, or space. Indeed much of the information explosion that makes the access to pertinent knowledge increasingly difficult may be traced to the fact that most professional associations view the relentless promotion of their activities to be an unmitigated good (cf. Abbott 1988: Chap. 6).

The spatial tension between universities and professions is recognizable in many sociodynamic guises. Sociologists, following Alvin Gouldner (1957), see university versus profession as a case of "local" versus "cosmopolitan" allegiances. Political theorists interested in designing a

"Republic of Science" may see a couple of familiar options for representing the disciplined character of knowledge: the subordination of professional to university interests, on the one hand, and the subordination of university to professional interests, on the other hand. The subordination of professional to university interests is analogous to representation by geographical region, whereby the republic is conceptualized as a self-contained whole divided into departments. The subordination of university interests to professional interests resembles representation by classes, whereby a given republic is simply one site for managing the interplay of universally conflicting class interests. One might expect the teaching-oriented faculty to prefer regional representation, whereas research-oriented ones prefer the more corporatist model.

Perhaps the most suggestive way to present the structural contradiction in disciplined knowledge is in terms of Immanuel Wallerstein's (1991) world-system model. This model attempts to explain the course of modern history as temporary resolutions of the ongoing tension between the proliferation of capitalist markets across the world (most recently in the guise of transnational corporations) and the attempts by nation-states to maintain and consolidate their power base (most recently in terms of high-tech military systems).

How close is the analogy between capital and professional expansion, on the one hand, or national and university consolidation on the other hand? Considering just the first analogy, sociologist Irving Louis Horowitz (1986) argued that transnational publishing houses have been decisive in the proliferation of professional specialties. As publishers make it easier to start journals than to publish books, journals have attracted a larger and more interdisciplinary audience, but in a one-shot fashion that generates much smaller revenues. This phenomenon reflects the traditionally transient character of most interdisciplinary endeavors: Once the specific interdisciplinary project is complete, the parties return to their home disciplines.

Beyond this rather literal case of professionalization as a form of capital expansion, a fruitful site for investigation is intellectual property law. Here the explicit treatment of knowledge as a material, specifically economic good forces professional bodies to think of themselves as companies and universities to think of themselves as states (Fuller 2002b: Chaps 2, 4). As the economic consequences of embodied forms of knowledge become more apparent (especially as the difference between "basic" and "applied" science vanishes), universities are claiming proprietary rights to knowledge products and processes that would otherwise be more naturally identified with the professional skills of its creator. Will there come a point in which a widely distributed technology is more closely associated with the name of a university than of its creator's profession? How literally should we take the nickname of the first patented genetically engineered animal, "The Harvard Mouse"?

In presenting the spatial tension surrounding a discipline, I may have given the impression that, on balance, professional interests are more "progressive" than university-based ones. This notion may be true if one means by progress the tendency to make the academy more permeable to the public. Surely professionalism shares capitalism's motive to reduce indigenous social barriers to increase the mobility of the labor force and the number of paying customers.

Professionalism, left to its own devices, will reify itself into perpetuity. This tendency, one that this book is largely designed to combat, is of professional associations to cast themselves as having special access to distinct realms of being. In this case, the university functions as an effective foil as budgetary constraints naturally curb ontological pretensions. To think that knowledge is best served by maximizing the pool of funds available is a mistake. At most an ample budget will enable all to continue on their current trajectories as they see fit. However, whether the undisturbed course of "normal science" will likely lead to genuine epistemic growth remains an open question. Tight budgets, by contrast, provide an incentive for interpenetration. A discipline is forced to distinguish essential from nonessential aspects of its research program, and to recognize situations where some of those aspects may be more efficiently done in collaboration with, if not turned over to, researchers in other disciplines. Nevertheless, the emancipatory character of budgetary constraints is often obscured because of the bad rhetoric that accompanies talk of "eliminating programs," which forces departments to think that some of them will benefit only at the expense of others. In Chapter 8, I discuss this matter under the rubric of the *principle of epistemic fungibility* (cf. Fuller 2000a: Chap. 8).

A version of the fallacy of division that I dub *The Dean's Razor* superimposes fatalism on this image of fatalities. On this reasoning, because interdisciplinary programs consist of people trained in regular disciplines, nothing essential to the knowledge production process will be lost by eliminating the programs (and keeping the original disciplines) when times are tough. Instead of a razor, a better instrument for the Dean to wield would be what economists call "zero-based budgeting," whereby each discipline would have to make its case for resources from scratch each year.

I would go further. In the university's accounting procedure, faculty members would continue to be treated as university employees. However, faculty would no longer be considered the exclusive properties or representatives of particular departments. Specific departmental affiliations would be negotiated with each academic year. Departments would take on the character of political parties. Departments would push particular (research) programs, probably at the behest of professional associations, but also would allow for some locally generated interdisciplinary alliances, to which faculty will need to be recruited from the available pool each year. In practice, few faculty members would often want to shift departmental affiliation. Nevertheless, such a set up would loosen the grip that professional associations often have on the constitution of departments, as departments would have to come up with ways to attract particular personnel who might also be desired by competing departments within the university.

There are more epistemic consequences to budgetary practices, specifically at a national level. I turn to these after discussing the temporal tension that defines a discipline. A discipline's temporal tension can be analyzed in terms of two countervailing forces: the *prospective* judgment required to legitimate the pursuit of a research program and the *retrospective* judgment that figures in explaining the research program's accomplishments. Our earlier example of the fate of political science makes the point nicely. The original promise of the discipline, repeatedly stressed by its most innovative theorists, was to explain the totality of political life by mechanisms of power, ideology, and the like, whose ontological purchase would cut across existing disciplinary divisions in the social sciences. However, when forced to speak to the field's empirical successes, political scientists fall back on, say, the many studies of voting behavior, which display the virtuoso use of such discipline-specific techniques as cross-national questionnaires, but which make little direct contribution to the larger interdisciplinary project.

Reflected in the tension of these judgments are two sorts of strategies that philosophers have used to account for the "success" of science. *Realists* emphasize prospective judgments often expressed as quests for a desired set of mechanisms or laws able to bring disparate phenomena under a single theory. Realists see the scientific enterprise as continuing indefinitely, anticipate many corrections and even radical reversals of the current knowledge base, and regard the current division of disciplinary labor, at best, as a necessary evil and, sometimes, as a diversion from the path to unity. By contrast, *instrumentalists* stress retrospective judgments of scientific success. These judgments turn on identifying specific empirical regularities that have remained robust in repeated tests under a variety of conditions. These regularities continue to hold up long after theories explaining them have come and gone. Indeed any new theory is born bearing the burden of "saving" these phenomena. Quite unlike the realist, the instrumentalist welcomes the increased division of disciplinary labor as issuing in a finer-grained level of empirical analysis and control.

Many philosophers fail to see that the relative plausibility of realism and instrumentalism depends on the historical perspective on science that one adopts. From the standpoint of the present, the realist is someone who projects an ideal future in which the original promise of her research program is fully realized, whereas the instrumentalist is someone who reconstructs an ideal past in which the actual products of her research turn out to be what she had really wanted all along. Both perspectives are combined in the history of science that all philosophers have told since the advent of positivism. The story goes as follows.

The Greeks started by asking about the nature of the cosmic order. Today we have answers that, in part, complain about the ill-formedness of their original questions and, in part, specify empirical regularities by which we can elicit more "order" (properly redefined) than the Greeks could have ever imagined. In this context, philosophers commonly claim that, insofar as the early Greeks were "seriously" inquiring into the nature of things, they would recognize our accomplishments as substantial steps in that direction. The difference between the Greeks looking forward to us and our looking backward at them reflects an underlying psychodynamic tension. Generally speaking, the history of disciplines presents a spectacle of research programs whose actual products are much more modest, if not actually tangential, than what their original promise would suggest. Still those products would probably not have been generated had inquirers not been motivated by a more comprehensive project. Consequently, one doubts that any of the special sciences would have inspired much initial enthusiasm if its proponents promised merely to produce a set of empirical correlations, the reliability of which could be guaranteed only for highly controlled settings. Such prescience on the proponents' part would have doomed their project at the outset!

The psychodynamics between the realist and instrumentalist orientations may provide a neat explanation for what Hegel and Marx called "the cunning of reason" in history. But from the standpoint of social epistemology, this psychodynamics has more immediately pressing implications. Consider a comprehensive statement by the U.S. government on research funding and evaluation: *Federally Funded Research: Decisions for a Decade* (Chubin 1991). This report, prepared for Congress by the Office of Technology Assessment, drew attention to the fact that research funding increasingly goes to glamorous and expensive "megaprojects," such as the Human Genome Project, the Orbiting Space Station, and the Superconducting Supercollider. These megaprojects promise major breakthroughs across several disciplines and many spinoffs for society at large. However, a megaproject is rarely evaluated by its original lofty goals. Rather, continued support typically depends on a series of solid empirical findings. Although likely insignificant and too limited to justify (in retrospect) the amount of money spent to obtain them, these findings are nevertheless typically couched as "just the start" toward delivering on the original promises. But that does not stop policymakers from being suckered into supporting projects that can only be counted on to deliver diminishing returns on continued investment.

The interactive effects of the policymaker's prospective and retrospective judgments on research make any solution to this problem complicated. One might reasonably argue that even findings of limited scope would not have been made had scientists not aspired to more. And yet such a judgment becomes clearer as it seems less feasible to divert funding from that line of research. This quandary should give us pause.

Our quandary is strikingly characterized by the political theorist Jon Elster (1979, 1983). The realist vision of a megaproject is necessary to "precommit" policymakers to a funding pattern that they would otherwise find very risky. In that sense, realism girds the policymaker against a weakness of the fiscal will. But evaluating the products of a megaproject by the instrumentalist criteria of particular disciplines makes the policymaker prone to develop a version of "sour grapes." Called "sweet lemons," this version offers an exaggerated sense of the project's accomplishments that results from deflating "what can now be seen" as the project's original pretensions, which no one could have been expected to meet. Even so does sour grapes do anything more than pervert precommitment? In whose moral psychology is self-deception an adequate solution to weakness of the will?

My point is not to dump the idea of megaprojects. As yet I do not have a substitute for the motivational role that the realist vision has played in scientific research throughout the ages. However, if delusions of grandeur are unavoidable at the planning stage of a megaproject, it does not follow that such delusions must dominate the evaluation stage. In particular, policymakers should be able to separate out their interest in sustaining the vision that informs the megaproject from whatever interest they might have in supporting the specific research team that first proposed it.

Sour grapes may result from too closely associating the project's potential with the actual research results. Policymakers are then led to indefinitely support the team behind the results regardless of whether that team is now in the best position to take the next step toward realizing the project's full potential. To address this problem, one must carefully distinguish the processes of *rewarding* and *reinforcing* scientists for their work. Scientists who first staked out a megaproject should be rewarded initially, but not indefinitely, for their pioneering work and, ultimately, be expected to move away from their original trajectory. Incentives may be set in place. For example, the terms of grants could be changed to encourage the research team to break up and recombine with members of other teams in other projects. The megaproject's future would then be placed in the hands of another team (or at least a significantly altered version of the original one).

THE TASK AHEAD (AND THE ENEMY WITHIN)

Whether one approves or disapproves of the current state of knowledge production, "science" is often seen as a unitary system, a *universitas* in the original medieval sense, which emphasizes the departmental over the professional character of disciplines. This view suggests that the disciplines see themselves as part of the same team, engaged in relations of mutual respect, if not outright cooperation. In that case, criticisms of the knowledge enterprise should appear as rather generic attacks on academic practices, not as cross-disciplinary skirmishes. Indeed this characterization describes the scope of science evaluation ranging from science policy advisors to popular critics of science. Not since C. P. Snow's famous 1959 Rede Lecture on "two cultures" has anyone systematically raised the social epistemological consequences of disciplines' refusal to engage issues of common and public concern because they suspect *one another's* methods and motives (Snow 1964; cf. Sorell 1991: chap. 5). The rhetoric of interpenetration addresses this most open of secrets in the academy.

The Canadian sociologist Sheldon Goldenberg (1989) performed an invaluable service by surveying both social and natural scientists about their attitudes toward the knowledge enterprise: What books influenced how they think about the pursuit of knowledge? Can work in other disciplines be evaluated by the same standards used to evaluate work in their own? If not, is the difference to be explained by the character of the discipline or of its practitioners? Before proceeding to my own specific interdisciplinary incursions, a sense of the dimensions of the task ahead might be useful for the social epistemologist interested in having disciplines deal with each other in good faith. Goldenberg, thus, enables us to map *the structure of academic contempt.*

Telescoping Goldenberg's data somewhat, we can discern three general attitudes to the knowledge enterprise that are in sharp tension with one another. These attitudes are associated with *natural scientists*, *social scientists*, and *philosophers of science.*

Natural scientists tend to think that something called the scientific method can be applied across the board. However, social scientists typically fail to do so because incompetence, politics, or sloth get in the way. In this portrayal, social scientists suffer from weakness of the will, whereas natural scientists persevere toward the truth.

Not surprisingly, social scientists see the matter much differently. Social scientists portray themselves as reflective, self-critical inquirers who are not so easily fooled by the idea of a unitary scientific method bringing us closer to the truth. Natural scientists appear, in this picture, to be naive and self-deceived, mistaking big grants and political attention for epistemic virtues.

Philosophers of science occupy a curious position in this debate. Social scientists are more likely to read the philosophical literature than natural scientists. Yet social scientists are more likely to disagree with it insofar as philosophers tend to believe that science does indeed work if applied diligently. Therefore, social scientists often regard philosophers as dangerous ideologues who encourage natural scientists in their worst tendencies, whereas philosophers regard the natural scientists as spontaneously vindicating philosophical theses in their daily practices. Philosophers, in this regard, see their job as raising the efficacious aspects of scientific practice to self-consciousness, because scientists tend not to have the broad historical and theoretical sweep needed to distinguish what is essential from what is nonessential to the growth of knowledge. Here philosophers and social scientists agree: Natural scientists are typically ignorant of the principles that govern their practice. The difference between the two camps is that philosophers also tend to believe that science works *despite* that ignorance, as if it were governed by an invisible (philosophical) hand.

The rhetoric needed to perform social epistemology in this environment consists of a two-phase "argumentation practice" (Keith 1995). This practice may be illustrated by the following exchange between "you" and "me."

Before I am likely to be receptive to the idea that I must change my current practices, I must be convinced that you have my best interests at heart. Here the persuasive skills of the Sophist come into play as you try to establish "common ground" with me. The extent of this ground can vary significantly. At one extreme, you may simply need to point out that we are materially interlocked in a common fate, however else our beliefs and values may differ. At the other, you may claim to be giving clearer expression to views that I already hold. In either case, once common ground has been established, I am ready for the second, more Socratic side of the process. I am now mentally (and socially) prepared to have my views criticized without feeling that my status as an equal party to the dialogue is being undermined.

Ideally, this two-step strategy works a Hegelian miracle, the mutual cancellation of the Sophist's manipulative tendencies and Socrates' intellectually coercive ones. For persuasion arises in preparation of an open encounter (and so no spurious agreement results), whereas criticism arises only after the way has been paved for it to be taken seriously (and so no fruitless resistance is generated).

The argumentation practice of classical epistemology is distinguished from that of social epistemology by its elimination of the first phase. Instead of establishing common ground between "you" and "me," the classical epistemologist simply takes common ground for granted. As a result, any failure on my part to respond adequately to the second phase, criticism, is diagnosed as a deep conceptual problem, not as the consequence of a bad rhetorical habit. The problem results from your failure to gauge the assumptions I bring to our exchange prior to your beginning to address me. This diagnosis of classical epistemology is supported by the following rhetorical construction of how the problem of knowledge is currently posed by analytic philosophers.

We must first realize this "modern problem of knowledge" is a technical problem of definition, most of which has already been solved. This awareness explains the narrowness of the debate over the "missing term." All parties to the debate seem to follow (more or less) Plato, Descartes, and Brentano in granting that knowledge is *at least* "justified true belief." The putative advance that has been made since World War II (according to a standard textbook, Chisholm 1977) is to realize that there is a little bit more to the story—but what? A major breakthrough was staged in a three-page article by Edmund Gettier (1963), who independently restated a point that was neglected when Bertrand Russell first raised it 50 years earlier. The breakthrough consisted of some thought experiments designed to isolate the missing term. In brief, the "Gettier Problem" is the possibility that we could have a justified true belief that ends up being mistaken for knowledge because the belief is grounded on a false assumption that is never made explicit.

For example, outside my house two cars are parked; I have a justified true belief that one belongs to John and the other to Mary. When asked for the whereabouts of one of the vehicles, I rightly say, "John's car is outside." Unfortunately, John and Mary traded cars with each other earlier that morning, and so the car that I thought was John's now turns out to be Mary's. If my interlocutor does not ask which car is John's, my ignorance will remain undetected as a false assumption. A tendency exists for people outside of epistemology to dismiss the Gettier Problem as simply more of the idle scholasticism for which they have come to fear and loathe philosophers. However, the unprecedented extent to which Gettier has focused the efforts of epistemologists over the last 40 years testifies to the rhetorical appeal of the problem bearing his name. A brief look at the social dynamics presupposed in the problem should, therefore, reveal something telling about the susceptibility of philosophers to persuasion.

Let us start by taking the Gettier Problem as a purely linguistic transaction or speech act. I am asked two questions by you, my didactic interlocutor. In response to the first, I correctly say that John's car is outside; in response to the second, I incorrectly say that Mary's car is John's. You frame this sequence of questions as occurring in a context that changes sufficiently little to allow you to claim that our second exchange is an attempt at deepening the inquiry begun in the first exchange. As a piece of social dynamics, this "deepening" is simply your ability to persuade me that your evaluation of my second response should be used as a standard against which to judge my first response. Prior to your asking the second question, this point seemed to be unproblematic. But why should I assent to this shifting of the evaluative ground? The reason seems to be that I accept the idea that my second response was implied by my first response and, in that sense, constitutes the deep structure of the first response. As the "essence" of the first response, the second response existed *in potentia* all along. If nothing else, this linguistic transaction defines the social conditions for attributing the possession of a concept to someone: to wit, I have a concept, if you can get me to follow up an initial response with an exchange that you deem appropriate to the situation.

Now this ontologically loaded view of language as replete with hidden essences and deep structures—"concepts," to say the least—recalls the

Socratic rhetoric of *anamnesis*, the recovery of lost memories. However, a social constructivist would argue that reality normally transpires at a coarser grain of analysis than our language is capable of giving it. This analysis implies that if all talk has some purchase on reality, then it is only because talk can bring into being situations and practices that did not exist prior to their appearance in discourse. In terms of the Gettier Problem, why should we suppose that, under normal circumstances, I would have something definite to say about which car is John's prior to your actual request? Moreover, why should we suppose that the answer I give to your request has some retroactive purchase on my answer to your previous query, instead of simply being a new answer to a new question posed in a new context?

The constructivist view that I make up new levels of analysis as my interlocutor demands them of me, and then back-substitute those levels for earlier ones, puts a new spin on the verificationist motto that all conceptual (or linguistic) distinctions should make an empirical (or "real-world") difference. The Gettier Problem shows that the epistemologist, in her role as my interlocutor, can produce empirical differences in my response based on the conceptual distinctions raised in her questions. The epistemologist proves herself a master dialectician. She manufactures a world that I am willing to adopt as my own even at the (unwitting) expense of relinquishing my old one.

If the reader detects perversity in the epistemologist's strategy of manufacturing occasions that enable her talk to acquire a significance that it would not have otherwise, then you have just demonstrated some rhetorical scruples. Joseph Wenzel (1989) observed that a good way to tell the "rhetoricians" from the "dialecticians" (or philosophers) among the Sophists was that the rhetoricians engaged arguments only as part of a general plan to motivate action. Dialecticians argued so as to reach agreement on a proposition. What philosophers have traditionally derided as "mere persuasion" is simply the idea that talk only goes so far toward getting people to act appropriately.

From the standpoint of appropriate action, it may make no difference whether everyone agrees on a given proposition or whether they instead deviate from or even misunderstand each other's point of view. Contrary to what many philosophers continue to believe, rhetoricians realize that consensus is not a prerequisite for collaboration. In fact, consensus may often prove an obstacle if, say, a classical epistemologist has convinced the practitioners of different disciplines that they must agree on all the fundamentals of their inquiry before proceeding on a joint venture. In that case, the convinced parties would have simply allowed the epistemologist to insert her project ahead of their own without increasing the likelihood that theirs will ever be carried out. The *social* epistemologist promises not to make that mistake!

The social epistemologist cannot be expected to resolve incongruous, contempt-breeding, cross-disciplinary perspectives immediately. Yet she may begin by identifying modes of interpenetration appropriate to situations where several disciplines already have common concerns, but no effective rhetoric to articulate those concerns as common. Four such modes are examined in the first part of this book. They vary along two dimensions.

The first dimension concerns the difference between *persuasion* (P) and *dialectic* (D): rhetoric that aims to both minimize the differences between two disciplines and highlight those differences. In terms of a pervasive stereotype, persuasion is the Sophist's art, dialectic the Socratic one. Persuasion seeks common ground, dialectic opposes spurious consensus.

The second dimension concerns the direction of cognitive transference, so to speak. Does a discipline engage in persuasion or dialectic to import ideas from another discipline (I) or to export ideas to that discipline (E)? This distinction corresponds to the two principal functions of metaphor (Greek for "transference") in science, respectively: to test ideas in one domain against those in another ("negative" analogy) and to apply ideas from one domain to another ("positive" analogy).

Together the two dimensions present the following four interpenetrative possibilities. Each possibility is epitomized by a current interdisciplinary exchange in which I have been a participant. In the elaborations that follow in the next four chapters, I do not pretend that these exchanges represent "pure" types. However, for analytical purposes, we may identify four distinct processes, which are interrelated in Figure 2.1

Rhetorical Aim / **Trade Strategy**	PERSUASION (Difference Minimizing)	DIALECTIC (Difference Amplifying)
IMPORT (Negative Analogy)	INCORPORATION	EXCAVATION
EXPORT (Positive Analogy)	SUBLIMATION	REFLEXION

FIG 2.1 The modes of interdisciplinary interpenetration.

(P + I) *Incorporation*: Naturalized epistemologists claim that epistemology can be no better grounded than the most successful sciences. Classical epistemologists counter that naturalists presuppose a standard for successful knowledge practices that is logically prior to, and hence must be grounded independently of, the particular sciences deemed successful. The stakes are captured by the following questions: Is philosophy autonomous from the sciences? Is philosophy's role to support or to criticize the sciences? Have the sciences epistemologically outgrown philosophy? The stalemate that typically characterizes this debate is often diagnosed in terms of the radically different assumptions that the two positions make about the nature of knowledge. However, I see the problem here as being quite the opposite; the two sides have yet to fully disentangle themselves from one another. The naturalist, especially, often shortchanges her position by unwittingly reverting to classicist argument strategies. But after the naturalist has disentangled her position from the classicist's, she needs to address specific classicist objections in naturalistic terms. The naturalist, then, needs to "incorporate" the classicist. Otherwise, the rhetorical impasse *will* continue.

(D + E) *Reflexion*: Disciplinary histories of science tend to suppress the fact that knowledge is *in* the same world that it is *about*. No representation without intervention; no discovery without invention. Yet knowledge is supposed to pertain to the world prior to any "artificial" transformation it may undergo during the process of knowing. The natural sciences can suppress the transformative character of knowledge production more effectively than the social sciences. The discourses of the natural sciences are relatively autonomous from ordinary talk, and their techniques— "laboratories" in the broadest sense—for generating and analyzing phenomena are relatively insulated from the normal course of events. By contrast, because societies have placed some fairly specific practical demands on the social sciences, they have not enjoyed the same autonomy and insulation. The seams of social intervention in social scientific representations are easily seen. When social science tries to explain its own existence in its own terms, the results typically reveal the discipline's blind spots and highlight the artifice with which disciplinary identity is maintained. For example, economics has appeared most authoritative in periods of economic turbulence; economists are hired to dictate policy to a market supposedly governed by an "invisible hand." However, the point of revealing such a paradox by historical "reflexion" (a process both *reflexive* and *reflective*) is to undermine the division of social science into discrete disciplines. Together the social sciences have the investigative apparatus needed to show that the natural sciences, too, are world-transformative enterprises.

(P + E) *Sublimation*: Practitioners of the Sociology of Scientific Knowledge (SSK) and artificial intelligence (AI) should be natural collaborators, bringing complementary modes of analysis to their common interest in computers' cognitive capacities. To date, however, most exchanges are based on mutually stereotyped views that reverberate of earlier debates—"mechanism versus humanism" or "positivism versus holism"—often filtered through the coarse-grained representations of the mass media. As science gets a longer history and becomes more permeable to public concerns, this tendency is likely to spread. The solution explored here is for each side to export ideas that are essential to the other's project. Thus, differences are "sublimated" by showing them to be natural extensions of one another's position. To test empirically the cognitive capacities of a particular computer, the AI researcher needs to see that competence is a social attribution. Conversely, the SSK researcher should realize that the possible success of AI would testify to the constructed character of cognition, such that not even the possession of a human body is deemed necessary for thought. Given the tendency of debates of this sort to amplify into a Manichaean struggle, the presence of the computer as a "boundary object" of significance for both sides turns out to be crucial. A boundary object helps facilitate the sublimation process by forcing each side to map its cosmic concerns onto the same finite piece of matter (Star and Griesemer 1989; cf. McGee 1980, on "ideographs," as pieces of language that perform much the same function).

(D + I) *Excavation*: After the initial promise of studying science historically, the history and philosophy of science (HPS) appears to be at a conceptual standstill. As a result, HPS is not prepared to leap beyond the disciplinary boundaries of history and philosophy to STS. I diagnose this inertia as a failure, especially on the part of historians, to explicitly discuss the assumptions they make about theory and method. These assumptions are often at odds with what the social sciences have to say about these matters. Especially suspect are the assumptions about the human cognitive condition that inform historical narratives, even narratives that avowedly draw from cognitive psychology. To "excavate" these assumptions is to articulate long-suppressed differences between humanistic and social scientific approaches to inquiry. A willingness on the part of humanists to hold their research accountable to the standards of social science would tend to break down the remaining disciplinary barriers that inhibit HPS's passage to STS. Moreover, the historian could use the social scientists' own methods to keep them scrupulous to historical detail. I suggest that some of the normative issues that have made philosophers impatient with historians could be better addressed by experimental social psychology, and perhaps even the "case-study" methodology traditionally championed in law and business schools.

HERE I STAND

Let me state briefly my own position in each interpenetration. In the case of Incorporation, I am a staunch naturalist who nevertheless believes that the letter of classical epistemology has compromised the naturalist's spirit. In the case of Reflexion, I am a staunch advocate of social science. I also believe that the field's fragmentation into disciplines has undermined the social scientist's capacity for critiquing and reconstructing the knowledge system. In the case of Sublimation, I am a staunch supporter of the sociology of scientific knowledge who agrees that yet again philosophers have injected false consciousness into another community of unsuspecting scientists—namely, researchers in artificial intelligence. But I also believe that the sociologists are duplicitous when they make *a priori* arguments against the inclusion of computers as members of our epistemic communities. Finally, in the case of Excavation, I want to facilitate the transition from HPS to STS. Still I believe one is naive to think that this transition can succeed if both parties simply adopt new theories and look at new data. A new social formation is needed.

THOUGHT QUESTIONS

☙ Does interdisciplinarity signify a failure of expertise? Does the ideology of interdisciplinarity advocate an end to expertise?

☙ What does the process of interdisciplinary interpenetration look like on Fuller's model? What role would rhetoric play in this process? Does Fuller's notion of interdisciplinary interpenetration necessarily lead to the abandonment of traditional disciplines?

☙ What is the reason for the existence of modern academic disciplines? As currently configured, are academic disciplines the best means to pursue and to disseminate knowledge? By what other means could universities organize and pursue knowledge?

☙ How do disciplines present themselves, rhetorically, as making progress?

☙ According to Fuller, the "most vital epistemic function for rhetoric in the academy" is to aid in the renegotiation of disciplinary boundaries. How would the process of negotiation be structured? Who would be the principals? Could the renegotiation of disciplines begin absent social and institutional circumstances? What circumstances would need to be in place to promote interdisciplinary negotiation? What examples can you provide of this process?

☙ What is knowledge policy? Whom does Fuller identify as having a strategic role in divining knowledge policy? Consequently, do you agree with Fuller's description of how academic society, generally, and academic labor, specifically, function?

☙ What is the "rhetoric of interpenetrability"? What is the goal of this rhetoric regarding disciplines? What difficulties might a rhetorical theory based on Friedrich Engels' laws of dialectics and Karl Marx's concept of structural contradiction face in negotiating knowledge policy? What special problems does tolerance pose to the process of negotiating knowledge policy? What special problems does pluralism pose to the process of negotiating knowledge policy? Why is confrontation necessary in determining knowledge policy?

☙ Describe the point at which disciplines interpenetrate. On what bases might disciplines evaluate on another? What does Fuller's example of the negotiation between history and psychology suggest regarding the role of the rhetorician?

☙ What is the pluralist form of interdisciplinarity? What problems does it entail? How might the method of triangulation serve or hinder the process of interdisciplinary negotiation?

☙ What are the differences among the approaches to "revolutionary theorizing" between social epistemologists and classical rhetoricians and philosophers? In what rhetorical tradition do social epistemologists find themselves?

☙ Fuller argues that the social epistemologist regards the process of translating disciplinary differences into an "overarching program of research" as a "bottom–up affair." To what philosophical tradition does this approach react? How does the "bottom–up" approach square with Fuller's conception of academic labor? How does the social epistemologist's approach to interdisciplinary negotiation differ from the "trading zone"? How do Galison's and McCloskey's concepts of the trading zone differ? How could Marxist principles lend creative solutions to the process of disciplinary negotiation?

☙ How are disciplines defined spatially and temporally? In what ways is science different from or related to other disciplines in using resources to produce and distribute knowledge and information? Are philosophical conceptions of disciplines contingent on assumptions about the spatial and temporal requirements of knowledge production? What are the differences between realists and instrumentalists in their perspective on knowledge policymaking?

☙ What argumentation practices are necessary to sustain the project of social epistemology? Are these the same practices needed to help renegotiate disciplinary boundaries? What is the difference between persuasion and dialectic?

PART II

INTERPENETRATION AT WORK

3

Incorporation, or Epistemology Emergent

When people query the point of doing philosophy, they are engaged in "metaphilosophy." Traditionally, philosophy has staked its ground in relation to religion. But for the last 100 years or so, science has provided the relevant frame of reference. Out of the modern relationship of philosophy and science the following tension arises. On the one hand, is the philosopher engaged in an enterprise that is legitimated on grounds quite apart from science, which, once grounded, can then pass judgment on the legitimacy of science? Yes, says the *classicist*. Or, on the other hand, is the philosopher really only a "scientist of science," whose own legitimacy is only as good as that of the scientists she studies? To this the *naturalist* assents. By all accounts, these arguments have gone nowhere except to secure income for those pursuing them. Is this yet another proof of the sterility of philosophical dispute? Resisting this counsel of despair, I explore the possibility the self-styled progressive in the dispute, the naturalist, has yet to make a clean break with the classicist's position. More specifically, the naturalist has failed to abide by a simple procedural rule of argument.

TYCHO ON THE RUN

Tycho's Doctrine: Separate but (Not Quite) Equal

Let us begin with the position that will be criticized: *Tychonic Naturalism*. The Tychonic Naturalist holds that, in formulating the metatheory of her activity, the epistemologist can do no better than strike a balance between the classicist and the naturalist. Because the standard moves made by the two positions cannot be transcended, mutual accommodation is the best we can do. Such is the spirit of the 16th-century astronomer Tycho Brahe, who continued Ptolemy's practice of treating the earth as the static center around which the sun moved, but then followed Copernicus in having the other planets circle the sun.

Consider the case of Alvin Goldman's *Epistemology and Cognition* (1986), a veritable summa of naturalized epistemology. The calling card of Goldman's Tychonism is the book's two-part structure. The first, "Ptolemaic" part is a largely *a priori* conceptual analysis of the defining features of the epistemic process. The second, "Copernican" part is devoted to empirically isolating the cognitive mechanisms that instantiate those features. Thus, after defining knowledge as the reliable production of true beliefs, Goldman proceeds to look in the mind for some reliable mechanisms—all along presuming they exist to be found.

Goldman's commitment to naturalism is a clearly mitigated one. In particular, he does not take seriously the possibility that *nothing* in our psychological makeup conforms to the concept of a reliable-true-belief-forming mechanism. Indeed Goldman frequently overrules a psychologist's claim to have shown that a defining feature of knowledge is empirically unrealizable. He does this by challenging the "intelligibility" of humans acting irrationally most of the time or holding mostly false beliefs. These examples illustrate just two of the epistemologically inauspicious conclusions psychologists have been prone to draw (especially 1986: 305-23).

Goldman believes that his naturalism binds him to a version of Kant's "ought implies can" principle. On this principle, individual human beings must be able to follow the norms of rationality if the norms are truly to have force. This commitment seemingly motivates Goldman's attempts to discredit experimental demonstrations of irrational judgment in individuals. Still why should a naturalist tie norms to the abilities of *individuals*? For example, say a popular philosophical model of rationality consistently picks the better theory to test. However, individuals are unable to follow the logic prescribed by the theorem. Then, perhaps, the theorem is suited for some other sort of being. In another instance, the model might govern a digital computer's selection of theories and the resulting theorem used to characterize an emergent property of a certain kind of social interaction. An example of characterizing an emergent property might be Popper's falsification principle. In this case, each scientist was to act as her own conjecturer and her neighbor's refuter. Yet in any case, the scope of the theorem's governance is, as the naturalist would have it, a matter for empirical inquiry. Often forgotten is that Kant first proposed "ought implies can" as an argument for the existence of a faculty that enables us to be moral agents. Therefore, a norm postulates a (perhaps yet to be discovered) realm of beings that are governed by it.

In Goldman's case, the Tychonic spirit is moved by an interest in keeping the disciplinary boundary between philosophy and psychology intact—itself a rather peculiar interest for a naturalist to have. Why not, instead, take the empirical unrealizability of a piece of conceptual analysis to suggest that the analysis may be off the mark? Naturalists typically advertise their sensitivity to the historical character of knowledge production. Nevertheless, in respecting the disciplinary boundary separating philosophy from the empirical sciences, naturalists act as if it delineated a historically invariant, "real" difference in subject matter. To be truly naturalistic, however, one must realize that the disciplinary boundary separating psychology and philosophy has been contingently shaped over the course of history—and, even in our own day, across different nations. I have called this application of naturalism to the naturalist's own argumentation *reflexive naturalism* (Fuller 1992b). Reflexive naturalists would not allow the current disciplinary divide between philosophy and psychology to be automatically interpreted as indicative of a real difference in subject matter.

On the surface, reflexive naturalism may sound like a radical suggestion. Yet reflexive naturalism is simply the sort of consideration that has traditionally led both positivists and social constructivists to be skeptical about drawing ontological conclusions from the division of cognitive labor in science. My point here is *not* that the naturalist ought to distrust any hard distinction that might be drawn between the tasks of epistemology and cognitive psychology. Rather, she should simply distrust any proposed distinction based on "conceptual" considerations, which abstract from the changing historical character of the two disciplines. Instead the naturalist should roll up her sleeves and design some epistemologically relevant psychology experiments, argue with the psychologists about methodology, and *then* decide where (or whether) the boundary between the two disciplines should be drawn (cf. Heyes 1989).

Naturalists could take a lesson from the logical positivists. Recognizing the completely conventional character of disciplinary boundaries, the positivists transgressed them whenever it seemed necessary, as in the service of "unified science" (cf. Zolo 1989: Chap. 5). Still, just as the naturalist cannot conceptually ground the separation of epistemology from psychology, she cannot, simply by argument, empirically eliminate epistemology in favor of psychology—a move commonly found in such radical naturalists as Willard Quine, Donald Campbell, Richard Rorty, Paul Churchland, and Ronald Giere. Both moves neglect the historical dimension of the epistemic enterprise.

I agree with these radicals that the contemporary pursuit of classical epistemology is best seen as the artificial continuation of Descartes' and Locke's 17th-century psychological theorizing. Yet *identifying* the errors fostered by such theorizing is not quite the same as eliminating the practice that continues to grant legitimacy to those errors. For as we have seen in Chapter 1, to be truly naturalistic one must start with things as they already are (i.e. *in medias res*) and work from there. My fellow radicals often make it seem as though the replacement of epistemology by psychology would occur "spontaneously" once people realized that the latter was the scientific successor of the former (i.e. epistemologists would simply start doing psychology or face extinction). On the contrary, I hold that this notion overintellectualizes the matter, as if one "good argument" could solve what is essentially a sociological problem. In a sense, my radical friends need to naturalize their conception of argument to make room for *burden of proof*. As the rhetorical analogue of institutional inertia, burden of proof enables epistemologists to proceed unperturbed by the findings of empirical psychology (Fuller 1988a: 99-116; 1989: 68-69). The eliminativist essentially has the rhetorical disadvantage of trying to persuade her audience to make a career shift! (In the last chapter, I return to the rhetorical implications of burden of proof.)

To make psychology rhetorically more palatable to epistemologists is to alter psychology itself. After all, psychology's character comes largely by defining itself in relation to neighboring academic disciplines such as philosophy and sociology. This relationship has led psychology to strategically adopt and oppose developments in those other disciplines. For example, psychology has generally adopted the methodological individualism of the moral sciences and the positivism of 19th-century experimental physics. Yet once neighboring disciplines are transformed, psychology's need to continue in its usual manner is unclear. In this regard, *reductionism* is a better model for the naturalized epistemologist than *eliminativism*. Traditionally reductionism has had a prescriptive thrust—namely, a call for, say, psychology and neuroscience to develop translation manuals between their two theoretical languages. In developing such manuals, so the idea goes, the two disciplines will realize that they are talking about the same thing to such an extent that they can come to agree on a common tongue for future joint pursuits. In short, then, reductionism may be seen as primarily a program to synchronize the activities of conceptually neighboring disciplines by forcing them to communicate with each other. Such a strategy was certainly behind the logical positivist ideal of "unified science."

Tycho Goes Social—Too Little, Too Early

Reflexive naturalism is the proposal that the results of psychology should be applied reflexively to both psychologists and epistemologists. The result defines the line of joint inquiry that the two currently distinct groups will subsequently pursue. This interpenetration of psychology (as well as the other social sciences) and epistemology, in turn, enables a transformation of both into a single project. This rhetorical proposal aims to make "epistemology emergent."

As we have seen, evidence for the current lack of interpenetration of epistemology and psychology comes in two forms: (1) In matters of philosophical reasoning (e.g. the reliability of introspectively based conceptual analysis), psychology does not seem to have progressed beyond the 17th century. Still state-of-the-art psychology is used to identify the appropriate knowledge-producing mechanisms specified by philosophical reasoning. (2) Philosophers use psychological findings more often to exemplify conclusions reached by "philosophical" means than to use findings as evidence to overturn such conclusions.

The call to interpenetration does *not* entail that either epistemology or psychology has final epistemic authority over *its own* field of inquiry. Philosophical naturalists typically accord too much local sovereignty to the disciplines on which they rely. This attitude only serves to earn them the scorn of classical epistemologists, who deem the naturalists slavish followers of scientific fashion bereft of all philosophical scruples. The classicist's objection is avoided by going "meta" and considering the consequences of applying psychology to the psychologists— something that they would typically not do. Perhaps the most important consequence of the reflexive application of psychology is to cast aspersions on the idea that a sharp distinction can be drawn between *individual* psychology (and epistemology) and *social* psychology (and epistemology).

In one sense, epistemologists like Goldman hardly draw any distinction between individual and social psychology—and their counterparts in epistemology. Epistemologists are ultimately concerned only with functionally equivalent individuals. Accordingly, both contemporary American experimental psychology and analytic epistemology are committed to methodological individualism even in their accounts of the social. Thus, a "social psychology" or a "social epistemology" is "social" only in the sense that one is studying the social knowledge of individuals, or, in more down-to-earth terms, what people think about each other. Moreover, social knowledge is assumed to be uniform across individuals as if no epistemologically salient differences in social knowledge could arise from differences in, say, the class background or role expectation of individuals. Social epistemological policy advice, apparently, should be the same for everyone.

I would argue that the first principle of a truly socialized epistemology is that everyone should *not* be given the same epistemic advice—or be expected to take the same advice in the same way. For, if one takes seriously the idea that knowledge is a social product (i.e. the product of a certain pattern of human interaction) then one no longer needs to think about individuals as having common cognitive powers and interests. Rather, one must consider that individuals' different powers and interests function together to collectively produce a form of knowledge for the whole community, even though no single individual could be expected to have mastered all of its parts.

Indeed the collective identity arising from disciplinary knowledge suggests a more moral, perhaps even an emotional, commitment by scientists to accept joint responsibility for the work of any of its members. When philosophers talk about the distinctive products of science—theories (Hempel), paradigms (Kuhn), research programs (Lakatos), research traditions (Laudan) – a moment's reflection reveals that they refer to epistemic units that could not possibly be stored in any single individual's head or, arguably, even in a single book that an individual could be expected to use with facility. Instead these products are distributed in parts across an entire scientific community. For example, for a subfield of physics to become part of the physics knowledge base, many theorists, experimenters, and technicians need to be involved in research. However, no one would claim to understand all the inferential chains that forge the subfield.

The problem here is not simply that a physicist's memory is not large enough to store all the knowledge produced by her specialty. The problem runs deeper. Assuming that the physicist could chunk the knowledge of her field into a manageable size, she would still be unreliable in drawing the relevant deductive and inductive (i.e. probabilistic) inferences that together turn this information into a cognitive map of some domain of inquiry. To appreciate the significance of this point, consider that the *smallest* epistemic unit that philosophers have typically found distinctive about science—the theory. The theory is epitomized by a formalized version of Newtonian mechanics. Physicists, in this instance, are expected to calculate indenumerably many deductive inferences from factual premises about the motions of the planets in conjunction with universal physical principles. This prospect places an impossible computational load on the physicist. Therefore, what physicists must share are little more than bonds of mutual trust and a self-identity as, say, "solid state physicists."

But how does the reflexive application of psychology encourage this turn to the social? The first step is to generalize the main point of the previous paragraph: All of our concepts are heuristics—that is, fallible shortcuts in reasoning that are biased toward our interests. For example, the main use to which we put a concept of causation in everyday life is to coordinate our actions in relation to other things in the immediate environment. The things deemed "causal" are the ones whose movements are likely to make some difference to what we decide to do, and these are typically the things that most readily catch our eye (Kahneman 1973). We ordinarily have no need to speculate about whether there is anything more to the object's motion than the history of its interactions with the environment, or whether the object's motion is synchronized with the motions of other visually occluded or distant objects. However, these speculations become relevant once we start wondering whether what we see is all that there is (i.e. whether individual objects are the right units for thinking systematically about reality).

Our causation heuristic is ill-suited for satisfying a metaphysical impulse of this sort. Nevertheless, without an appropriate theory to act as corrective, these notions function as a default theory that biases our thinking toward treating individuals who move freely in the visual field as having some kind of metaphysical ultimacy. For example, naturalized studies of science have tended to see the scientist as an agent who makes things happen in the world by exercising her intrinsic powers (Giere 1988). This view directly plays to our cognitive biases. We convert the palpable fact that scientists freely move about the lab into a sign that they are self-moving, or autonomous, beings who can be held personally responsible for their actions. Of course, no one assents to this view in quite so bald a form. But we naturally fall back on it when evaluating science: If a discovery is made, a scientist is credited; if fraud is committed, a scientist is blamed. We may nod sagely that these events are, "strictly speaking," the systemic effects of class, status, and power acting "through" the scientists. Yet we still intuitively believe that we must see something move something else before the first thing is called a cause: Scientists move apparatus, but as far as the eye can see, class struggle doesn't move much of anything in the lab.

As our understanding of science proceeds fairly smoothly with this bias in place, the wiles of a more reflexive naturalist, the social epistemologist, will be needed to throw a spanner in the works. For example, most people cannot see the need to postulate power differences to explain a single transaction that might be observed between two people. They ask: Why not simply invoke the intentions of the specific individuals involved, and avoid reference altogether to an occult entity like power? The plausibility of power as an explanatory principle grows with an awareness that many such transactions occur in many places and times that are systematically interconnected by counterfactually realizable situations (e.g. if one party does not conform, then the other party can impose force). The entirety of events transcends the intentions either of any of the constitutive individuals or of any given observer of a particular transaction. But all that is just to say that one has to stop using the limits of one's visual field as the intuitive measure—or metaphor, if you will—of explanatory adequacy for social action (cf. Campbell 1974). A better image would be to regard the scientist as a body whose movements are the result of a variety of forces that have been imparted in the course of its interaction with other such bodies. Although we can see no strings attached to a scientist, we can see in her behavior the marks left from her interactions with various teachers, colleagues, and so on. One is tempted to say that what makes each scientist distinct is simply the uniqueness of her history of interactions.

Tycho Gets Blindsided by the Rear Guard

I have argued here and elsewhere (Fuller 1988a, 1989) that the best opening gambit to show the essentially social nature of knowledge is to devalue the cognitive powers of the individual. Indeed taking a cue from Karl Popper, I claim that the ever restless (or "progressive") character of our epistemic pursuits would be undermotivated if, as individuals, we were not innately endowed with trenchantly false ideas that require long-term systematic effort to overcome. In short, while science may not require human beings for its conduct, it does require beings whose cognitive biases and limitations are comparable to those of humans, and who then see in science a way to collectively transcend their finitude as individuals.

These considerations are unlikely to move our next Tychonist, Rom Harré. Harré, the Oxford philosopher most closely associated with discursive social psychology, has faith in the cognitive competence of humans that runs deeper than Goldman's. For just as Tycho was moved, in part, by a respect for commonsense intuitions about the stationary character of the earth, so too Harré is moved by a respect for the richness of ordinary usage of folk psychological concepts—a richness that is typically overlooked by the flagship discipline of naturalism, experimental psychology (Harré and Secord 1979; cf. Greenwood 1989, for a sophisticated elaboration of the Harrean position, in the face of defenses of experimentalism). Experimental psychology seems to be the heir apparent to naturalized epistemology because its typical unit of analysis—the interface between an individual organism and its environment—most closely resembles the setting in which the problem of knowledge of the external world was classically posed by Descartes (Quine 1985). Here one might mention that the issue is unclear whether experimental psychology would loom so large for naturalists who focused more on modeling the problems of theory choice and conceptual change that have typified debates in the philosophy of science. In that case, even the logical positivist Hans Reichenbach (1938: 3-16) realized, the sociology of knowledge would be a more suitable "naturalization."

According to Harré and Secord (1979), "aggression" is not the convergence of a couple of empirical indicators in a laboratory subject. Aggression is a deep-seated human disposition that may be elicited in a variety of ways under a variety of circumstances. The sum of these circumstances may be explicated by conceptually analyzing ordinary language used in the so-called *natural settings* of everyday life. Since his work over the past 20 years has been devoted to articulating the "ethogenic" paradigm in social psychology, Harré might seem more inclined to naturalism than Goldman. Still Goldman argues largely with classical epistemologists and does not seem to have ever altered any of his fundamental tenets in light of psychological evidence. Further, Goldman (1989) has modified aspects of his reliabilism in light of *conceptual* considerations, much like the sequence of revisions that Noam Chomsky has made to his theory of generative grammar. Unfortunately, Chomsky's stress on the conceptual at the expense of the empirical also explains "the rise (and surprisingly rapid fall) of psycholinguistics" (Reber 1987).

However, just as Goldman's naturalism is mitigated, so too is his antinaturalism: He cites particular experiments when they serve his purpose, he discredits other experiments when they do not, but he does not call into question the appropriateness of the experimental method to the empirical study of human beings. The explanation might simply be (so says Goldman) that not everything the psychologist does is relevant to the normative mission of epistemology. By contrast, Harré wants to recolonize psychology for the version of classicism represented by ordinary language philosophy. This move means that the deliveries of conceptual analysis are the primary data of psychology, to which empirical research must conform accordingly. In fact, this methodological dictum is the first that Harré and Secord (1979) lay down. Here is Harré's justification:

> The science of mechanics made rapid advances after careful and detailed analysis of the concept of "quantity of motion" had revealed the need for a distinction between "momentum" and "kinetic energy." These conceptual distinctions did not emerge from experimental studies. They were arrived at by analysis. Once achieved they facilitated a more sophisticated and powerful empirical science of bodies in motion. (Harré 1989: 439)

In practice, Harré abandons laboratory experiment for the sort of "on-site" ethnography commonly pursued by anthropologists. Ethnography, seemingly, provides the interpretive freedom needed to plumb the putative depths of human expression codified by ordinary language.

I do not mean here to cast aspersions on ethnographic inquiry's contributions to the human sciences. Such inquiry may feature in ways that would cater to the experimental proclivities of the more robust naturalist—not to mention the STS practitioner (as becomes clear at the end of this chapter). In particular, I have in mind versions of ethnomethodology inspired by Harold Garfinkel's work, such as "experiments in trust," in which the inquirer tests the extent of normative constraint by disrupting the "naturalness" of the settings in which a norm ordinarily operates. For naturalists, experimental intervention is a precondition for the norm to be represented (cf. Turner 1975). By contrast, the use to which Harré puts ethnography—namely, as exemplifying of empirically unrevisable folk psychological concepts—removes that method from the arena of hypothesis testing and, hence, from proper naturalistic inquiry. This point is worth dwelling on briefly because I will draw on it again in the course of overcoming the canonical form of the classicist–naturalist exchange.

Although philosophers commonly say that naturalists are devotees of the experimental method, whereas classicists prefer conceptual analysis, this way of putting things is misleading. From this rendering, naturalists and classicists appear to be engaged in mutually exclusive activities, as epitomized by the typical locations in which these activities occur—the laboratory and the lounge chair. What happens in the laboratory is supposedly *a posteriori*, whereas what transpires in the lounge chair is *a priori*. However, this characterization only revives the dogmas of empiricism so as to make classicism and naturalism seem more irreconcilable than they need be. Luckily, the history of science is a ready source of counterexamples to stereotypes that hark back to a world well lost (i.e. before Quine 1953).

On the one hand, experiments have been conducted in the name of the *a priori*. That is, experiments have been used as a means to provide concrete demonstration of truths derived by conceptual means. This attitude toward experiment was typical of those 17th-century thinkers whom we now call "philosophers," such as Descartes and Hobbes (Shapin and Schaffer 1985). In contrast, Boyle and Newton are usually credited with turning scientific opinion toward experiment as a genuine and even preferred source of knowledge, rather than as an illustrative device of some incidental heuristic value (Hall 1963). From that standpoint, Harré's mobilization of the ethnographic method is a rearguard action.

On the other hand, conceptual analysis has been used to arrive at eminently falsifiable empirical hypotheses, and thereby forward the cause of the *a posteriori*. The real difference between the classicist's "apriorism" and the naturalist's "aposteriorism" lies not in the kinds of activities each pursues, but rather in the degree to which each is inclined to revise her claims in light of unintended or unexpected outcomes of those activities. In that case, Imre Lakatos (1979) is right that there lurks a classical epistemologist in the metaphysical hard core of every scientific research program. What typically makes conceptual analysis the mark of the classicist is the control that the analyst has over her introspections. Like Descartes, then, a certain private illumination ultimately determines that the analysis can be revised no further. However, if Descartes had believed that he needed a second, potentially overriding, opinion to evaluate his introspections, then he would have been doing conceptual analysis in a naturalistic vein. In fact, as we will see, a discipline exists that systematically offers such second opinions, ethnosemantics (cf. Amundson 1982; Lakoff 1987).

Tycho Sans Class(icism)

Let us now return to our naturalistic rejoinder to Harré: What would it mean to employ ethnography "naturalistically" to test a particular analysis of folk psychological concepts? For example, how might an ethnosemanticist "analyze" the folk concept of *aggression*? She would proceed by surveying the usage of aggression in a particular language—say, American English—and quickly observe the variety of contexts in which it arises. To these particular facts about the word's usage, she would add general empirical facts about natural languages, especially facts pertaining to words used in contexts too numerous to be monitored for mutual compatibility and propriety. Given this information, some ethnosemanticists might see in Harré's approach a more "ecologically valid" ethnosemantics, in which polysemy is taken as an indicator of some measure of conceptual depth (e.g. Lakoff 1987). However, most ethnosemanticists would probably conclude that the deep-seated disposition that Harré sees lurking beneath the multifarious character of aggression talk is a mirage: to wit, homonymic drift passing for synonymic stability (cf. Fuller 1988a: 117-38).

Notice the anti-Tychonic character of this rejoinder. Harré presumes that ordinary agents already have a reasonably reliable introspective understanding of their own minds. In interesting counterpoint, Goldman (1986: 66) restricts such self-understanding to the judgments that philosophers make in "reflective equilibrium." (L. J. Cohen 1986 provides an extended defense of this point in the aid of establishing a distinct subject matter for analytic philosophy.) In any case, Harré seemingly believes we are entitled to his presumption because he further presumes that self-knowledge is essential to our routinely successful encounters with each other and the world. Our tendency to associate polysemous words with conceptual depth is taken to be a good starting point for Harré's ethogenic inquiry. In contrast, our ethnosemanticist presumes nothing of the sort (cf. Fuller 1988a: 139-62, for a defense of uncharitable interpretive principles). Yes, natural language speakers provide a privileged database for the study of word usage, but their second-order musings do not provide a privileged database for the interpretation of those data. The second-order musings—what I make of the multifariousness of my aggression talk—is just more first-order data for the ethnosemanticist to study. Why? An individual, from a naturalistic standpoint, is a biased source of information about their own activity given the disproportionate amount of data they record about themselves (usually for their own purposes) vis-à-vis the amount of data they record about other relevantly similar individuals. Consequently, under ordinary circumstances, an individual will have an inadequate basis for judging the representativeness of their self-reports. This bias is manifested in people's tendency to ignore what probability theorists call the "base rates" of some phenomenon's occurrence (i.e. the likelihood that something will happen given its track record) when making predictions (Kahneman et al. 1982). (For the sake of argument, I have ignored the point that much of the bias in the data that an individual records may be attributed simply to flaws in the data recording device itself —i.e., memory).

The ethnosemanticist has the interpretive advantage of the third-person perspective, which enables her to compare that individual's utterances with those of others. Of course this point applies to the naturalized interpreter's own behavior. It is also best studied from the third-person perspective. One is reminded of the joke about two behaviorists greeting each other. One says to the other, "You're OK. How am I?" The ultimate trick, however, for any naturalized interpreter is to determine exactly what the data provided by a speaker's utterances are best taken as *evidence for* patterns of neural firing, sentences in the language of thought, socially constructed contexts, or objective states of affairs? An entire branch of experimental psychology is devoted to interpreting "verbal reports as data." Ericsson and Simon (1984) see the interpretation of verbal reports as a matter of identifying the sort of data that is regularly registered by human speech or, in more behavioral terms, a matter of determining the factors that control verbal emissions. In this respect, the project is in the spirit of the "radical translation episode" in Quine (1960). Yet Ericsson and Simon do not presume that their interpretation is constrained by the need to make most of a speaker's utterances turn out to assert truths or even reasonable beliefs.

The Tychonic Naturalist sees only three possible outcomes to our debate: The classicist wins, the naturalist wins, or a mutual accommodation, enabling the peaceful coexistence of both sides, is reached. In the case of Goldman and Harré, the Tychonist favors the third option. This result is achieved roughly by gauging how much naturalism a classical epistemology can absorb and still be recognizably philosophical. Missing, however, is the possible crucial outcome that the two sides may be transformed in the course of debate so that each incorporates in its own terms the issues raised by the other side. Such a tricky possibility requires that we briefly resurrect the ghost (or *Geist*, I should say!) of Georg Wilhelm Friedrich Hegel.

HEGEL TO THE RESCUE

A Matter of Principle

Naturalized epistemologists typically find themselves at a dialectical disadvantage. Part of this disadvantage may be explained in terms of how the burden of proof is distributed in the classicism–naturalism debate. After all the naturalist is the latecomer. What does not follow, if the naturalist must bear the burden of proof, is that she must confine herself to the types of arguments used by her classicist opponent. Yet as dramatized in the following debate the naturalist often succumbs to appeals to conceptual analysis, transcendental arguments, and commonsense intuitions. Confined to these sorts of arguments, she is no match for the expert classicist.

The classicist rarely slips into naturalistic appeals for her own position. Nevertheless, the classicist does wax naturalistic when she defends the mission of providing foundations for knowledge in terms of its longevity—as if the fact that people have associated epistemology with the classical version of the project for over 350 years somehow contributes to the *conceptual* well-foundedness of the enterprise. In short, the classicism–naturalism debate would benefit from a certain methodological consistency. Naturalists should argue naturalistically and classicists classically. They should neither be forced to argue in ways that contradict their metaphilosophic principles nor be allowed to tailor their opponents' metaphilosophic principles for their own purposes. Thus, I propose the following procedural rule:

> *The Principle of Nonopportunism*: When either defending her own position or attacking her opponent's, the philosopher must employ only the sort of arguments that her own position licenses. She cannot avail herself of arguments that her opponent would accept, but that she herself would not.

There are *constitutive* and *regulative* versions of this principle. The constitutive version says that nonopportunism enters into the very construction of the position taken in debate. If I want to hold my opponent accountable to certain standards, then I had better be sure that I can be held accountable to them myself. By contrast, the regulative version of the principle presumes that the two positions were constructed independently of each other prior to the debate. In that case, nonopportunism circumscribes the field of appropriate engagement between the two positions. Given what we have seen in the first two chapters as the conventional character of disciplinary boundaries, the centrality of interpenetrative rhetoric, and, most of all, my "normative constructivism," I generally prefer the constitutive version of nonopportunism.

The "opportunists" who violate the principle of nonopportunism are stereotyped sophists, classical skeptics, and sometimes reflexive practitioners of STS (more about which in Chapter 9). All are prone to throw their interlocutor's favorite form of argument back in her face without feeling compelled to engage that form themselves. An example of opportunism would be for a philosopher to cite the empirically based disagreements between various schools of psychology as an argument against endorsing the findings of any of the schools, when in fact the philosopher herself does not believe, as a matter of principle, that the data could resolve such theoretical disputes. Metaphysically speaking, the opportunists follow in the footsteps of the Sophist Gorgias. Like Gorgias, opportunists share a fundamental mistrust of communication as a process that can dissolve the incommensurable presumptions that invariably separate people in the first moment of encounter. Heirs to Gorgias are opportunists because they believe that if common ground is not present *a priori*, then it cannot be forged *a posteriori*. Given this Hobson's choice, Gorgias' most dogged opponents—from Socrates to Habermas— have argued that common ground is present a prior, either in a realm of universally communicable forms or in a set of transcendental conditions for pragmatics. We need not let Gorgias dictate the terms of the debate any longer. We can grant that there is no (or very little) common ground at the start of an exchange, but at the same time maintain that that common ground can be built through a nonopportunistic argumentation procedure.

Returning to the debate at hand, nonopportunism places some interesting constraints on permissible moves in arguments between advocates of classicism and naturalism. Two are worthy of note here. For starters, as far as dialectical resources are concerned, nonopportunism prevents the classicist from turning to her advantage the naturalist's arsenal of historical and scientific findings and methods. Likewise, the naturalist must steer clear of the classicist's repertoire of conceptual analysis, *a priori* intuitions, and transcendental arguments. Admittedly, the difference between these dialectical resources often boils down to matters of presentation. Many of the same points that can be made by appealing to *a priori* intuitions, for example, can also be made by appealing to scientific findings. This last point turns out to have more metaphilosophic significance than it may first seem, which brings us to the second, subtler point.

I have argued only that adherence to the principle of nonopportunism would promote a fair debate between the classicist and the naturalist. But I have also claimed that nonopportunism would have the epistemologically deeper consequence of dislodging the two sides from their current dialectical impasse. To see how that might happen, let me introduce a term of art, *Hegelian Naturalism*, to describe the strategy of articulating classical epistemological concerns within the dialectical constraints available to the naturalist. To play the epistemological game by Hegelian rules is to ask which side is more effective at transcending the difference in perspective that the other side poses: Who is the better synthesist? Notice that this question presupposes that the two positions in the debate have been clearly disentangled from one another—as "thesis" and "antithesis," if you will—such that the terms of disagreement are appreciated by both sides. However, the main problem with the classicism–naturalism debate is that the two sides tend to argue at odds with their respective positions. This problem, in turn, suggests that the terms of disagreement between them have yet to be properly identified. If true, what may be useful, as *propaedeutic* to debate, is for each side to catch the other in self-contradictions or "immanent critiques." These preparatory practices would be nonopportunistic precisely because they are meant not to silence the opponent but to enable her to articulate her position more clearly.

The need to make one opponent's position dialectically tractable is especially pressing as analyzed in the following case. Here the classicist (Clay) must help the naturalist (Nate) tease out his own position before the naturalist can properly incorporate the classicist's objections in an attempt to transcend the terms of their disagreement. My interest will be in playing the naturalist's hand in this Hegelian game. But first we need a canonical formulation of the dialectical rut that gives rise to the need for the type of rapprochement I have sketched. What follows is an all too typical exchange between Nate and Clay over the metaphilosophic soundness of naturalized epistemology (see e.g. Siegel 1989 vs. Giere 1989; Siegel 1990 vs. Laudan 1996: pt. 4).

Nate: Epistemology—or at least philosophy of science—is viable only as a science of science.

Clay: But what's so philosophical about that?

Nate: We need to explain how science has enabled us to learn so much about the world.

Clay: But that presupposes that science does give us knowledge. But how does one *justify* science's claim to knowledge? That's the philosophical question you need to address.

Nate: I'm not so sure: You classicists have been going at it now at least since Descartes—and to no avail.

Clay: But all that shows is that *you* are frustrated and, hence, want to change the subject. You haven't actually shown that an epistemic justification of science is impossible.

Nate: Look, your whole way of talking supposes that epistemology is autonomous from science. In fact, I wouldn't be surprised if you thought that epistemology was *superior* to science!

Clay: My private thoughts are not at issue here. All I want to argue is that epistemology must be pursued *apart* from science if science's epistemic legitimacy is to be judged without begging the question.

Nate: But there are no categorical epistemic principles that establish science's legitimacy. There are only instrumental principles that tell us the most efficient course of action relative to a given end.

Clay: But aren't there ends of science per se? And how are they justified? Doesn't that bring us back to my original concern?

Nate: There has been only one end in common to the multitude of ends that have led people to pursue science throughout the ages—namely, an interest in finding out what the world is like. But in any given historical case, how the scientist proceeds to find out what the world is like will depend on the other ends that she is pursuing at the same time.

Clay: But at most that explains particular local successes of science, not the global success that you allege underwrites the epistemic legitimacy of science.

Nate: Well, I never said that the science of science had to be purely descriptive. After all, the cumulative instrumental successes of science strongly suggest that we have managed over the centuries to achieve a more general understanding of how the world works. Indeed, the point of proposing *theories* in science is to capture the nature of our understanding. Moreover, once articulated, theories can be used to inform future action.

By the end of the sixth round, Nate has been once again brought to saying that epistemology is only as well grounded as the science it grounds. Clay will undoubtedly reply that grounding is not enough. So we have returned to the start of the exchange, each side neither deepening his own position nor budging his opponent's. What is keeping the debate in such a rut? The naturalist continues to fall into dialectical grooves largely of the classicist's making.

These grooves run deep. Take the very thing that Nate and Clay are trying to justify and/or explain. To keep the debate somewhat focused, I have had both sides characterize this thing as "science." On the surface, this designation would seemingly bias the discussion in naturalist's favor. The word *science* signals a sociohistorically specific form of knowledge (one begun, say, in 17th-century Europe) that makes a point of refusing to rest on its epistemic laurels. The scientific call for the repeated testing and revising of knowledge claims goes against the classicist's interest in establishing an intuitive or conceptual terminus to inquiry. However, Nate's remarks do not make clear that this experimental attitude captures *his own* stance toward science. For Nate, science suspiciously partakes of some of the properties that Clay wants to attribute to knowledge. In particular, science does not seem to be an entity clearly bounded in space and time. Note the apparent indifference whether Nate talks about science as a body of knowledge, a cognitive process, a group of people, or a single individual scientist.

Nate also fails to clarify whether what impresses him as worthy of justification and/or explanation is how that unit operates on a day-to-day basis, only on exemplary occasions, or cumulatively over the long haul (starting when?). A related point is Nate's failure to see the possibility that the epistemic legitimacy of science may change during the course of its development. For example, if Nate followed Karl Popper (1970) in holding that science is epistemically impressive only during its revolutionary phases, then his attitude toward everyday science would not be too far from Clay's. Both would then bemoan the normally unreflective attitudes that scientists display toward the epistemic foundations of their enterprise. Nate and Clay would, of course, continue to diverge over whether there could be more to epistemology than relentless self-criticism. But Nate would begin to see that Clay's lingering doubts about science's epistemic legitimacy are based on something more than mere philosophical one-upmanship.

Here we might wonder just how incommensurable Nate's and Clay's starting points might be in relation to what has been historically identified as "science" and "knowledge." Given Nate's emphasis on the instrumental success of science, we can easily imagine him telling a story of science emerging as a *by-product* of our biological need to solve problems. I stress "by-product" because, on this view (associated with both Dewey and Popper), "science" is the repository into which ideas and techniques enter once they have been crafted to solve particular life problems. "Scientists," then, have the leisure to develop a discourse that interrelates these artifacts, especially so as to reveal ways in which the achievements of some of the artifacts overcome the limitations of others. This discourse—which is really the only part of Nate's story that would interest Clay—is the one whose utterances are routinely evaluated as being "true" or "false." Now, by believing this story, Nate is in a position to have any of the following attitudes toward the relation of "science" and "knowledge":

1. Nate may think that pursuing science for its own sake is an indirect but, ultimately, best route to increase human problem-solving ability. Knowledge, still defined as problem solving, will thereby increase. In that case, the role of science in our pursuits will have changed from mere by-product to explicit aim. (This captures the spirit of Popper's [1972] "evolutionary epistemology.")

2. Nate may think that pursuing science is worthy only if it contributes to human problem-solving ability. In turn this ability would be judged by welfare standards independent of those used to judge the progress of pure science. He would be sensitive to an overzealous pursuit of science that produced "useless truths" that do not deserve the title of knowledge. (This captures the "finalizationist" school of philosophers of science who follow Habermas [cf. Schaefer 1984].)

Nate may hold a historically informed combination of (1) and (2). At first, (1) was a good strategy. Unfortunately, since 1945, science's magnitude has made pure research a very uneconomical way to address human problems. The turn to (2) came out of the need to deploy enormous resources to create an artificial environment for testing a particular scientific claim's truth or falsity. (This is in the spirit of Feyerabend's [1979] call for downsizing the scientific enterprise.)

Notice that none of these attitudes takes either science or knowledge as existing in a vacuum for all times and places. A suggestion exists that what Clay calls "knowledge," although relevant to the discursive development of science, may not be particularly relevant to what Nate calls "knowledge," especially once the pure pursuit of science is called into question, as in (2) and (3). By having Nate adopt (1), I minimize the level of potential incommensurability his attitude toward knowledge and science.

The Principle in Practice

Suppose we ask Clay what transpired in his exchange with Nate. Clay would say that Nate merely slid into the dialectically least tractable position in the classicist's game—the proffering of intuitions. However, Nate would say that he changed the rules of the epistemological game. Of course the classicist is expert at calling intuitions into question, namely, by challenging their "clarity" and "distinctness." Clay might therefore ask whether Nate's conception of science is internally consistent, and if so, whether it can be distinguished from other conceptions of knowledge. Since the ontological dimensions of Nate's "science" are somewhat uncertain, assessing the clarity and distinctness of his conception is difficult. But according to the principle of nonopportunism, we should not expect Nate to be impressed by Clay's tactic. Instead, Nate should translate into his own terms what Clay means by treating the naturalistic conception of science as an unanalyzed intuition.

Empirically speaking, "science" is a disciplinary cluster including at least all of the natural sciences and probably most of the social sciences. All of these disciplines are interested in "how the world works." Consequently, each discipline has preferred surrogate for truth or the ultimate end of inquiry. Newtonian mechanics gave science the truth-surrogate of parsimony: that which explains the most by the least. Darwinian biology provided the truth-surrogate of survival, which has been recently popularized by the cognitive scientist, Daniel Dennett (1995). Among the truth-surrogates inspired by the social sciences, welfare economics has offered the greatest good for the greatest number, while electoral politics has contributed a variety of consensus models. Naturalists have typically alternated between these surrogates as if they were functionally equivalent or at least converged at the limit of inquiry. Thus, the American pragmatist Charles Sanders Peirce (1955: 361-74) seemed to believe that the simplest theory was the one with the highest survival value and the one that would command the consensus of inquirers. Their lives, in turn, would be made better off by accepting the theory than by accepting any alternatives.

However, Nate must admit, if his naturalism extends to the history of science, that the disciplines responsible for these truth-surrogates arose and have been maintained under circumstances that cast doubt on the claim that their "ends" are in lockstep. For example, a politically inspired naturalist may claim that truth is consensus (or that a proposition is true because it enjoys the consensus of scientific opinion). A biologically inspired naturalist can respond that theories have been known to survive for long periods as the source of productive research, even though they never held most scientists in their sway. Indeed, biologically speaking, those theories may be understood as having avoided the excess of "overadaptation," whereby a species loses its dominant status once its hospitable environment changes slightly.

Clay would jump on this last point as evidence for the ambiguity and indistinctness of Nate's conception of science. This charge will force Nate to be more selective in his endorsement of science. He can't have both survival and consensus as truth-surrogates if they grant epistemic legitimacy to different theories. How, then, does Nate decide between biology and politics as models of the knowledge- production process? For Clay, this question signals the need to transcend disciplinary differences and to appeal to a more global sense of epistemic legitimacy, one quite familiar to and contestable by the classicist. At this point, some naturalists (e.g. Bhaskar 1979) turn classicist by appealing to transcendental arguments. However, Nate can hold his ground by inferring that "science" does not pick out a natural kind of knowledge, an epistemic essence common to the natural and social sciences (Rorty 1988). That would certainly explain the "incoherence" that Clay sees in Nate's conception.

More generally, the non-naturalness of science is a problem for Nate only if he expected that all the epistemic virtues would line up behind one theory at the end of inquiry. That truth emanates from one source to which all inquiry then aspires is a Platonic residue in classical epistemology. Nate can simply reject such a picture and argue that science's epistemic superiority rests on the tradeoffs made from among the cluster of virtues exemplified by the different truth-surrogates. Thus, whereas, say, certain monastic religions value the long-term survival of their beliefs at the expense of all the other epistemic virtues, science trades off survival for some other truth-surrogate, such as parsimony or fecundity, after a certain point.

In short, Nate can escape backsliding into classicism by portraying the world in which knowledge is produced as one that does not permit all the epistemic virtues or truth-surrogates to be jointly maximized. Behind this picture is much more than human finitude: The ends of knowledge have become so diversified over the course of history that the best one can be is an "epistemic satisficer" among mutually conflicting ends (Giere 1988: 141-78; cf. Fuller 1989: 42-49). The epistemologist's role, then, is to ensure that the different truth-surrogates are traded off in some appropriate fashion. Nate has now finally relinquished the classicist's ideal of one best theory on which all knowledge can be grounded. He nevertheless manages to shore up what Clay feared was generally lacking from naturalized epistemologies— namely, a robust normative orientation that is potentially critical of current epistemic practices. In the original exchange, Nate suggested only two ways to think about how the "ends of science" existed—either a generic end that is associated with science per se or specific ends that are associated with the personal goals of the people who pursue science. As Clay then countered, the "generic end" plays into his own understanding of science. The "specific ends" amount to simply accepting at face value the reasons why scientists do what they do.

Here is the lesson to be learned from this dialectic: The history of science creates the need for epistemological intervention as first-order empirical knowledge becomes the basis for disparate second-order conceptions of the ends of knowledge. This argument supposes that the need to make value judgments arises from concrete exigency, with the judgments evaluated by the exigencies to which they subsequently give rise. Yet a contemporary naturalist like Nate can diminish the force of this argument by unwittingly presuming part of the classicist's position. We can see this in the final round in terms of Nate's sanguine attitude toward the instrumental success of science.

Nate seems to claim that if applying a certain theory increases our control over nature, then that theory can be automatically credited with success, which in turn earns the theory a place in the storehouse of human knowledge. The *post hoc*, *propter hoc* fallacy in that line of reasoning is easily spotted. But still worse, from the standpoint of a naturalist, is Nate's failure to evaluate the consequences of the theory's application in terms of the exigencies that arise. These exigencies are what economists would recognize as the process, opportunity, and transaction costs that are by-products of a theory's "instrumental success." (For economists, *process costs* and *opportunity costs* refer to, respectively, the effects of doing something now on the ability to do something else later and the outcomes that probably cannot be realized because of what one has already done. *Transaction costs* are the additional things that need to be done to realize the desired outcome.)

Nate's negligence here is an instance of what Dewey and other pragmatists derided as intellectualism. But notice that to be "anti-intellectual" in this sense is not to be "anti-rational." As Nate's responses indicate, the naturalist simply denies that there is a species of rationality associated with knowledge production as unanalyzable as a form of instrumental rationality. Thus, a truly rational naturalist should be interested in all the consequences of her actions, not only the ones that formally test her theory. Nevertheless, Nate and other so-called naturalists in epistemology and the philosophy of science remain narrowly intellectualist, as if theories normally had consequences only for the conceptual development of science (Lakatos 1979; L. Laudan 1977; Shapere 1984).

BUILDING THE BETTER NATURALIST

Behind Nate's latent intellectualism is a view of language—at least of theoretical language—that is shared by Clay. This seemingly innocent view is that theorizing does not transform the world in the manner of other productive activities; rather, it merely produces causally inert "mirrors of nature." Thus, the only consequences of theorizing that concern Nate are the ones that determine the extent of the world's conformity to his theoretical expectations. But because classical epistemologists have also shared this view, they have made a point of introducing a distinctly normative dimension of justification alongside the empirical one of explanation. In this way, the classicist may intervene in the knowledge-production process for purposes of criticizing and perhaps even revising the foundations of that process. Naturalists typically fail to make such a distinction. Consequently, Nate is easily read as having no normative interests aside from the clinical ones of assessing the extent to which theories are confirmed or the extent to which means achieve their ends. Clay fails to catch Nate in a commitment to anything more robustly normative only because of Nate's refusal to take to heart the naturalist dictum that, because knowledge is part of the same world as the objects of knowledge, every theoretical representation is ipso facto a causal intervention (Hacking 1983).

More specifically, theorizing—especially the sort of metascientific theorizing that a naturalized epistemologist is likely to do—can be either a *passive* or an *active* form of causal intervention. Good examples of passive intervention may be found in the ethnographic accounts of "laboratory life" (e.g. Latour and Woolgar 1986) that constitute much of the empirical base for STS. The accounts profess to offer descriptions of ordinary scientific practice shorn of all normative epistemological baggage. The ethnographers tend to tell fairly prosaic tales of the labs. People and other medium-sized dry goods are shunted back and forth in a setting only slightly less structured than the average industrial plant. However, because this "neutral" description of science clashes with the expectations of readers, most of whose images of science are already very norm-laden, the net effect of these ethnographies has been to inspire a wide-ranging reevaluation of the epistemic legitimacy of science. Yet the ethnographers themselves claim they are merely describing what they have observed. Thus, the ethnographies passively intervene in the scientific enterprise simply by offering a perspective that differs substantially from standing expectations, thereby unintentionally questioning the groundedness of that enterprise.

How does this appeal to science "as it actually is," also known as descriptivism, turn out to be so rhetorically effective, even though it is rarely tested? From a rhetorical standpoint, a description is a verbal representation of some object to some audience, such that the speaker is able to change the audience's attitude toward the object without changing the object itself. Thus, the trick for any would-be describer is to contain the effects of her discourse so that the object remains intact once the discourse is done. In descriptions of human behavior, this trick is often very difficult to manage, as the people being described, once informed of the description, may become upset and proceed to subvert the describer's authority. STS research finds that this predicament extends even to the natural sciences, even though their objects do not seem capable of either eavesdropping or talking back. Nevertheless, natural objects typically have their own spokespeople (experts) who are capable of personifying the challenge that a description may pose to the disposition of the objects described. Thus, if the STSer claims that a given theory works only because the relevant people agreed, the spokesperson for nature could always rejoin that the STSer hasn't examined the depth or detail of the natural process in question. In that case, the spokesperson's plea for comprehensiveness disguises an attempt to keep the burden of proof squarely on the STSer's shoulders.

If the STSer wants to secure for her descriptions the aura of detachment that comes from representing things as they are, then she should construct her descriptions in a language that only the describer's intended audience will understand. Hence, an elective affinity exists between capturing the world "as it actually is" and operating from an autonomous disciplinary standpoint. This affinity, I believe, explains the sense of objectivity that often accompanies the introduction of technical terminology. Not surprisingly, the call to descriptivism in STS has invited the cultivation of arcane "observation languages" that only fellow STS researchers—and not the scientists under study—can understand. In one sense, developing a discourse community is a step toward the disciplinization of STS. Yet this move ultimately goes against the democratizing mission of the field. Ironically, the "reflexive turn," an attempt that prima facie appears to aim at a more comprehensive picture of science, actually exacerbates descriptivism's tendency to provincialize audiences as "comprehensiveness" becomes relative to whether the author's presence is integrated into *her own* text (cf. Ashmore 1989).

By contrast, a theorist actively intervenes in the knowledge-production process when she tries to remake the process in the image of her theory. This straightforward idea is typically neglected by intellectualist accounts that portray theories as things that predict and explain, but not construct, phenomena. However, construction is arguably the most important role that theorizing plays in the social sciences. As my remarks on descriptivism suggest, I disagree with those (e.g. Hacking 1984) who believe that theoretical intervention distinguishes the social from the natural sciences. Next I focus on the styles of active theoretical intervention in *anthropology*, *economics*, and *psychology*.

Anthropologists commonly draw a distinction between an insider's everyday, *emic*, knowledge of social life and an outsider's scientific, *etic*, knowledge (M. Harris 1968). Often this distinction is cast as the difference between the "first-person" perspective of the native and the "third-person" perspective of the analyst (e.g. Fuller 1984). Often these two perspectives are made to look mutually exclusive, complementary, and exhaustive. Accordingly, if the anthropologist is to abide by an agent's normative categories, then she must also abide by the judgment calls that the agent makes on the basis of those categories (i.e. go emic). If, however, the anthropologist simply imports her own alien categories into the agent's situation, she, at least implicitly, questions the validity of the agent's categories (i.e. going etic). However, the journey from emic to etic affords a rhetorical way station. This *second-person* perspective, as it were, involves appending to the agent's own categories a tighter procedure for accounting for the agent's behavior. As a check on the agent's self-explanations, trained external observers (and, in more recent years, cameras and other more reliable recording devices) can be introduced into the situation. Not surprisingly, if one examines Francis Bacon's and other early justifications for the experimental method as a privileged source of knowledge, they spring from an awareness that if we were to scrutinize each other's behavior a little more closely than we normally do, the surface rationality of everyday life would yield to an assortment of biases and liabilities that "succeed" largely because they remain unchecked.

Bringing this anthropological insight back to the history of our own culture, Shapin and Schaffer (1985) masterfully analyzed the alignment of interests in 17th-century Britain that ultimately authorized experimenters to speak for a deeper analysis of ordinary experience. Experimental observations thus trumped the accounts of both naive observers (the emicists of the day) and learned scholastics (the eticists). The modern scientific mentality emerged once people started to regard the tighter accounting procedures as a *de*contaminant, rather than as a contaminant, of everyday life. Science, then, was seen not as artificially restricting our intercourse with nature, but as removing the obstacles that normally inhibit such intercourse. Once this long-fought battle was won, proposals of varying degrees of merit were made to reconstitute ordinary language in scientific terms. Thus was born positivism's "popular front"—that gallery of linguistic reformers extending from Jeremy Bentham ("science of legislation") to Count Korzybski ("general semantics").

Economics has recently become a favorite naturalistic model for reconceptualizing normative epistemology (Kitcher 1993). Philosophers are attracted to the field because the qualities of economic modeling, its abstract, reductive, rigorous, *a priori* character, most resemble analytic philosophical reasoning. However, focusing on these qualities obscures how economic models function in policymaking (cf. Lowe 1965). For example, a theoretical model of the market sets the standard that defines normal and abnormal economic behavior, as well as the obstacles that need to be overcome to approximate the market ideal more closely. Increasingly, economists interested in socially embedding the policy process have challenged this way of deploying models. These models presume the normative standard—the ideal market—to be a fixed equilibrium toward which economic activity eventually gravitates, with or without help from the government (e.g. Block 1990, especially Chap. 3). Such an orientation neglects irreversible moves away from the original state of equilibrium—many of them beneficial—that are produced by innovative entrepreneurship, the institutional absorption of transaction costs, and simply a change of scale in the economy (cf. Georgescu-Roegen 1970). In this real economic world, thinking of abstract models as "rigid rods" in terms of which actual economies are gauged and corrected no longer makes sense. Indeed naturalists attracted to economics would do well to study the recent attempts to formalize a more "relativistic" (in the Einsteinian sense), even stochastic, conception of market norms for real economies (cf. Mirowski 1986, 1991).

However, ongoing debates in psychology over the "external validity" of experiments perhaps provide the Hegelian naturalist with the most immediate insight into the problem of reconstituting, in empirical terms, the classicist's conceptually derived epistemic norms (cf. Berkowitz and Donnerstein 1982; Fuller 1989: 131-35). Critics of the experimental study of human beings typically argue that subjects' performances in the laboratory are too artificial to form the basis of generalizations about normal human behavior. Defenders then respond that experiments are designed to determine the contribution that an isolated variable (or set) makes to an overall effect. If the effect is a positive one, then the point would be to restructure the environments outside the lab more like the conditions that enabled the variable to contribute to the effect observed inside the lab. Thus, if the variable in question is a heuristic that subjects used to solve artificial problems, then the task ahead would be to transform normal problem-solving settings into ones in which the heuristic would also work. These heuristics tend to be drawn more or less explicitly from epistemic norms that epistemologists and philosophers of science have proposed. Consequently, a Hegelian naturalist could easily reinterpret classical talk of "ideal epistemic agents" and "rationally reconstructed histories" as first passes at specifying the laboratory conditions in which certain norms could be demonstrated to have epistemically efficacious consequences (cf. Gorman and Carlson 1989; Fuller 1992b; Shadish and Fuller 1994).

Given the precedents set by anthropology, economics, and psychology, naturalists, perhaps unsurprisingly, have failed to take to heart their own dictum that every theoretical representation is a causal intervention. Or perhaps not. After all, naturalists are often portrayed as either hostile or indifferent to metaphysics. Quine (1953) is a good example of someone who manages to project both images at once. Yet the naturalist loses ground to the classicist precisely when she ignores ontological considerations. Nate, concerned more with locating the consequences of theorizing in conceptual space than in physical space, unwittingly adopts the classicist's transcendental conception of language. As a result, Nate short-circuits the interventionism that gave John Dewey's naturalism its distinctive normative slant.

When the classicist defines knowledge as "justified true *belief*," she typically assumes that the same belief can be embodied in many different ways. These "multiple instantiations", include states of consciousness, unconscious states of the brain, the linguistic and nonlinguistic behavior of human beings, and perhaps even the behavior of nonhuman beings. Back in the Middle Ages, the problem of knowledge was not thought to be adequately addressed unless the philosopher could account for the multiple instantiation of a belief, or what was then called the "communicability of the form of the belief." Admittedly, recent classical epistemologists have had little to say about how it is possible that all these different sorts of things embody the same belief. But, in large measure, this silence simply reflects the post-Kantian tendency to treat epistemological questions as separate from questions of metaphysics and even the philosophy of mind. Thus, the classicist presumes that beliefs can be communicated in various forms. Then she characterizes a special epistemic relation in which one such communicated form stands to some external reality. In short, the classicist asks: What makes my (true) belief that S is P a belief *about* S's P-ness? The fact that a particular instance of my belief that S is P inhabits the same world as—and hence stands in some causal relation to—a particular instance of S being P is immaterial to the classicist's epistemological concerns. Nevertheless, this relation is material to the naturalist's concerns. When the naturalist asks what should one believe, she simultaneously makes implicit reference to a vehicle for instantiating a belief (whether it be a neural network, a piece of electronic circuitry, or a pattern of social interaction), the likely causal trajectory of that vehicle in relation to other things in the world, and the relative desirability of the possible outcomes of that vehicle's interaction with those things. Naturalism, as a result, looks more like science policy than literary criticism. It is quite different from the theories of knowledge to which classicists are accustomed, but at least it is one worth arguing about.

NATURALISM'S TRIAL BY FIRE

250 years ago, David Hume notoriously prescribed that books whose claims were grounded in neither logic nor experience should be cast into the flames. Hume, one of the acknowledged progenitors of naturalism, thought that books of metaphysics and theology should take their rightful place amidst the timber in his fireplace. But perhaps we need to apply another dose of his harsh medicine to ourselves: *If the epistemologist is neither noticeably improving the production and distribution of knowledge in society nor accurately describing current practices, then what exactly does she think she is doing?* From what we have seen in this chapter, perhaps an unwholesome third way has been paved: Epistemologists are devoted to describing what an improved state of the knowledge system "would look like"—with the subjunctive left dangling in midair. In practice, the accuracy of such a description is relative to the ideal that the particular epistemologist has in mind, her "intuitions," as it is sometimes called (L. J. Cohen 1986). These intuitions may be conceived and refined before an audience of fellow epistemologists generally far removed from the people who would need to be persuaded for any of these intuitions to be realized. Ironically, then, the naturalized epistemologist, despite being preoccupied with the causes of our beliefs, is herself causally insulated from the workings of the very enterprises whose norms she would legislate!

This irony speaks to the ultimate violation of the Principle of Nonopportunism that today's naturalists tend to commit: They accept, without question, the classicist's conception of *what it is to be a norm*. For the classicist, a norm commands our attention if it makes sense "on paper" or in a discussion with our similarly trained friends. The relevant criteria for evaluating norms, then, include aesthetic satisfaction, logical coherence, and overall intellectual and pragmatic suggestiveness. Missing from this list are criteria specifically associated with *governance*, such as the propensity for gaining the consent of the governed. Even a logical positivist like Hans Kelsen realized that a statement is not a norm, regardless of its content, unless it has the power to bind action—a lesson that rhetoric has been teaching for over two millenia.

THOUGHT QUESTIONS

☙ Is the debate between classical and naturalistic approaches to epistemology an intradisciplinary debate? An interdisciplinary debate? What are the terms of, and positions in, this debate? What is Tychnoic Naturalism? Why, according to Fuller, is "striking a balance" between the classical and naturalized epistemology unsatisfactory? Can this debate, procedurally, be cast in the same way as interdisciplinary debates?

☙ How does Fuller characterize Alvin Goldman's position? What is the relationship between naturalism and cognitive psychology? In outlining this debate is Fuller offering an object lesson in interdisciplinary interpenetration? How do the sensibilities of naturalists and social epistemologists differ with respect to the process of interdisciplinary interpenetration?

☙ Why do psychology and naturalized epistemology need each other? Does Fuller's defense of the necessity of epistemology, in relation to psychology, run counter to the aim of interdisciplinary interpenetration? Why or why not?

☙ What argumentation strategies are needed for psychology and epistemology to renegotiate disciplinary boundaries? How might the transformation of physics and the neuroscience change psychology? More generally, can one account for the ways in which change in one discipline may directly or indirectly lead to change in another discipline?

☙ What would the interpenetration of psychology and epistemology entail? How would our understanding of "the individual" and "the social" change as a result of the interpenetration of psychology and epistemology? Why do disciplines need a social epistemology? What difficulties occur in relying on individuals in the process of generating knowledge?

☙ What biases do we hold, according to Fuller, regarding the conduct of science? How does social epistemology act as a corrective to these biases?

☙ How does Fuller characterize Rom Harré's position? How does Harré's conception of epistemology and psychology differ from Goldman's? How does Harré characterize the difference between experiment and analysis? How does ethnography fit into Harré's analytical scheme?

☙ What role might ethnomethodology play in have in the process of interdisciplinary negotiation, generally, and in the debate between naturalist and classicist epistemologists? What is ethnosemantics? What perspectives might an ethnosemanticist provide which might transform interdisciplinary debate?

☙ How might Fuller's "principle of nonopportunism" be extended to interdisciplinary debates and, hence, the process of interdisciplinary interpenetration? Must the principles involved in interdisciplinary negotiation begin by admitting and determining that the positions the hold may be incommensurable? Must the principles involved have an agreed upon understanding of history?

☙ Is the process of theorizing interpenetrative? What role might theorizing play in the process of interdisciplinary negotiation? How do the styles of active theoretical intervention in anthropology, economics, and psychology compare? How do you account for the similarities and differences?

☙ Has epistemology improved the production and distribution of knowledge within traditional disciplinary structures? What role might epistemology play in the process of interdisciplinary negotiation? Fuller appears to advocate a both rhetorically informed conception of theory and a rhetorically informed conception of epistemology. How would the interpenetration of rhetoric and theorizing or epistemology aid in process of interdisciplinary interpenetration?

4

Reflexion, or the Missing Mirror of the Social Sciences

HOW SCIENCE BOTH REQUIRES AND IMPOSES DISCIPLINE

Here is a possible story about how science developed. Science originally arose in the area where humans displayed the most knowledge and interest—namely, themselves. Gradually, the human cognitive grasp moved outward: first, toward the nonhuman things with which they had the most in common, then to the more remotely nonhuman. Ultimately, humans made sense not only of nonearthly things (e.g. the heavens), but made sense of a perspective that was literally a "view from nowhere." Such was pure objectivity (cf. Nagel 1987). The general strategy behind this outward reach would be for humans to model the nonhuman as much as possible on facets of themselves and then use the points of disanalogy as the basis for an autonomous body of research that eventually issues in a full-fledged science. Thus, on our story, biology would be expected to have spun off from sociology after a critical number of nonhuman properties had been recognized in animals. The newest science should be the most nonhuman study of them all, cosmology, which conceives of reality as a "universe" of which humans are an infinitesimal part.

Now, of course, this story is not only fictional, but the exact opposite of the true story. What went wrong? As a first approximation, our story may have taken too seriously the idea of explaining the unknown in terms of the known. On second thought, perhaps a little too uncritically. For one of the most instructive themes in the history of science is that more knowledge is better than less *only after science is already in place*, and quality controls have thus been instituted for the production of knowledge. Otherwise, less knowledge is better especially regarding things—such as observable physical objects— whose remoteness from the human condition makes an objective evaluation easier to provide. Now in what exactly does this "remoteness" consist? Remoteness consists in a *rhetorical impoverishment*— a lack of alternative discourses for characterizing the phenomena in question.

Rocks, streams, and stars—the stuff of which both early Greek cosmology and Renaissance physics were made—have rarely been elaborated with the richness or complexity of human creations. Consequently, these things provide a more natural basis for standardized observation languages, which in turn enable both smoother communication and easier conceptualization, which in turn offer the possibility for greater manipulation and control of the motions of the things themselves (cf. Dear 1987 on the influence of this line of thinking during the establishment of the first scientific societies). Although ordinary language affords many ways to imagine the agents and resultants of change in human behavior, the number is considerably smaller in the case of, say, rock behavior. Even today textbooks in the more "scientific" of the social sciences—psychology and economics— will routinely introduce their subject by talking about the need to regiment our common talk about human beings. Such textbooks, to help preempt student worries that they merely clothe the obvious in jargon, typically point out that too much unanalyzed information is often worse than too little.

The lesson to learn from our false story is that the development of science involves something other than the spontaneous accumulation of knowledge or the spontaneous generation of ideas. Rather, scientific development requires *discipline*. Discipline requires the cultivation of a consistent perspective by adopting a language and techniques that focus the inquirer's attention generally to the exclusion of other potentially observable matters. Perhaps this point is the one most frequently taken from Thomas Kuhn's seminal work, *The Structure of Scientific Revolutions* (1970). However, the point invites belaboring because, with the exception of Toulmin (1972), only relatively recently have disciplines been studied in systematic detail. The variety of approaches to them may be found in Graham, Lepenies, and Weingart (1983), Willard (1983), Shapere (1984), Whitley (1985), and Bechtel (1986). In terms of the things one might study about science—including theories, concepts, research programs, and the like—only disciplines as units require the cooperation of the rival historiographical approaches in science studies. These rival approaches are the *internal* approach, devoted to charting the growth of knowledge in terms of the extension of rational methods to an ever larger domain of objects, and the *external* approach, devoted to charting the adaptability of knowledge to science's ever-changing social arrangements (cf. Fuller 1989: pt. 1). Disciplines mark the point where methods are institutionalized, where representation is a form of intervention, where, so to speak, *the word is made flesh* (cf. Fuller 1988a: Chap. 8).

My own contribution to this discussion has been twofold. First, I have observed how the referential character of a discipline's discourse draws attention away from the discipline's source of power. The audience is beckoned to focus on certain prescribed objects whose identities are detached from the speaker's. At the sign of a successful science, all objects become externalized from the speaker's identity so that conceptual space is no longer available to hold the speaker accountable by the standards imposed on the prescribed objects. A piece of technology, for instance, is routinely regarded as the "application" of physics. Still the physics is portrayed as being already embodied in the technology rather than as something which the technologist physically adds (Fuller 1988a: especially 188). Second, I have noted that once objects have been externalized in a disciplined manner, they serve as standards against which to evaluate and calibrate human performance. Consequently, the history of science reveals the "human" to be a "floating signifier," the shifting residue of our behavior that resists standardization. I have examined this issue most closely in terms of computers used to model thought (Fuller 1989: pt. 2).

The rhetorical character of disciplinary boundaries in the social sciences provides an especially good context for examining the embodiment of knowledge as a source of worldly power. Epistemologists and philosophers of science typically neglect this topic because they tend to think of knowledge as a politically indifferent, or "disembodied" phenomenon.

I start with an apparent technical problem in the philosophy of science: Is it possible to demarcate criteria for demarcating science from nonscience? Recent philosophers have despaired of finding such "metaboundaries" and, as a result, have begun to call into question the very identity of the philosophy of science. Against this line of reasoning, I argue that the demarcation project's failure only shows that attempts to study science scientifically, as the philosophers have wanted to do, often result in science deconstructing its identity. Clearly, the epistemic authority of science has worked to block such self-deconstructive moves in the normal course of inquiry. How? I propose a strategy for addressing this question—namely, to examine how science exercises worldly power by rhetorically drawing our attention to the fact that scientific knowledge *represents* the world and away from the fact that it also *intervenes* in the world (Hacking 1983). (I speak of this duality of knowledge in terms of its being both *about* and *in* the world.) Given the social sciences' uphill battle to secure epistemic legitimacy, the rhetorical seams of their attempts to represent the world, without appearing to intervene in it, are easy to see. After surveying the canonical historiographies of five social sciences for this theme, I focus on the battle fought between economics and political science over the contested field of "politics." Finally, I return to the subversive, and hence relatively unexplored, possibilities for studying science scientifically. These include various social sciences of science, as well as deconstructions of the natural sciences' historical ascent to worldly power.

Studying disciplinary *boundaries*—their construction, maintenance, and deconstruction—adds a new dimension to the "interface" role already played by disciplines. Rather than merely continuing a recent line of inquiry, this one goes to the heart of what has made philosophy of science a distinct specialty throughout most of this century. Philosophers of science are most familiar with disciplinary boundaries from Carnap's and Popper's quests for *demarcation criteria* that systematically discriminate the sciences from nonscientific (and especially pseudoscientific) forms of knowledge. Disciplinary boundaries provide the structure needed for a variety of functions ranging from the allocation of cognitive authority and material resources to the establishment of reliable access to some extrasocial reality. Historically, however, philosophers have not agreed on specific demarcation criteria. In part, philosophers have drawn the science–nonscience boundary to cast aspersions on the legitimacy of particular pretenders to the title of science (Laudan 1983). Popper's (1957) attempt to use the criterion of falsifiability to undermine the scientific credibility of psychoanalysis and Marxism is a clear case in point. An implication of this point is that the only property common to all disciplines deemed scientific may be the approval of the person doing the deeming. If true, then science has no ahistorical essence. Philosophy of science is doomed to failure insofar as it has been devoted to the divination of such an essence.

Notice what has happened here. In trying to bound the boundary of scientific from nonscientific disciplines, philosophers have come to discover only that no such "metaboundaries" are to be found. This finding suggests, somewhat unwittingly, a strategy for subverting existing disciplinary structures; showing their long-term *lack* of discipline. More generally, the philosophical project shows that no epistemically privileged way exists to confer epistemic privilege. Although one might think that this insight would enable philosophers of science to radicalize their understanding of knowledge production, the contrary has been the case. If anything, philosophers have debunked the idea of a demarcation criterion without debunking the things (i.e. the "sciences") that are supposed to be demarcated by such a criterion. Instead of taking seriously the possibility that philosophers might be getting at some privileged, albeit nonepistemic, means of conferring epistemic privilege, Larry Laudan (1996: pt. 5), for one, has advised that the whole project be scrapped. Thus, philosophy of science should be reabsorbed into epistemology. In effect, this move turns away from such relatively social units of epistemic analysis as "sciences," "paradigms," and "research programs" to more subjective and purely formal units, such as "beliefs" and "theories." Similar regressive moves have been made by Arthur Fine (1986), who would have the philosophy of science wither away, leaving the history and sociology of science in its wake, and Dudley Shapere (1984), who would have philosophers be content with raising successful scientific practice to methodological self-consciousness.

Admittedly, a certain perverse nobility exists in sacrificing the identity of one's own discipline for the sake of another. John Locke originally coined the word *underlabourer* to capture this tendency in himself vis-à-vis that "master builder," Isaac Newton. Today's philosophers of science have shed the demarcation problem to become once again underlabourers for the dominant scientific paradigms (Fuller 2000b: Chap. 6). To be sure, a sense of self-protection inspires this move. For if philosophers followed the full logical consequences of the demarcation problem, then they might be forced to reconceive the nature of science and challenge both professional and lay understandings of science. Few philosophers today have the courage —or nerve—of the positivists and the Popperians to take up that challenge.

WHY THE SCIENTIFIC STUDY OF SCIENCE MIGHT JUST SHOW THAT THERE IS NO SCIENCE TO STUDY

The task of demarcating demarcation criteria is "reflexive" in two senses. I have focused so far on one sense, whether the concept of a demarcation criterion is itself demarcatable: Have there been any significant properties common to the criteria proposed through history? The fact that these criteria have shared few, if any, significant properties seems to provide indirect evidence that no principled science– nonscience distinction can be drawn. Notice that *no more* than an indirect link between the premise and the conclusion can be asserted here since science may have an essence (in the classical positivist sense of there being an optimal methodological route to knowledge). But most philosophers—preoccupied as they are by the epistemic squabbles of their times—have failed to grasp the possibility. Indeed maybe one philosopher—Popper, say—got the criterion right. Yet his own immersion in local squabbles has tended, after the fact, to cast aspersions on the universality of his claims. In our post-Enlightenment age, we are prone to overlook the underlying point—*that some periods in history may be better than others for uncovering truths that apply to all periods.*

A deeper, second sense exists in which demarcating the demarcation criteria is a reflexive enterprise. For example, Laudan's (1996: pt. 5) negative conclusion is persuasive, in large measure, because he claims to have used scientific means for determining whether a science–distinction can be drawn. Laudan took a representative sample of opinions from the history of philosophy and found little mutual agreement. Indeed he claimed that one could predict the criterion that a particular philosopher proposed simply based on what she took to be the disciplines that were granted undue cognitive authority in her day. In sum, Laudan presented the lack of historical continuity in the demarcation proposals as inductive disconfirmation of the claim that science has an enduring nature. In this sense, scientifically studying the nature of science may reveal that science has no nature to study. Laudan, I argue, should not have the final say in this matter.

Philosophers may be right. No epistemically privileged way to confer epistemic privilege may exist. But from that assertion it does not follow that there is no *nonepistemically* privileged way. Specifically, behind the variety of demarcation criteria may lie a function that must be regularly performed in maintaining the social order. The relative constancy of philosophers' motives for proposing such criteria already hints at what this function might be. I have elsewhere defined the function in terms of the Baconian Virtues (Fuller 1988a: Chap. 7). A discipline is deemed to possess the Baconian Virtues once it is credited with producing the sort of knowledge that is necessary for maintaining the social order. Philosophers of science have perennially wanted to have a hand in determining that function of science. The natural sign that a discipline possesses these virtues comes in the production of esoteric knowledge that serves only to enhance its perceived centrality to society. As a result, a "cult of expertise" develops, which, in turn, enables the discipline to have access to vast political and material resources, including the seats of power (Abbott 1988: Chaps. 3-6). The epistemic variation potentially allowed in satisfying this social function is vividly illustrated in the shift in qualifications for the diplomatic corps between the 19th and 20th centuries. Earlier, a classical liberal arts education was believed to deepen the diplomat's appreciation of the representatives' (mostly European) shared values and ideals. In negotiations these values would serve strategically as rallying points for agreement. Over the past 100 years, however, the humanities have been eclipsed by engineering and economics as the preferred educational background for diplomats, reflecting a less elitist and less personalized sense of worldly power.

Two points stand out in trying to develop a nonepistemic way to confer epistemic privilege: The first pertains to implications for locating the center of the knowledge production process, and the second pertains to implications for "decentering" it.

In one sense, the nonepistemic route is a search for an implicit principle of social organization. The sociologist Niklas Luhmann (1979) introduced the concept of *self-thematization* to capture the fact that any well-bounded social system is marked by the presence of Kuhnian "exemplars." In a self-thematized system, one can point to a component activity that synecdochically represents the working of the entire system. The performance of the system's other components can then be evaluated in terms of that exemplary component. For example, if the scientific exemplar is Faraday's experimental work on electromagnetic induction (as was becoming the case in the second half of the 19th century), then one should expect some disciplines to start emulating the exemplar in their own work, perhaps even in dubious or superficial ways, so as to draw on the exemplar's epistemic authority.

Other disciplines, however, may be so removed from the exemplar that they would first have to erase their current identities before being taken seriously as sciences. The humanities, for example, have dealt with this social relocation from the center to the periphery of the knowledge-production process. For most of the Scientific Revolution, knowledge of rhetoric and the classical liberal arts was held to be the key to worldly power. Indeed the experimental tradition responsible for the ascendancy of the natural sciences first laid claim to the scientific exemplar by appearing to be more powerful in the terms set by rhetoric (Shapin and Schaffer 1985).

By the late 19th century, the sense of "worldly power" had sufficiently changed so that few took seriously the idea that rhetoric and the other humanistic disciplines were among its sources. Stephen Toulmin (1990) documented the transition as a shift in the seat of knowledge power from (personal) "influence" to (impersonal) "force." However, a more illuminating contrast may be seen in terms of the staging needed for the display of power. Take the idea of "law" before and after the emergence of the experimental paradigm in natural science. Before the rise of experiment, a law was a norm whose validity resided in its usefulness as a standard against which to evaluate and shape behavior. To explain something by such a law was to understand it as either conforming to or deviating from the norm. Thus, the expression "natural law" was indifferent to epistemic and juridical usage (cf. Zilsel 1942). A "monstrous" birth, for instance, was both an accident to be explained and a misfortune to be justified. However, the prediction of behavior had little to do with these two tasks. Knowing the laws of nature, or "nomothetic knowledge," did not entail that all the relevant phenomena were already disciplined by those laws. Rather, possessing such knowledge entitled the possessor to use the influence or force necessary to make the phenomena conform to the laws— including the elimination of certain persistently deviant creatures. For, even deviance-elimination could be represented, in Aristotelian fashion, as the human completion of nature by bringing the normative to full "self-realization."

However, the rise of experiment changed the character of nomothetic knowledge. It no longer referred to the *original authorization* of force in the name of normative order. Rather, nomothetic knowledge referred to the *subsequent achievement* of normative order once such force, or discipline, had been imposed. This point is most apparent in what positivists (e.g. Hempel 1965) call the "symmetry" between explanation and prediction. Accordingly, a scientific law does not explain a range of phenomena unless the law can also be used to predict those phenomena. (One might think of this as the principle of the unity of theory and practice in science: If I truly understand something, I can then control it.) Of course given their abstract character, scientific laws are fairly useless in predicting phenomena unless the phenomena have been disciplined in advance, say, by minimizing the number of interacting variables in a laboratory environment. A highly disciplined setting, endemic to all experimental inquiry, is typically articulated in a ceteris paribus clause attached to the law. From this expression follows the various background conditions that must be maintained for the desired law-like regularity to be displayed in the laboratory. A major finding of on-site studies of science in action is the vast human and material resources—often quite specific to lab locales—that are required for maintaining such background conditions on a regular basis (cf. Collins 1985). In the modern day this activity happens behind the scenes of science and is ferreted out by diligent sociologists. But before the age of experiment these events transpired in the open—indeed, in public trials.

In sum, the transition from humanistic to experimental epistemic cultures may be captured as follows: Humanistic experimental culture issues laws to license overt politicking in the name of science. Experimental epistemic culture issues laws only to reward such politicking that succeeds by conferring on it the name of science. In humanistic culture, then, the "natural law" of the judges and the philosophers are the same. In experimental culture, the "positive law" of judges enters just when the "positive laws" propounded by social scientists fail to significantly constrain people's behavior. Deconstructing sources of worldly power, therefore, requires a process of reversing the transition from humanistic to experimental cultures by revealing the moments of politicking. Moments, for instance, when various parties' voices are amplified, silenced, or in some other way strategically represented, as their ends are translated into the means to someone else's ends (cf. Callon and Latour 1981).

Not surprisingly, in the last 100 years, as the humanities have decisively lost their claim to worldly power, their epistemic aspirations have also changed. Instead of touting their role in the creation and maintenance of cultural values, the humanities have turned to the academic pursuit of knowledge "for its own sake." In this context, humanistic inquiry occurs without regard for the social consequences beyond its institutional boundaries. In *Social Epistemology*, I tagged this move "the retreat to purity" (Fuller 1988a: Chap. 8). One way to understand what has happened is by appealing to the social psychological concept of *adaptive preference formation*, a class of strategies for rationalizing failure whereby one adapts aspirations to match expectations (cf. Elster 1983). In short, you come to want what you are likely to get. Indeed one sign that a discipline is receding from the exemplar of science is its refusal to be judged by concrete outcomes, especially the production of real world effects.

However, if adaptive preference formation has influenced the recent history of the humanities, two points need to be kept in mind: (1) the humanities have not always been so coy about being judged by practical consequences (e.g. the claims to worldly power made for humanism during the 16th-century Renaissance and the early-19th-century German university movement); and (2) the success of a discipline's claims to worldly power is based largely on folk perceptions about the discipline's ability to transform the world, which in turn serve to define the exemplar of worldly power.

Of course supposing that someone, at some point, actually demonstrated that the natural sciences were more efficacious than the humanities would be extremely misleading. Both the historical record and current divisions of labor reveal that technologies have been developed and maintained despite users' ignorance of the relevant physical principles. This fact provides sufficient reason to seek alternative explanations in science (cf. Laudan 1984: Chap. 5). Nevertheless, people have been persuaded to presume that the efficacy of natural scientific knowledge is behind effective technology. This presumption serves to *preclude* the need to demonstrate that natural science has indeed generated the relevant effects (cf. Mulkay 1979; Fuller 1988a: Chap. 4).

Clearly, some artful rhetoric must be involved to enable most people to routinely ignore the empirical and conceptual doubts previously raised. After all, what might otherwise be taken as grounds for skepticism (of the efficacy of natural scientific knowledge) is instead just taken for granted. I have elsewhere called this phenomenon, whereby lack of explicit refutation is taken as implicit confirmation, *the inscrutability of silence* (Fuller 1988a: Chap. 6). Simply referring to popular ignorance or impressionableness will not do because artful rhetoric brings about and can explain the appearance of these factors. My own explanation turns on the difference between the contexts in which the social and natural sciences are held accountable for their knowledge claims.

To impute "success" to an enterprise is to imply both a standard of evaluation and a procedure for evaluating cases. One of Arthur Fine's (1984) arguments against the need for a distinct specialty called "philosophy of science" is that the natural sciences' alleged track record of theoretical and empirical successes does not withstand close historical scrutiny. However, scrutinizing the record is difficult because, until relatively recently, historians of the natural sciences have had a remarkable capacity for recalling the same cases as successes and forgetting virtually all the failures (cf. Fuller 1988a: Chaps. 3, 9). In contrast, historians of the social sciences tend to disagree about what should count as successes and failures. Consequently, less of the actual history is consigned to silence. These histories often center on disputes that seem to be of greater import than their inconclusive and temporizing outcomes. Not surprisingly, since less of the actual history is suppressed by historians of the social sciences, the success rate of those disciplines seems weaker.

Interestingly, this contrast is reflected in the microhistories that are constructed in the literature reviews that preface articles in the natural and social sciences. Larry Hedges (1987) found that physics articles routinely report greater cumulativeness in their lines of research than psychology articles. The reason for this finding is not because physics research is so much more replicated and extended than psychology research; rather, the different statistical techniques that the disciplines use to analyze and synthesize data. In brief, physicists are inclined to intuitively throw out studies that would make for extreme data points and to use fairly elementary statistical techniques to elicit clear empirical regularities. Psychologists tend to integrate every available study into a complex statistical formula that has the level of lowering the level of certainty and reliability of their findings. (The practice is called "meta-analysis": see Shadish & Fuller 1994: chap. 7.) This difference reflects the roots of statistical reasoning in studying social phenomena, where it was presumed that special methods would be needed to tease order out of chaos (Hacking 1990). For nearly two centuries after Newton's *Principia Mathematica*, the natural sciences were not felt to have any such need (Porter 1986).

The reader should begin to see that the "success" of the natural sciences may be an artifact of their relatively loose accounting procedures. For example, the rhetorical force of cost–benefit analysis can be explained largely in terms of people coming to think that the rigor of the cost–benefit calculations implies a similar rigor in the method by which numbers are assigned to variables in the relevant equations (Porter 1995). This fallacy is only partly responsible for the natural sciences' success. Measuring success is not only evaluating cases in a certain (perhaps pseudo-rigorous) way, but also setting the standards of evaluation so that the successes can be easily seen against a backdrop of insignificant failures. Here, too, a double standard exists for the natural and social sciences. We are typically satisfied with the effects and entities that natural scientists, especially physicists, reliably generate in their labs (however unrelated to our experience of nature these things may be). In some vague way, we accept the idea that such artifices unlock the nature's secrets. Ultimately our marvel at the artifices seemingly compensates for the vagaries involved in translating the laboratory's "internal" validity into the real world's "external" validity (cf. Fuller 1989: pt. 3).

Yet awe is hardly present in the public reception of social science. If it were, then B. F. Skinner's intricate schedules for shaping pigeon behavior should have inspired as much intrinsic fascination as the discovery of a new microphysical particle. Typically, social science research must not only provide explicit demonstration (not presumed), but also must demonstrate a contribution toward solving an important social problem (cf. Campbell and Stanley 1963). If such high expectations were routinely imposed on the natural sciences, they too would seem singularly unimpressive. Luckily for natural scientists, when the most is expected from their work, such as in the launching of a spacecraft or the deployment of a new drug, public scrutiny is lowest. When scrutiny is highest—that is, when an experiment is closely observed by a host of interested lay parties—the laboratory spectacle is taken to be largely its own reward. Herein lies the full secret of the success of the natural sciences: They *inversely* vary the levels of expectation and scrutiny.

From these last remarks follows the second point about the nonepistemic route to epistemic privilege. Tellingly, for both the humanistic and the experimental disciplines to be seen as having performed the same "social function of science," one must quickly add that the sense of "worldly power" attached to these two forms of knowledge is rather different. Why not, then, simply deny that there is any interesting sense in which medieval *scientia* and 20th-century "science" have played the same role in society? Simply, science and *scientia* are two institutions with etymologically continuous names that perform rather different social functions in rather different societies. Accordingly, might the very idea of a social history of knowledge production, which extends from ancient Greece to the present, be a figment of the sociological imagination fostered by an illusion of what Wittgenstein would call "surface grammar"?

THE ELUSIVE SEARCH FOR THE SCIENCE IN THE SOCIAL SCIENCES: DECONSTRUCTING THE FIVE CANONICAL HISTORIES

My argument has pursued some ways in which the scientific study of science may reveal that there is no science to study. After examining the recent deconstruction of the demarcation criteria problem, I considered the Baconian Virtues as a basis on which the attribution of "science" could be made to a social practice. But this approach throws into doubt both the existence of any univocal conception of science and any univocal conception of power in terms of producing real-world consequences. The nature of those consequences and their social function have changed considerably over time. Indeed, at the end of the last section, these changes left us wondering whether a continuous history of science can be had at any level of analysis. I now argue the affirmative case, using as my linchpin the idea that the ultimate ground for the "Knowledge Is Power" equation is *rhetorical*. The thread running through the history of science from the Greeks to the present day is that people come to be convinced that particular forms of knowledge are embodied in the world—in skillful people and crafted goods—and are, in that sense, the hidden sources of power over the world.

Rhetoric works only on "receptive audiences," and experiment works only given the proper "initial conditions." Therefore, the only sort of power we can be sure that a form of knowledge generates is an interest in producing particular effects. People come to be convinced that certain deliberately staged events are exemplars of the knowledge-power nexus, that more "natural" events are to be interpreted charitably in terms of these exemplars, and that potentially troublesome events are to be ignored altogether. Rhetoric declined as a discipline during the Scientific Revolution as people came to newly scrutinize the relation between rhetoric's claim to knowledge and the real-world consequences of possessing that knowledge. However, rhetoric is not alone in being vulnerable to this critique. Social constructivists argue that experimentally derived knowledge in the natural sciences appears epistemologically sound only because we have learned to turn a blind eye to the many times when avowed methodology and actual practice diverge (cf. especially Collins 1985; Woolgar 1988b).

This last point has serious reflexive implications for how the history of the social sciences is conceptualized. On the one hand, the perennial need to persuade, or move people generally, is a robust social fact worthy of scientific treatment. On the other hand, if we believe scientific treatment of this phenomenon is possible, that is because we have been persuaded to see knowledge as having been embodied in particular ways on particular occasions. One clear consequence of this reflexive tension is that histories of the social sciences tend to self-deconstruct. The closer these histories get to detailing the knowledge-production process, the more the authoritativeness of their accounts is jeopardized. As a result, the reader implicitly receives the analytic tools to subvert the distinction between knowledge as being *about* the world and knowledge as being *in* the world. However, this process serves only to cast aspersions on the accounts including (ironically) their generalizability to the natural sciences. Yet if the history is written so that knowledge is exempted from explicit consideration as a social fact, then the reader is sustained in conceptualizing knowledge in disembodied terms. Thus, the social sciences can, at most, draw on the epistemic authority of the natural sciences. Managing this tension is the task of the social sciences' *canonical histories.*

Disciplinary practitioners write canonical histories with the express purpose of painting a panglossian picture of disciplinary development. First, true to Dr. Pangloss, this definition does *not* mea, that the events recounted in the history are, when taken by themselves, exemplary. For example, the history of political science is canonically emplotted as successive attempts to recover the original (largely Greek) unity of theory and practice from successive practitioners of the science. Indeed political science is as close as one could get to a discipline whose history has been written by the *losers*. (I have elsewhere called this "Tory History": see Fuller 2000b: Intro.; Fuller 2002b; Fuller 2003a.) Second, this definition does not mean that a canonical history must tell a story of ever greater scientificity. On the contrary, the author may be forced to conclude that the discipline has not, on the whole, enhanced its scientific credentials—or perhaps even its disciplinary autonomy. Yet the author would try to show, in proper Panglossian fashion, that "it has all been for the better." The author, for instance, may find that the discipline now recognizes the inherent richness of the phenomena under study, which in turn encourages blurring disciplinary boundaries. Authors of canonical histories are disciplinary partisans entrusted with structuring vast information for use by fellow partisans and sympathetic onlookers. Accordingly, one would expect to find the authors fusing their own perspectives with those of their subjects. A tendency then follows to impute to a historical personage foreknowledge of the role that her actions ultimately played in the formation of the discipline.

I elaborate three sorts of questions that may be posed of canonical histories and offer brief accounts of the canonical historiographies of five social sciences: anthropology, sociology, political science, economics, and psychology. These accounts are intended to guide the reader interested in answering the three sets of questions, but I do not pretend to have provided conclusive answers here. At the end of the accounts, however, I sketch a case in which impressions made on readers, inscribed in two histories of roughly the same vintage and scope, helped determine which discipline gained control over a contested domain.

1. Did the practitioners of these disciplines think that the definition of their discipline rested on its resemblance to an exemplar of science? Could one have a well-defined yet nonscientific discipline of, say, anthropology? Or would a nonscientific anthropology be essentially indistinguishable from other humanistic studies? In short, is every disciplinary boundary question also an implicit question of demarcation in the sense that interested the positivists and Popper? My own view suggests an answer of yes.

2. Do these debates occur at a time when it is unclear which disciplines are exemplars of science? Did all of the defenders of "science in the social sciences" propose to construct their discipline on the model of experimental physics? This "received wisdom" has been subject to an interesting systematic treatment. Peter Manicas (1986) argues that the foundational debates in the social sciences presupposed the faulty self-understanding that natural scientists had of their own epistemic pursuits, most of which were justified in terms of misunderstandings of what Newton had achieved.

3. Do the differences between earlier and later historical accounts reflect a difference between the relative openness and closure of the disciplinary boundaries? I would expect an answer of yes to the following three elaborations of this question: Do the later accounts present a continuous narrative vis-à-vis a fragmentation of perspectives in the earlier accounts? Do the later accounts present the disciplines as more "internally driven" (i.e. fewer references to events in other disciplines or society in general as influential) than the earlier ones? Are contemporary concepts and theories attributed to disciplinary founders in the later accounts, even though these entities had not been named when the founders wrote?

The three sets of questions just enumerated can be answered in terms of the canonical historiographies surveyed. Each of the five disciplines presupposes a model of the human subject, which may be used as a de facto demarcation criterion for the discipline. The criterion is encapsulated in two questions.

1. The *ontological* question: Is the subject's behavior determined principally by things happening inside her or by things happening outside her?

2. The *epistemological* question: Is the subject typically aware of the things that determine her behavior?

The following paragraph provides an idealized survey of the answers that bring out the implicit terms of peaceful coexistence among the social science disciplines. The complexity of the actual disciplinary histories is then elicited in the rest of this section.

Since anthropology's subject matter is usually defined as incorporating the inquirer in some capacity, all four logically possible answers to the ontological and epistemological questions are conceptually permissible. However, the other four disciplines have characteristic biases. Emphasizing communal bonds, traditions, and norms, sociology tends to conceive of the subject as internally but subconsciously determined. Diametrically opposed to this conception is political science. The field's concepts of power and forces imply a subject who is externally determined, yet sufficiently aware to turn these entities to her advantage. Economics also presents a subject who is aware of her behavior's determinates, yet they are now internally defined entities such as utilities and expectations. Finally, from its inception as an experimental science 40 years before behaviorism, psychology has been fixated on the image of the atomized organism as a function of its environment. Consequently, psychologists presume that the subject is determined by forces outside her—forces of which she need not be aware. Although I have undoubtedly stereotyped matters somewhat, a good test would be to see how different social sciences handle cognate areas (e.g. "social psychology" is handled by sociology and psychology), especially the extent to which the differences in treatment are attributable to differences in conceptualizing the human subject.

Anthropology

Anthropology is now called the science of "culture" to highlight the discipline's commitment to studying the humanly mediated, "artificial" (as opposed to "natural") features of reality. However, Edward Tylor's original focus on "culture" was meant to stress the habitual character of the human condition, which signaled our evolutionary continuity with the rest of the animal kingdom (cf. Lévi-Strauss 1964: Chap. 17). From these two understandings of culture come the two main research sensibilities, christened by Marvin Harris as the "emic" (symbolic-idealist) and the "etic" (ecological-materialist) traditions.

The emic tradition, illustrated by Malefijt (1974), portrays anthropology as the general study of humanity with quite open disciplinary boundaries and a commitment to methodological eclecticism. The etic tradition, illustrated by Harris (1968), portrays anthropology as increasingly turning to natural scientific models (borrowed especially from systems ecology and evolutionary biology) to overcome the folk beliefs of our own and other cultures. Each sort of history tends to blame the excesses of the other for anthropology's notable embarrassments. For example, emic historians locate the roots of racism in etic attempts to reduce social to biological phenomena (e.g. Social Darwinism, ethology). Yet etic historians trace racism to the emic tendency to reify perceived cultural differences (e.g. Romanticism, Nazism). Moreover, the excesses seem to go both ways. If current outlooks fault etic anthropologists for importing Western standards of efficiency to evaluate the rationality of native practices, a more ethnographic approach does not necessarily remedy matters. After all, in the 19th century, racism was more virulent among first-hand observers of native cultures than among armchair systematists like Tylor, who tended toward *a priori* pronouncements about the unity of humankind admittedly to justify bringing the natives up to Western standards (Stocking 1968).

Both camps of anthropologists have taken the "reflexive turn" and thought of their discipline as a culture in its own right (Clifford and Marcus 1986). Anthropologists' previous lack of reflexivity was a by-product of their studious avoidance of charges of ethnocentricism. Interestingly, the objectively oriented etic anthropologist has been somewhat more reflexive than her relativistically oriented emic colleague. Although the etic anthropologist typically admits her own disciplinary culture's presence (and its superiority to the natives'), the emic anthropologist presents herself as ideally a (universal) mirror of whichever native culture happens to be under study. The emic self-image usually presupposes that the natives see just as much of a difference between their culture and the anthropologist's as the anthropologist sees. Ironically, anthropology's recognition of its own presence in the study of culture has coincided with the global interpenetration of cultural boundaries through mass communications (i.e. "globalization"). In turn, many anthropologists wonder whether the very idea of their discipline as a coherent culture might be an anachronism (Marcus and Fischer 1986).

Sociology

Lévi-Strauss (1964: 354) distinguishes the methodological orientations of Emile Durkheim and Bronislaw Malinowski in a way that is emblematic of the ontological difference between sociology and anthropology. Sociology studies society as a thing, whereas anthropology studies things as social. Despite Talcott Parsons' (1951) efforts, sociology has forsaken any pretension of providing a unified theory of the social sciences in favor of pursuing an autonomous disciplinary course. A conspicuously provincial sense of the scope of "social theory" results—namely, whatever issues flow from the pens of "theoretical sociologists," who have themselves become largely autonomous from empirical sociological research (cf. Giddens and Turner 1987). Moreover, canonical histories of the field (e.g. Martindale 1960; Bottomore and Nisbet 1977) have consistently presented an endless proliferation of "schools of thought." Systematic works, starting with Parsons (1937), resemble medieval encyclopedias even to the point of being criticized for not having included all the major schools. Contemporary systematists, such as Collins (1988), are preoccupied with delimiting each school's relevance within an all-encompassing picture of society.

Yet the canonical histories portray these schools as having some rather peculiar qualities These schools, for instance, are neither necessarily linked to particular academic lineages, nor are they populated by unique sets of members. Such is the case of "structural functionalism," whose founders supposedly include Weber and Durkheim, and sometimes even Marx. In addition, sociological schools are presented as laying claim to the entire subject matter of the discipline (much like Kuhnian paradigms). More likely each school emerged from, and is most plausibly situated in, only part of that subject matter. This characterization confers on sociology a spurious sense of internal division—one due more to lack of communication than to genuine disagreement. For example, the microperspective of Simmel's formalism and the macroperspective of Durkheim's functionalism appear in conflict. Yet this view results only if one neglects the fact that Simmel is usually talking about specific sorts of face-to-face interaction, while Durkheim is talking about systemic features that are necessary for any social interaction to take place. This example points to a major reflexive weakness of sociology: The discipline is not well constituted as a social unit. To classify schools of thought by common themes, rather than by academic lineages, suggests that the same ideas can arise independently in different social settings. In turn, the extent to which knowledge is socially determined gets called into question. Indeed, histories of the field typically begin by invoking "the experience of modernity" shared by such otherwise disparate figures as Marx, Weber, and Durkheim. Until an account explicitly renders how an abstract experience like "modernity" can emerge from the quite different social settings in which these thinkers lived, the locus of sociological inquiry will remain in doubt.

Political Science

Canonical histories (e.g. Wasby 1970) make political science seem like the social scientific equivalent of comparative morphology (i.e. devoted to producing "middle-range theories" consisting largely of correlations between types of political structures and functions). This image keeps alive the spirit of political science's inauspicious origins as the loser of the *Methodenstreit* over the scientific status of political economy that took place in Germany in the 1890s (Manicas 1986: Chap. 7). Today's political scientists descend from proponents of the "historical method" in economics. These researchers were interested in assembling the social record of various nations to discover statistical tendencies that would simultaneously contribute to social science and social policy. Moreover, histories of the field vacillate over whether the "middle range" is to be subsumed under more general theories of human nature or to be further deployed as tactics in political practice (cf. Ricci 1984).

Strikingly, political scientists who tried to transcend the middle range by using interdisciplinary research as a vehicle for modeling political complexity (e.g. Harold Lasswell, Karl Deutsch, Murray Edelman) have been repeatedly treated with respect during their professional careers, but promptly forgotten thereafter (J. Nelson 1987). Indeed, the most persistent pursuit of "grand theory" in political science has taken the form of a subterranean "classical tradition" that follows Machiavelli, Hobbes, and Locke. This tradition derives political counsel directly from a general account of human nature, bypasses the middle range entirely (cf. Skinner 1987), and regards 20th-century political science as little more than a temporary aberration. By mid-century, the subterranean tradition was associated with such cultural conservatives as Michael Oakeshott and Leo Strauss. However, now it has taken on a more progressive cast as the study of politics seeks to reestablish its ties to civic responsibility (e.g. J. Nelson 1986).

The need to have an independent object of inquiry politically incapacitates political science's practitioners (Karp 1988). In the case of the dominant tradition, "political realism" portrays the politician as someone who mediates conflicting "forces." The macrohistorical nature of these forces resists the intentions of the individuals who serve as its vehicles (cf. Keohane 1986). These forces, often called *ideologies*, are discussed as if they were reified states of mind (e.g. "terrorism" as a contemporary political force). However, more like physical forces, political ones may be constrained by particular human interventions, but never fully eliminated. Thus, not only the 20th-century political climate, but also the ontological demands of political science, serve to stereotype politics as a Bismarckian *Realpolitik*, where the goal is always the containment, but never the resolution, of political differences.

The subterranean tradition, in contrast, falls back on an equally incapacitating eschatological model of political history. Thus, the scholar keeps alive the classics until "the time has come" to reunite political theory and practice (Gunnell 1986). Of all the social sciences, political science has the severest cross-cultural identity problem. Should it be housed in the law school (as in Germany), the school of public administration (as in France), or the college of liberal arts (as in the United Kingdom and the United States)?

Economics

Histories of economics seem preoccupied with the scientific status of the discipline. Earlier histories (e.g. Robbins 1937) portray the field's progress in terms of the gradual conceptual, if not practical, separation of the economist's value judgments from those of the economic agents. Later histories (e.g. Blaug 1978) shift the motor of progress from this sort of conceptual analysis to the increasing willingness of economists to submit their theories, however value-laden they may be, to quantitative tests. This shift matches a move in the preferred philosophy of science from positivism (via Max Weber) to Popperianism and fits a trend toward portraying Neoclassicism as having definitively triumphed over Marxist, Keynesian, and other institutionally oriented economic paradigms.

The reflexive worry that dogs the history of economics turns on the tension between the economist's rationality and the economic agent's rationality. Classical approaches presume that the object of economic inquiry is the natural order that emerges from the sum of individually rational and self-interested agents. Yet Western economic instability during the period coinciding with the history of economics would indicate that, as a matter of fact, no particular sense of economic order or natural "equilibrium" exists (Deane 1989). Is this because economic order can be maintained only through intervention (à la Keynesianism) or because the natural order has been subject to external interference (à la Neoclassicism)? In either case, the economist must apply her own scientifically defined sense of rationality to compensate for the inadequate rationality displayed by the agents (Lowe 1965).

The need for two senses of economic rationality is usually attributed to differences in economic scale (Georgescu-Roegen 1970). Individuals, then, are good utility maximizers relative to their own cognitive horizons, subject to periodic revision, which unfortunately are insufficient for understanding the entire economic system (Keynes 1936). The economist's special sense of rationality occurs both retrospectively and prospectively. When regarding the past, economists have become particularly adept at interpreting seemingly noneconomic practices, such as in the emergence of an elaborate system of patent law to provide both stimulus and protection to innovation, as latently economic (cf. Lepage 1978: Chap. 3). When regarding the future, economists exhibit their rationality in using statistics as "indicators" of long-term trends that normally escape the deliberations of the economic agents (McCloskey 1985: Chap. 9). Consequently, economists have entered the political arena more readily than other social scientists, including political scientists. Simultaneously, however, the public demand for economists reflects the sense in which the concept of economic rationality is an artifice of economic science. In masking the manufactured character of economic rationality behind spurious analogies with idealized closed systems in the physical sciences (Mirowski 1989), economists risk a communication breakdown between themselves and the public (cf. Klamer et al. 1988).

Psychology

Canonical histories of psychology conform best to the canons of ordinary historical scholarship in terms of pinning down who did what when and where (especially Boring 1957). This approach may result from the field's strong sense of academic lineage, owing largely to its origins in the German research university. Thus, students successively pass through a laboratory under the directorship of a major professor. Psychology, then, presents itself (ironically) as the social science that is most self-consciously a social construction, even to the point of conducting research under distinct identities (i.e. "experimenter" and "subject") in a distinctive setting (i.e. the lab; cf. Morawski 1988). Unlike sociology, schools of psychology are clearly identified with nonoverlapping sets of people. The cross-fertilization of schools typically involves the member of one school spending time in another school's lab and then revealing the effects of that contact in subsequent work.

Recent histories (e.g. Schultz 1981), however, have had difficulty accommodating post-World War II developments to the canonical lineages. As a result, these histories are the least reliable in the social sciences as guides to current research. Psychology may have become too technical for its own historians to fully grasp, or, more likely, that the field has implicitly relinquished the narrative principle of earlier histories. That is, psychology aims to provide no longer a general theory for the entire human psyche, but merely special theories of perception, cognition, motivation, and other processes that can be read off individual behavior.

Moreover, these processes are increasingly treated not as uniquely human, but rather as instances of still more general processes studied by other disciplines (e.g., the modeling of cognition on computers, the subsumption of motivation under animal ethology). Indeed psychology' recent fragmentation may vindicate Foucault's (1970) prognosis for "the death of man." Questions arise, then, whether the research programs that today travel under the name of psychology are unified by anything more than a common physical object (the individual human) studied in a controlled physical environment (the laboratory). This conclusion would be especially ironic for a discipline that—starting with Wundt's distinction between immediate and mediate experience and culminating with B.F. Skinner's strategy of treating the behaving organism as a physiological "black box"—has repeatedly taken great pains to distinguish its subject matter from that of the physical sciences.

HOW ECONOMISTS DEFEATED POLITICAL SCIENTISTS AT THEIR OWN GAME

Let us consider a case in which differences in canonical histories appear to have contributed to one discipline's overtaking from another discipline's a mutually contested domain. The domain in question was *politics*. Here politics entails an amorphous mix of classical philosophy, legal history, and folk sociology typically studied by aspiring civil servants at British universities in the 19th century (Collini et al. 1983). The contesting disciplines were economics and political science, and the outcome has had far-reaching consequences. Economists in the 20th century came to dominate the higher echelons of "policy," be it as art or science, even though political science seemed to provide a richer and more relevant background for the aspiring policymaker or analyst. Indeed some political scientists study economists for their policymaking skills (e.g. Galbraith 1988), a compliment that an economist would never think of repaying. Harold Lasswell (1948: 133), America's leading 20th century political scientist, bemoaned his discipline's lack of "public image." Even Harold Laski, who succeeded Graham Wallas in the Chair in Politics at the London School of Economics, felt compelled to call himself an economist to enhance his credibility. Economics and political science epitomize stories of success and failure, respectively, in the art of discipline building, artful and artless ways of reading one's past into the future.

By the early 1900's, two books had become famous in Britain for presenting a "scientific" study of politics, Alfred Marshall's (1920) *Principles of Economics* and Graham Wallas' (1910) *Human Nature in Politics*. Given the diverse constituency for such a science—academics, bureaucrats, journalists, and practicing politicians—Wallas appeared to have the natural advantage. His book was shorter, easier to read, and showed signs of being written by someone who had spent time in Parliament and away from academia. Wallas had the advantage of networking from London while Marshall remained ensconced in Cambridge. However, the self-images that we have found to be characteristic of political science and economics can be equally seen in the historical perspectives that Wallas and Marshall, respectively, adopt toward the domain of politics. I first consider the stance that each takes toward his discipline's past. I then look at how each handles a contemporary rival for epistemic authority—psychology.

There are two ways to measure the control that Marshall and Wallas exert over the histories of their disciplines. The first way examines the relative ease with which past and present analogies are transferred to establish a continuous line of inquiry. On the one hand, we have Wallas' (1910: Chap. 4) Sisyphean struggle. He undertakes transferring the concept of democracy from the rationally tempered, homogeneous Greeks to the volatile, heterogeneous nations he finds in early 20th-century Europe. On the other hand, we have Marshall's (1920: Chap. 1) nimbler efforts. He shows that modern capitalist economies preserve what was worthwhile in precapitalist conceptions of trust and cooperation. Critics claimed that capitalism had led to increasing dishonesty, which had decisively corrupted the economic scene. Marshall countered that more opportunities to express dishonest sentiments had appeared, the extent of which has remained unchanged throughout history. In fact, Marshall struck a blow for both science and morality by refusing to accept at face value the 18th-century dogma that "private vices make for public virtue." Rather, he took, as an empirically open question, the economists' determination of who the true "captains of industry"—people whose enterprise made the decisive difference to economic growth—really were. They did not necessarily correspond to the Dickensian stereotype of the rapacious industrialist (Collini et al. 1983: 335).

The second way to measure control over the historical record is by examining the cooperation or resistance that authors receive from their predecessors. Quite telling in this respect is the contrast in metaphors that Marshall and Wallas use when they speak of "founding" a science. Marshall tends to see the process as a matter of "building an edifice." Wallas regards the process in terms of "clearing the ground." Wallas' image could be reasonably read as a precursor to Marshall's in some unified project. Significantly, however, they are writing at the same time, with presumably the same information on hand, which suggests a difference in the standards and expectations that the two authors bring to the task of founding a science. Clearly, Marshall's epistemic criteria are better adapted to the knowledge of his day than Wallas'. For his part, Marshall (1920: App. B) offers a brief but striking portrait of economics in the second half of the 19th century. He portrays economics as an international, cooperative enterprise devoted to completing the consumption side of the economic equation after the classical political economists had mastered the production side in the first half of the century. If anything, Marshall overplayed the cooperativeness of his contemporaries even with regard to their own understanding of what they were doing. Wallas (1910: Intro.) was once again less effective. His quite opposite attempt condemned the two major schools of political thought in late Victorian Britain—the bureaucracy-oriented utilitarians and the university-oriented idealists. He presented them as exemplifying the practice–theory split that perennially stymies the possibility of a science of politics. Even if just in his day, Wallas' charge served only to aggravate his potential allies within the political establishment and to discourage his potential allies outside of it (Collini et al. 1983: Chap. 12).

Marshall posed a direct challenge to psychology's contemporary authority. Economics required a wide scope to cover politics completely—namely, "men [*sic*] as they live and move and think in the ordinary business of life" (Marshall 1920: 14). Marshall argued that economics presupposed a psychological theory and method more fundamental than the ones normally treated by the discipline of psychology. But if economics accepted, without question, the psychic diversity of motivation, then the disciplines' quantitative basis would be undermined (as values cannot be calculated unless reducible to a common currency of utilities). Psychologists, then, reinforced the very difference between, say, smoking a cigar and drinking a cup of tea that Marshall needed to eliminate to render the two activities to economic analysis. Marshall's solution was to argue that smoking (a certain amount) and drinking (a certain amount) could be seen as functionally equivalent means to some common end. Thus, as long as one regarded psychological states as always transitional to some other state, then the possibility of exchanging the states at some rate was left open. The economist, as a result, had a distinct and important field of study—a field highlighting the agent's dynamic character in contrast to the more contemplative, hence less realistic, image depicted by the introspective psychology of Marshall's day.

Wallas, in contrast to Marshall, heavily relied on the instinct psychologies of William MacDougall and William James that were popular at the start of the 20th century. Wallas tried to argue that quasi-biological political impulses gave content and purpose to the rational calculations that economically oriented thinkers emphasized. Still, these impulses occasionally had effects on political behavior (e.g. riots and upheavals) that escaped calculation. However, Wallas ran into reflexive difficulty in trying to define the political. On the one hand, if politics' quasi-biological substance interrupts calculation too frequently, then the psychology of anyone attempting a rational science of politics (such as Wallas) should be held suspect. On the other hand, if these impulses rarely bubble to the mind's rational surface, then they can safely be ignored. This conclusion would lead us back to Marshall's vision of economics. As Marshall realized, Wallas' dilemma fed nicely into the commonsense notion of "politics" as "mere expedients." These were tactics that temporarily worked but failed to probe the deeper, long-term tendencies best handled by a deductive science like economics (Collini et al. 1983: 332).

At the outset, I suggested that the relative accessibility of Wallas' text should have given him a natural advantage over Marshall in shaping the future of politics as a field of inquiry. Indeed *Human Nature in Politics* was reviewed in a wider range of periodicals than *Principles of Economics*, including journals in disciplines outside of politics (e.g. *Ethics*, *Psychological Bulletin*) and even some highbrow magazines (e.g. *The Bookman*, *Saturday Review*, *Yale Review*). Although the general tone of Marshall's reviews was muted and respectful even when critical, Wallas' work elicited the entire gamut of responses—from adulation to sarcasm. Herein lies a hint of why Marshall succeeded and Wallas failed.

Marshall's book took great pains to accommodate potential opponents, even if by significantly reconstituting them in terms of his own project. As such, the book did not lend itself to either easy dismissal or easy appropriation. This move forced critical readers of Marshall's *Principles* into a dialectical corner. Since their own research agendas were likely incorporated into Marshall's disciplinary vision, critics disagreed on matters of detail rather than of overall conception. However, simultaneously, the book's systematic cast meant that they could not simply pick and choose the bits that suited their own purposes. Whoever wanted to deal with Marshall had to deal with him on his terms—although the terms were designed to be reasonably hospitable to the likely reader. Significantly, when Talcott Parsons (1937) made his initial stab at unifying the social sciences, he opened with an examination of Marshall's architectonic.

Wallas, in contrast, seems to have written *Human Nature in Politics* in an all too consumable and malleable way. Thus, reviewers presented Wallas' evidence, often drawn from recent newspaper stories, as striking in its own right, but without giving a clear sense of the claims the evidence was meant to support. As for the reception of Wallas by other disciplines, the reviewers responded enthusiastically. They believed *Human Nature in Politics* provided independent corroboration for positions that research in their own discipline had already established.

If we rely solely on such traditional epistemic criteria as explanatory breadth, predictive accuracy, and problem-solving effectiveness, economics and political science would probably not seem so far apart. Both disciplines are relative failures compared with the natural sciences. This judgment, although satisfying to philosophers of science, ignores some brute facts which suggest that the real value of bodies of knowledge lies in a different set of considerations. For example, economics resembles physics in being unrepentantly "academic." Both fields trade on ideal conditions and closed systems that bear little resemblance to the complexities of the actual world. Moreover, the authority of economics is not diminished by repeated failures to analyze costs and benefits accurately. Nor is the integrity of economic reasoning undermined by the ingenious ways, such as deficit spending and inflated currency, in which even accurate cost assessments can be finessed.

Where, then, lies the authority that economists exercise in government as policy consultants, especially compared to political scientists, the avowed experts in the area? After all, political scientists' aspirations to capture the daily complexity of the governing process, whether congressional voting patterns or public opinion change, stands in contrast, and seeming rhetorical advantage, to economists' abstract model-building tendencies.

However, first impressions, even though deceptive, persist through the histories of the two disciplines. Although survey research methods from political science have influenced the way data are gathered for policymaking, the actual policymaking is put in the hands of the economists. Why? To simplify the story considerably, much turns on the economists' skill in rhetorically converting their epistemic liabilities into political virtues.

Economists manage to portray their idealizations not as the false images of reality that, strictly speaking, they are (a point that physicists readily concede about their own idealizations; cf. Cartwright 1983), but as normative standards against which reality can be judged and toward which reality can be corrected. Thus, the free market model becomes less an empirically inadequate picture of economic activity and more a desirable goal toward which policy ought to be directed. Crauwford Goodwin (1988) observed that economists moving from the academy to the policy arena signal this normative turn by exchanging metaphors from physics for those from medicine: Abstract equilibria become symptoms of a healthy market. The self-assumed imperative of bending the real to the ideal is, for the most part, lacking from the assertions of political scientists. Stressing the causal significance of locally varying factors makes political scientists more guarded about giving any general advice (cf. Almond 1989: Chap. 2). Consider, even, the rationality assumption that economists typically make of agents. In contrast, political scientists, instead of measuring the agent's shortcomings from a superior vantage point (as economists might), remind themselves that the full rationality that belies the surface discontinuities in an agent's behavior remains unfathomed (Keohane 1988).

The difference between economists and political scientists occurs both at the level of practice and at the level of theory. Unlike Wallas, Marshall refused to get caught up in the tariff policy debates of his day. Indirectly this position served to increase Marshall's political clout by enabling him simultaneously to claim the high moral ground and to avoid any immediate tests of his hypotheses (Collini et al. 1983: 336-37). Economists have learned the following lesson from Marshall's practice: The more long term one's empirical perspective is, the more it becomes a *de facto* normative perspective. Accordingly, one's rhetoric shifts from talk about what will happen next to talk about whether the time is right to assert what will happen next. Talk cannot temporize forever, but economists have dictated the times and terms in which they pronounce on policy (cf. Galbraith 1988). A sign of the economist's "expertise" is the ability to manage the circumstances under which one exercises power—perhaps more than the actual power itself (cf. Fuller 1988a: Chap. 12). Rhetoricians have long recognized this phenomenon as kairos (cf. Kinneavy 1986; C. Miller 1992, 1994). In another instance, social psychologists have focused on manipulating the moment of decision as an effective tool for a minority to use to determine an outcome (Levine 1989). In the economist's own terms, such meta-management allows her to minimize the hidden "process costs" of making policy statements that turn out not to work; for example, losing credibility when making future pronouncements (cf. Sowell 1987: Chap. 4).

The economists' rhetorical deftness is actually built into some of the core concepts of economic science. Consider two such concepts: *scarcity* and *unintended consequences*. Of course the very need for economics arises from the phenomenon of scarcity. Scarcity is portrayed as the ultimate reality indicator, the material equivalent of logical contradiction (Xenos 1989). Two wants cannot be satisfied at once. One must either change the logical space in which the decision is made by expanding the time frame or range of resources, for example, involved in the decision. Or, as economists more typically advise, hold the space constant and make tradeoffs between the conflicting demands. In their more metaphysical moods, economists have argued that scarcity is the source of real choice for a world of pure abundance would generate an unmanageably large number of possibilities for satisfaction. Consequently, free will can be experienced only if the range of available options is sufficiently restricted so that the agent feels that deliberating on them can determine her course of action. Informed by the economist's sense of meta-management scarcity becomes a rhetorically powerful idea. The economist can "structure" a decision so that policymakers feel they are freely choosing from among an array of options. Still the economist knows that the outcome of any of those choices is likely to fall within a narrow range of events.

By contrast, although the appeal to scarcity closes the logical space of action, the appeal to unintended consequences opens up the space at a crucial juncture, when the economist needs to temporize. In appealing to unintended consequences, the economist would show concern that hasty policy intervention may unwittingly disrupt the self-corrective processes of the market that take place over the long term, even though the signs of recovery remain, at best, ambiguous at the moment.

Hirschman (1977) identified what may be the original bifurcation of economists and political scientists' attitudes toward the rationality of agents—attitudes that stem from Plato and Aristotle, respectively. Capitalism was clearly predicated on a view of human psychology that enabled agents to calculate their "interests" and act on them accordingly. However, it remained to be seen whether such interests were a philosophical fiction or some suitably sublimated version of the "passions" that were known to motivate human behavior, normally in quite irrational ways. The proto-political scientists took the Aristotelian line that the wayward nature of the passions made them ontologically ill suited to numerical representation. As a result, agents could not be expected to make consistent utility assignments in their thinking. By contrast, the proto-economists took this numerical intractability as a moral failure. Specifically, agents needed to engage in certain forms of self-discipline and transform their passions into mathematically focused interests. With this micro-Platonism, the groundwork for a "moral science" was laid. Over the next two centuries, links were forged among the determinate outcomes of mathematical procedures (e.g. cost–benefit analysis), the correspondence of those outcomes to real-world events, and the decisiveness of the course of action dictated by those procedures. As we have seen in this section, such a difference appeared in the psychological arguments that Marshall and Wallas used to stake claims for economics and political science as sciences of the political.

THE RHETORIC THAT IS SCIENCE

Canonical histories of the social sciences must walk a fine line between subverting their own epistemic status and reinforcing the status of the current scientific exemplars. This line may be measured in terms of the awkwardness, if not downright silence, with which these disciplines confront the possibilities of, respectively, an anthropology of science, a sociology of science, a political science of science, an economics of science, and a psychology of science. By way of example, consider the psychology of science.

More than any other social science, psychology had to negotiate its disciplinary identity between the sciences and the professions. At the dawn of the 20th century, such notable German experimentalists as Oswald Kuelpe and Hugo Munsterberg did not see such a sharp line (Ash 1980). Kuelpe welcomed the housing psychology departments in medical schools, as the "normal" counterpart to psychiatry. For his part, Munsterberg, once he moved to the US to found Harvard's psychology laboratory, innovated the social role of psychologist as "expert witness" in legal proceedings. However, the professional status of psychology has remained controversial over the last 100 years as defenders of a pure "scientific" psychology have worried that the premature deployment of psychological techniques could do more harm than good. Without passing judgment on these suspicions, psychology's easy ability to generate fear is undoubtedly connected with techniques borrowed from medical practice. These techniques have a greater capacity for leaving traces both *about* and *on* individual human bodies than, say, the abstract aggregative techniques of economics.

Psychology claims to offer authoritative knowledge about the nature of the mind. Two preconditions for exercising such authority are that the psychologist can explain her own mind better than a nonpsychologist can and that the psychologist can explain people's minds better than the people themselves can. Although psychoanalysis, to its credit, has been singular in its efforts at meeting the first precondition (i.e. by requiring that the analyst be analyzed before analyzing others), every school of psychology has claimed to have met the second precondition. These claims typically assume that scientific reasoning is especially good at explaining things. But have psychologists explained the source of science's explanatory power? If science were like any other field psychology might study, psychologists' success at explaining scientific reasoning would depend on regular access to the scientific community. Such access would require at least temporary control over what scientists do. For example, scientists might be routinely sequestered like jury members to undergo various psychological tests and experiments to elicit salient patterns of reasoning. The institution of such a procedure would signal the power that the psychologist had to draw scientists away from their work and into her own.

As a matter of political fact, science is *not* like any other field. Thus the psychologist of science can mobilize little more than undergraduates on a regular basis—and then at universities (e.g. Bowling Green State [Tweney et al. 1981] and Memphis State [Gholson et al. 1989] Universities) far from the centers of major scientific research. Given this state of affairs, psychology of science is considered a "fledgling" or "marginal" specialty. In canonical histories (e.g. Boring 1957), however, events taken to have been pivotal in the founding psychology turn out to be moments, such as the discovery of the "personal equation" in astronomical measurement, in which science is needed to counteract the *shortcomings* of scientists. The methodological control that psychologists have subsequently gained over mental phenomena—via sophisticated experimental designs and inference strategies—has come from learning how to systematically check their own mental biases and limitations. Indeed if psychology is more advanced in the range of methods it offers rather than in the results it has reached by those methods, that would suggest that most of psychology has been psychology of science. However, the story of psychology would then be told as the development of more clever compensations for the cognitive weaknesses of scientists, not as the cumulative growth of knowledge by exemplary cognitive agents (cf. Campbell 1989).

Emplotting the history of psychology as a psychologically involved activity heightens certain tensions regarding its scientific status that would remain neglected if psychology were treated as merely "about" minds. Yet with the exception of Merleau-Ponty (1962, 1963; cf. Rouse 1987), little recent philosophical treatment exists of the implications of the proposition that of knowledge is both *about* and *in* the world. Indeed among analytic philosophers the post-Kantian tendency to regard matters of epistemology as separate from matters of ontology runs deepest. Accordingly, before attempts to reveal the embodied character of knowledge are likely to be generally persuasive, we must return to a historical point when a highly familiar form of knowledge—one that we routinely treat as being about, but not in, the world—came to be embodied.

I take this embodiment to be a twofold process that is intimately connected with the *internalization* and *externalization* of standards. By "internalization," I mean that people are capable of making judgments about new cases even without the presence of standards in the environment. Drawing on the social behaviorism of George Herbert Mead and Lev Vygotsky, Rom Harré (1979) usefully called this process "privatization" (which, I believe, most of cognitive psychology is trying to get at in its characteristically reified way). By "externalization," I mean that people come to forget that earlier encounters with standards have structured their later judgments. This process leads them to confer an "independent reality" on what is, in fact, a subliminally standardized world. Patrick Heelan (1983) has given the best phenomenological treatment of these realist intuitions, while Donald Campbell offers the best social science explanation for them (Segall et al. 1966).

The familiar form of knowledge in question is experimental knowledge, the basis of the distinctive power currently wielded by the natural sciences. Shapin and Schaffer (1985) present the 17th-century debate between Thomas Hobbes and Robert Boyle on the epistemic authority of experiment as the originating myth of the modern knowledge-power nexus. For their own part, Hobbes and Boyle were clear that the ultimate objects of prediction and control were human beings. Still Hobbes regarded Euclid's geometric proof as the exemplar of science. As a result, he explained the source of knowledge's power in terms of an explicit reasoning process, which enabled the scientist to communicate directly with his audience. The audience could either agree or disagree with the scientist, but everyone could be clear, because there was no asymmetry between the scientist's and his audience's knowledge of the situation. Of course this "ideal speech situation" did not guarantee the resolution of any disagreements that arose, especially if the scientist's audience is large and heterogeneous. Indeed this aspect explained to Boyle's satisfaction why scholasticism had failed to advance the frontiers of knowledge. Even Hobbes used reason's inability to resolve such disputes as grounds for introducing a strong sovereign to "channel" the discussion in a more "productive" direction.

Boyle leapt on the authoritarian turn in Hobbes' thought, citing it as a *reductio* of the dialectical approach. Boyle thought that "the way of talk" promoted disagreement at ever finer levels of analysis—until the heavy hand of the sovereign issued a final judgment. Instead, Boyle appealed to a coarser medium of epistemic exchange—one that could elicit assent from people who have verbally different accounts of the situation. Experimental observation did the trick once the audience was instructed on viewing the experiment in the right way. For Boyle could secure not only mass assent but also an assent, that seemed uncoerced. Observation, then, could be made automatic through instruction, and hence made to appear "natural."

In this regard, Boyle's "experimental method" is a version of the rhetorician's "method of places." Here the experimenter is taught how to embody knowledge in the world (i.e. how to code the concrete situation in theoretically significant terms) so that it can be recovered later (e.g. when observation is compared with theory after the experiment is done). For his part, Hobbes fully realized the rhetorical character of Boyle's experimentalism and did not object to using rhetoric to secure agreement on matters of opinion. However, Hobbes wanted the use of rhetoric to be employed self-consciously on the people whose agreement was being sought. Indeed he tended, in a Clausewitzian manner, to see both the explicit exchange of reasons for positions and the naked use of force to stop debate as two ends of the same continuum. In both cases, the audience could trace the source of the power being wielded and respond appropriately. In that way, the source became *accountable*. By contrast, Boyle's trained observer forgot all such traces, treating his own response as natural under the circumstances, unmoved by the artifice of rhetoric. For his part, Boyle was interested simply in embodying experimental observation as natural knowledge. Yet an entire system of rhetorical associations have come to make the cluster of practices we call "science" appear part of a seamless whole with transparent access to some natural reality.

We rather automatically presume that a mutual correspondence exists between the words and deeds of science—the two operating as convergent indicators of some "natural kind" that is the putative subject matter of that science. Moreover, the sui generis character of this natural kind is mirrored by the science's character as an institution seemingly separate from, and unanalyzable in terms of, other social institutions. However, the mounting anthropological and sociological accounts of science reveal that these mirrored wholes are more perceived than real. The laboratory, in this instance, is simply a point of confluence for structures and practices found in other, normally unrelated parts of society (cf. Latour 1987). Each structure and practice is the proper study of one of the social sciences, but is paradigmatically studied in a nonscientific social setting. For example, the self-interestedness of scientists crucially contributes to the growth of knowledge (cf. Hull 1988) yet the proper analysis of that trait will be found not in a study of science, but rather in a study of business behavior on which scientific behavior is parasitic.

As I have argued here and elsewhere (Fuller 1989: Chap. 2), science does not have an essence. Science is simply the sum of disparate strands of society. These strands are mutually reinforced in specific places, both by the scientists' behavior and by our learned perceptual bias to ignore the disparateness and to see science instead as embodying a common form of knowledge that is a source of worldly power. Because the social sciences continue to be perceived as only partially autonomous from the societies that support them, their histories provide a special opportunity—matched only by the emergence of experiment as a legitimate source of natural scientific knowledge—to examine the processes by which knowledge tries to be *about* the world without drawing undue attention to its existence *in* the world. Until we take seriously the thesis that knowledge inhabits the same world as its putative objects, we cannot fully appreciate the implications this point has for the legitimation of our knowledge enterprises.

THOUGHT QUESTIONS

☙ Fuller opens by offering a brief counterfactual history of how the natural sciences might have developed. Does this counterfactual suggest that the more humans know about a particular topic, the less they are motivated to pursue it? Or that "remoteness" from a given subject makes an objective account easier to provide? What examples support, or run counter to Fuller's assertion that, "more knowledge is better than less [knowledge] only after science is in place and quality controls have thus been instituted for the production of knowledge"?

☙ What is "rhetorical impoverishment"? Fuller asserts that our "remoteness" from a given subject results in fewer ways to express it. Does the "power" of science result, in part, from the discipline exerted over its language and, hence, fewer linguistic choices? Does the form of linguistic or rhetorical discipline identified by Fuller result in the formulation of academic disciplines? Do academic disciplines form and regulate discourse, or must both the discipline of the language and the field of study be in place for scientific investigation to begin in earnest?

☙ Do you agree or disagree with Fuller's assertion of the importance of studying disciplinary boundaries? What were the concerns of philosophers of science in studying disciplinary boundaries? What is Fuller's assessment of this project? Given Fuller's conclusions, what are the possibilities for interdisciplinary interpenetration? What are the possibilities for a philosophy of science?

☙ What is the problem of demarcating science from nonscience?

☙ What are the "Baconian Virtues"? How do these virtues maintain disciplinary order?

☙ Fuller suggests that there is a nonepistemic way to confer epistemic privilege; that is, there are other means by which we can confer favor on the processes by which knowledge is produced. What are they? What lessons, according to Fuller, did the sciences learn from the humanities to assert and maintain its privilege? How did the rise of experiment change the character of knowledge?

☙ What remedies have the humanities sought over the last century to maintain their "worldly power"?

☙ How do academic disciplines or fields determine what counts as (theoretical or empirical) "success"? How are failures determined and documented? How do publications in physics and psychology, for example, set up an accounting of their experimental successes and failures?

☙ How does Fuller define a "canonical history" of a discipline? What rhetorical purposes do the canonical histories achieve in presenting scientific or social scientific knowledge as either *about* the world or *in* the world? What are the ontological and epistemological questions that drive the five disciplines that Fuller surveys?

☙ What are the "etic" and "emic" traditions in anthropology? How do the canonical histories portray anthropology as a culture in its own right? What is anthropology's attitude toward society?

☙ What are the qualities of the canonical histories of sociology? How do the canonical histories portray sociology as a society in its own right? What is sociology's attitude toward society?

☙ What are the "middle-range theories" that appear to comprise political science? What is the result of a reflexive examination of the politics of political scientists?

☙ How are the concerns of the canonical histories of economics different from other social sciences? How do these concerns translate into a place in the political arena for economists?

☙ Fuller claims that psychology is, "the social science that is most self-consciously a social construction." How does this reflexive awareness affect the histories written about the development of psychology as a discipline?

☙ What accounts for the differences between the reception of Marshall and Wallas? What rhetorical advantages do economists seem to have over political scientists? How does this advantage play out in theory as well as practice?

☙ What might a social science of science—a psychology of science, an economics of science, a sociology of science, a political science of science—reveal about the way in which the natural sciences are both in and about the world? What opportunities might the canonical histories of the social science provide for examining how the social sciences are both in and about the world?

5

Sublimation, or Some Hints on How to Be Cognitively Revolting

Artificial intelligence (AI) wreaks havoc on anyone wishing clean boundaries between science and the public, and between the natural and the social sciences. "Artificial Intelligence" immediately suggests something neither natural nor social. Our story begins with an overview of how rhetorical impasses can develop as scientific knowledge circulates in and out of scientific circles. After enumerating the impasses that are relevant to debates over AI, I focus on a celebrated debate in which I and other STS researchers participated: Can computer models of scientific discovery refute the claim that science is socially embedded? I consider the line of argument pursued by a leading AI advocate, Peter Slezak. In so doing, I focus on how he mobilizes the historical record to portray a "Cognitive Revolution" already in full force. However, I then go behind the scenes to see whether the people enrolled in Slezak's holy war would admit to being on the same side. As it turns out, Herbert Simon and Noam Chomsky, two alleged allies, do not sit very well together—especially since they no longer have behaviorism to fight. This point reveals how cognitive functions as an umbrella term that obscures the social character of things. In response, I entertain the idea that cognitive machines, computers, are "virtual social agents." Consequently, a new political economy is needed to which both AIers and sociologists should have an interest in contributing.

OF RHETORICAL IMPASSES AND FORCED CHOICES

Scientific controversies often reach rhetorical impasses when differences of opinions solidify into mutually exclusive groups of followers who perceive themselves as bound to a common fate. Under these circumstances, comparing and, hence, negotiating the positions in question cannot occur. When a scientific controversy is transferred to the public sphere, an impasse may be created that had not existed in the scientific sphere. Participants in scientific debate are typically encouraged to treat major theoretical positions as ideal types. One appeals to these types in varying degrees depending on the particulars of the case under consideration. For example, when the so-called nature–nurture debate is conducted in the scientific literature, all sides tend to admit that an organism's behavior is determined by some combination of genetic and environmental factors. However, once the debate goes public, participants tend to mobilize into two fairly rigid camps, which come across as holding that *all* behavior must be *exclusively* determined by either genetics or environment (cf. Howe and Lyne 1992). Simplification of a scientific debate is due largely to the different possible outcomes in the public and scientific spheres. Thus, scientific success is often measured by the vicissitudes of journal citation patterns. Yet public success is announced by such unsubtle indicators as votes, appointments, and funding decisions, all of which involve forced choices of one sort or another. In a scientific controversy, rhetorical impasses emerge as a result of the restricted media available for expressing opinions.

I do not bemoan the prior state of affairs. After all, in a crucial respect, the mass media play much the same practical role as philosophy has aspired to play at a theoretical level. Both aim to unify and focus scientific debate by eliminating surface differences in the context of inquiry. However, philosophers and journalists have typically focused the ends of inquiry quite differently. Journalists aim toward integration with other strands in contemporary public debate. Philosophers ultimately want convergence with the histories of the other sciences. Thus, the rhetorical impassability of scientific controversy explains much of how the subject matter of the history and philosophy of science comes into being. Left to their own devices, scientists can entertain a variety of incompatible theories for an indefinite period. Yet the history of science that typically most interests the philosopher—the "internal" history of science—consists of a canonical series of great decisions that the scientific community supposedly made between rival theories. Most philosophers who study these decisive moments—as when Copernicus finally trumped Ptolemy—have presumed that the weight of the evidence, or some other "methodological" criterion, made the difference between accepting one theory and rejecting the other. However, ignored is the question of why the moment of decision was when it was and not earlier or later. As Larry Laudan (1984) observed, scientists seem to be able to converge on a theory *when they agree that it is time to make a choice*. And as Serge Moscovici's research in the social psychology of group decision making has shown, those who control *when* the decision is made control *what* decision is made (Levine 1989).

If rhetorical impassability is a robust phenomenon, then one ought to look to the restrictions on cognitive expression that result from translating a scientific controversy into the more coarse-grained currency of the public sphere. For example, German physicists argued back and forth about the merits of quantum indeterminacy without any felt need for resolution. However, the pressure to survive in an irrationalist culture dictated that they plump for the indeterminist interpretation (Forman 1971; cf. Fuller 1988a: Chap. 10). That the ambient culture simply "determined" the scientists' response would be too easy to say. For if we take Moscovici seriously, then a claim of that sort would unwarrantedly presume that closure *had* to be reached on the governance of quantum particles in Weimar culture. In fact, one of the things that a minority opinion group can do effectively is prevent the "moment of decision" from ever arising by arguing that more evidence needs to be gathered and analyzed. Thus, in some places and times, no orthodox opinion exists because the minority blocks that view from finally closing discussion. As the rhetorician would have it, time (*kairos*) is of the essence.

In what follows, the ideological wellspring of STS, known as the Sociology of Scientific Knowledge (SSK), or the "Strong Programme," constitutes a minority against the alliance of interests around Artificial Intelligence (AI) research. But does SSK function as any more than a spoiler? Once SSK shifts the argument to a new plane, interpenetrative rhetoric can transform SSK from *paralyst* to *catalyst*. My next move in this debate is to show how the intellectual project of AI is very much bound up with how its defenders mobilize allies and distance opponents. *AI*, then, *is an instance of SSK in action.*

SOME IMPASSES IN THE AI DEBATES

One intriguing feature of the swirl of controversy surrounding AI research is the extent to which highly abstract debates in the sciences are so permeable to public interpretation. Subsequent research reflects what was previously regarded as the public's conceptual coarseness and confusion. Unlike sociobiology, in which the media intervened only after "gene talk" had taken root in the scientific arguments of biology (Howe and Lyne 1992), AI research has been invested with public import from day one.

The rhetorical impasse surrounding AI may be epitomized by the multiple interpretations given to the following answers to the question: *Can computers think?*

1. *It's inevitable*: Computers will continually improve their cognitive capacities until they surpass humans in intelligence.

2. *It's impossible*: Computers will never demonstrate real intelligence because of the unprogrammable character of human expertise.

With some interesting exceptions (discussed later), most SSKers stand for (2), whereas advocates of "Strong AI," like Slezak, defend (1). What makes the difference between (1) and (2) impassable is that the terms of the debate defined by these two positions can be understood in a number of alternative ways. However, the coalition needs of the groups associated with each position make it imperative that those alternatives remain suppressed. Here are three questions in which unresolved ambiguity is put to strategic advantage in public debate:

a. Is the "intelligence" to which computers are held accountable defined by the range of behaviors that is normally criterial of human intelligence or by the mechanisms that produce such a range in humans?

b. Does "computers" literally refer to particular machines taken in isolation, or to machines functioning in some suitably normal environment (which may include interfaces with humans and other machines)?

c. Is the thrust of the major theses descriptive (i.e. a statement of fact about machine capabilities) or prescriptive (i.e. a statement of value about the possibilities for interpreting machine behavior)?

For each set of alternatives, it is common for (1)-inspired defenses of one option to be met by (2)-inspired attacks from the other option. (Indeed the entire career of Hubert Dreyfus may be reduced to variations on this strategy.) Thus, in debate (a), if (1) is defended by arguing that a given computer can produce certain behaviors, a proponent of (2) will reply that the computer cannot produce the behaviors in the way humans can. Similarly, in debate (b), if (1) is defended by pointing to the computational power of an individual machine, the proponent of (2) will respond by pointing out that features of human intelligence require an ambient social world to be recognized. Finally, in debate (c), if one musters support (1) by noting the tasks in which computers actually outperform humans, the supporter of (2) will challenge the wisdom of letting computers handle such tasks. In each of these three debates, the responses from the camps representing (1) and (2) are not in contradiction because they are not addressing the same issue. Here, the social epistemologist's task begins in earnest. To show the compatibility of two sides to a controversy is not to end it. Rather, one diagnoses two incommensurable positions on a yet-to-be-specified subject of common interest that can be better pursued in collaboration than in either spurious opposition or splendid isolation.

DRAWING THE BATTLE LINES

AI and SSK are moving targets that, over the past three decades, have charted orthogonal courses in the study of scientific reasoning. In 1989, the leading SSK journal, *Social Studies of Science*, devoted a special issue to the first head-on confrontation between these two heirs apparent to the throne of epistemology. Leading the offense was Slezak (1989), who claimed to have refuted the signature SSK thesis that science has an ineliminably social character. Specifically, he argued that computers could be programmed to reproduce at least some of the discoveries made by scientists of the past without reproducing their social context. Slezak, Australia's first self-described cognitive scientist, was significantly a classical epistemologist in disguise, whose doctoral dissertation tackled no less than Descartes. Here are the telltale signs of Slezak's philosophical rhetoric:

4. The argument is set up as a zero-sum game, which is rigged so that AI wins as long as SSK is not *necessary* for modeling science.

5. To model science is defined in terms—taken from the internal history of science—that make the evidence assembled look most persuasive.

6. Evidence supporting that his claim can likely be supported—that is, evidence once removed—is taken as sufficient grounds for demonstrating the claim.

The inimitable mark of Slezak's philosophical gamesmanship appears in (6), the idea that the burden of proof can be shifted by showing that a machine *could* capture what SSK *already* can capture. After all none of the classical philosophical conundra—especially the problem of skepticism and of the existence of other minds—would have ever gotten off the ground had philosophers respected the burden of proof that their radical queries bore. (Of course philosophers more than made up for their rhetorical insensitivity by conjuring up the specter of grave risk if their queries were *not* pursued.) Slezak manages to scare up a possible case by mustering an assortment of opportunistically chosen pronouncements from practitioners and theorists of AI and, more substantively, by citing Herbert Simon's ongoing series of BACON programs (Langley et al. 1987). These programs ultimately aim to derive the largest number of discoveries from the history of science—in the order in which they occurred—from the smallest number of heuristics. (Each heuristic is basically a set of nested "IF you're in this situation, THEN do this" statements.) The BACON programs are philosophically interesting because the computer makes its way through the history of science by "learning how to learn." Each solved problem and new discovery adds to the knowledge base for future reference and sometimes aids in developing new heuristics. Slezak devotes his original article to appeals to authority and other people's computer sketches.

The defense of SSK was mounted by an assortment of philosophers, sociologists, and psychologists more concerned with pointing out the weaknesses of AI (as presented in Slezak's paper) than defending SSK. The most explicit target of Slezak's original article, David Bloor, a founding father of SSK, refused to enter the fray. Slezak had one overt sympathizer among his interlocutors, a fellow computer modeler (Paul Thagard) who struck a conciliatory note. Thagard reassured SSKers that the social context of scientific work was not just eliminable "noise in the system" but a genuine anomaly that future computer programs will be able to solve. After that initial skirmish, Nobel Prize-winning super-social-scientist Herbert

Simon (1991a) intervened with some schoolmasterly fingerwagging at the SSK defenders, as each had failed, in some way or other, to fathom the epistemologically salient features of his computer programs.

I support AI work, but for reasons other than those of Slezak, Simon, or the ordinary AI enthusiast. I believe that the emergence of computer models in epistemological discussions marks a historic renegotiation of the meaning of science and, more generally, epistemic authority. AI has disrupted any easy notions that either of the scare-quoted expressions is uniquely human. In this regard, AI's success counts as evidence in favor of the basic SSK thesis that all concepts, even the ones that pertain to the concept makers, are conventional. If, however, we remain oblivious to the often subtle changes that the computer revolution has wrought in our ordinary self-understanding, empirical corroboration of SSK's thesis will not necessarily have salubrious consequences for society (H. Collins 1990, esp. Chaps. 13-14). Generally speaking, Slezak refuses to take seriously that the incompatibility of AI and SSK is an artifact of their sociohistorical circumstances. Where Slezak sees an essential difference, I see a lack of communication—one that has persisted for so long that AI and SSK have become incommensurable. This problem illustrates a cardinal principle of social epistemology: *Conceptual difference is born of communication breakdown* (Fuller 1988a: xiii).

AI AS PC-POSITIVISM

Slezak rightly associates the epistemic orientation of AI with the accounts of rationality implicit in "internalist" histories of science, which, following Bloor (1976), he calls the "teleological model." Moreover, an open question remains whether the BACON programs, as exemplars of AI, can surpass old-fashioned positivist ingenuity in devising efficient methods for selecting hypotheses. This issue should not be confused with the obvious fact that, over the years, machines have been designed to apply these methods to more problems more quickly. A striking case in point is Thagard (1988), who introduced all of his "problem-solving strategies" in mainstream philosophy journals long before he displayed the effects of prolonged exposure to a computer. The very names of these strategies, "inference to the best explanation" and "maximizing explanatory coherence," betray a philosophical lineage that goes back at least to Charles Sanders Peirce and perhaps even Sir Isaac Newton. Indeed, Thagard (1988) admits that his programs' alleged philosophical breakthroughs were made to develop an automated logic tutorial for undergraduates—that is, PC-Positivism! (And, I mean "PC" in the late 1980s sense of "Personal Computer," not in the early 1990s sense of "Political Correctness.") Only a sexier rhetoric and a bigger machine seem to separate the positivist's "logic of justification" from the AI researcher's "logic of discovery." After all, what BACON and other such programs simulate is the selection of the *right*, (read justified), hypothesis.

However, Slezak's acceptance of SSK's terms of engagement leads him astray in framing the internalist–externalist dispute. Consequently, he makes the internalist and the externalist appear to offer competing answers to the same question: Were "cognitive" or "social" factors primarily responsible for the acceptance of a given scientific claim? Yet the internalist and the externalist use the history of science somewhat differently. The internalist aims to test a *normative* theory of scientific reasoning. For example, the fact that Heisenberg would not have argued for quantum indeterminacy—had he not been sensitive to the rise of irrationalism in Weimar culture—does not undercut the fact that the arguments were sufficient (in the internalist's eyes) to make the case for quantum indeterminacy. In this instance, the externalist addresses the occasions that brought about the need to make indeterminacy arguments in physics. Still the internalist is concerned with the soundness of the arguments (given what was known at the time) regardless of their cultural timeliness. This difference in attitude toward time is especially acute in the case of Herbert Simon. He distances his computer simulations of scientific discovery from the actual history of science in exactly the same way as, say, Larry Laudan distances internalist from externalist historical interests (Laudan 1977: Chap. 7).

For Simon, "time" represents an abstract sequence of events; the sooner it transpires on the computer, the better. Simon expresses this sense of time most vividly in wondering how Kepler could take so many months to discover his three laws when BACON can do it in a few minutes. In effect, Slezak and Simon presuppose a strong content–context distinction, in which the role of context is either to impede or to facilitate the transmission of content. Ideally devoid of all context, the computer appears as the proverbial frictionless medium of thought. Any delay between posing the problem and stating the solution is explained solely in terms of the time it takes to go through operations expressly designed for reaching the solution. As context is added to this process—that is, as content is distributed among finite human beings with differential access to one another—the knowledge enterprise is impeded to ever greater degrees. Being students of context, SSKers are assigned (by Slezak and other AIers) with the task of detailing the various ways humans lag behind computers in their cognitive performance. In short, the internalist and the externalist, or AI and SSK, are doing different but compatible things.

Slezak, as well as Bloor and his SSK defenders, could benefit from seeing internal and external histories of science as asking different questions. In particular, SSKers have all too readily embraced Quine's thesis that theory choice is always underdetermined by the available data (i.e. strictly empirical grounds never exist for supporting one theory over another; for the ramifications of this point, cf. Roth 1987). As SSKers rightly see, if the thesis is true, then factors other than those sanctioned by the scientific method—especially "social factors" broadly construed—play a decisive role in theory choice. Unfortunately, Quine (1953) and other philosophers mean to allow these extra factors *only* because underdetermination is true. In other words, an implicit pecking order exists of explanations for science, which gives pride of place to internal factors. Thus, once internal factors have been exhausted, one turns to external factors to make up the difference. Quine differs from, say, a logical positivist only in *how soon* he believes one will need to countenance social accounts of science. In Laudan's terms, the underdetermination thesis implies the *arationality assumption*, whereby sociology takes over from philosophy to account for the arational residue (however large) of knowledge production. By acceding to the idea that they are offering an alternative—yet "external"—account of the same phenomena, SSKers tacitly accept second-class epistemological status. This dialectical error is ironic for SSKers to commit given the great pains that Bloor (1976: Chap. 1) initially took to stress the need for what he called "symmetry"—namely, for the social sciences to use the same principles to explain *all* episodes from the history of science *equally*—the good, the bad, and the ugly.

At first glance, SSK's dialectical disadvantage reflects a perverse way to divide up the intellectual labor. After all human beings appear to occupy the dregs of cognitive inquiry. But shouldn't computers be held accountable to human cognitive performance, not the other way around? Here, too, Slezak is onto something. Over the past 20 years, the identities of the "modeler" and the "modeled" in AI research have been subtly exchanged, representing a shift in the balance of power within the cluster of computer scientists, neurophysiologists, experimental psychologists, linguists, and philosophers who define "cognitive science" (cf. Pylyshyn 1979). Back in the 1950s and 1960s, when Simon, Alan Newell, and Marvin Minsky were first plying their trade, terms such as artificial intelligence and computer simulation were meant to be taken literally. That is, computers were seen as trying to model human intelligence and as succeeding most notably in the relatively narrow range of "formal thought" that was tractable to linear programming. Computers were then taken as simplifying and amplifying something whose complexity could be fully fathomed only by studying humans directly. However, as the prosthetic reasoning powers of the computer improved (e.g. in medical diagnosis, missile tracking, mathematical problem solving), the object of AI inquiry was subtly reconceptualized. What had been previously regarded as the rich complexity of human intellectual life was now portrayed as a "mechanical defect" that prevents humans from matching the cognitive efficiency of computers. Indeed, Zenon Pylyshyn (1984) christened the object of AI inquiry "cognizers," Descartes' *res cogitans* rendered computational.

Today the term artificial intelligence has become something of a misnomer. Now computers seemingly manifest intelligence in a pure, natural state, whereas human intelligence is corrupted (by error, emotion, and other context sensitivities) and, hence, is at best a first approximation of the ideal form. This shift in the balance of ontological power partly explains the ease with which Slezak and other AI researchers tend to ignore the relevant experimental work on human reasoning. Also psychologists have eagerly retooled to include computer programs as relevant test sites for models of human reasoning (e.g. Anderson 1986; Johnson-Laird 1988).

Slezak is absolutely right that SSK must take this trend seriously. Cognitive science promises to be the strongest pitch yet made by the combined forces of Platonism, Cartesianism, positivism, and other forms of internalism to command political and economic resources. Ironically, this pitch comes at a time when these forces are perceived within the humanities and social sciences as having been intellectually discredited. However, SSKers would be foolish to suppose that the critique launched against the internal–external (content–context) distinction in AI by, say, Weizenbaum (1976) and Dreyfus and Dreyfus (1986)—let alone the more global critiques in Rorty (1979) and Latour (1987)—has trickled down to the science policy boardrooms. Despite the valiant efforts of social constructivists like Harry Collins, AIers remain the primary authorities on how their research is interpreted. Consequently, opportunities for an SSK-style critique are strategically suppressed. AIers focus the policymaker's attention–much as a magician focuses the attention of her audience—on what the computer does "by itself" (i.e. as an "automaton"). In so doing, AIers minimize any awareness of the extent to which framing of the situation depends on constant intervention on behalf of her machine. These interventions occur at the beginning, middle, and the end of programming. In the beginning, the AIer selects and inputs data from the historical record. In the middle, the AIer must typically intervene in the program to supply needed data that the computer cannot get on its own. In the end, the AIer must find a target audience to make sense of the computer's output.

HOW MY ENEMY'S ENEMY BECAME MY FRIEND

From the preceding black-and-white presentation of "AI versus SSK," we might conclude that we have got two monolithic movements on our hands. Although certainly false in the case of SSK, this conclusion is even more strikingly false in the case of AI. If we take AI to include all those who call themselves cognitive scientists, we find influential advocates of SSK-like theses, such as Stich (1983) and Dennett (1987). For polemic, Slezak conveniently collapses ideological nuances and streamlines a tortuous history. Thus, he follows the canonical histories of the Cognitive Revolution that place Simon and Noam Chomsky on the same (winning) side of the battle against the behaviorists. This by itself is hardly a cause for criticism. After all Simon and Chomsky knew each other and participated in the conferences that would later be taken as having founded "cognitive science"—many of which were officially on "signal detection." Indeed, both have had occasion in interviews to cite each other as contributors to a common cause (Baars 1986). Now to philosophical ears, this last sentence emits a curious resonance: Does "contributors to a common cause" refer to the participants in the Cognitive Revolution? Or does it refer to corroborating evidence for the existence of the Cognitive Revolution's primary object, originally known as "the information-processing system," but nowadays simply called "cognition" or "intelligence"? In fact, I mean a little of both.

Popular Whig histories of the Cognitive Revolution (e.g. Gardner 1987) dazzle the reader with an array of laboratory victories, but obscure the overall strategy that won the war. As typically happens during scientific revolutions, the revolutionaries leave their most lasting imprint with a sensibility about the sorts of findings that are worth having and the sorts of theories worth testing. Thus, the Cognitive Revolution imparted to the study of the mind a legacy of *scientific realism.* This work is marked by the search for underlying mechanisms that explain how a seemingly disparate range of phenomena could have much the same structure, especially under ideal conditions of observation, which may be so ideal as to involve computer simulations of human output.

The gestalt switch caused by this turn to realism would be hard to overestimate. Try talking methodology with a normal practitioner of cognitive science. You will find that she has a hard time imagining how B.F. Skinner and other behaviorists could have thought they were doing science *precisely because* they failed to postulate mechanisms that were not susceptible to direct empirical test. In fact, quite sophisticated cognitive scientists have been known to forget what kept behaviorism afloat for 50 years in America. Despite persistent moral and intellectual objections, behaviorism's dogged commitment to "the methods of science" helped it prevail. Back then, these methods were primarily associated with the prediction and control of observable behavior. But behaviorism's methods were *not* associated with the search for underlying mechanisms, which, absent the appropriate operationalization, would just dissolve into a species of metaphysics. Chomsky, who plays a crucial role in Slezak's argument, contributed decisively to the return to realism.

Scientific realism has been a tricky business to pull off historically. The cases where scientific realism seems to work are ripe for a deconstruction of the actor networks that had to be built along the way. Bruno Latour (1988) has been especially struck by the actor network known as *explanation.* Latour conceives of explanation as a form of political representation in which the explainer represents a diverse constituency, spokespeople for particular sorts of phenomena that are subsumed under the explanation. According to "the politics of explanation," then, the so-called efficiency of a covering law (or universal generalization) lies in its ability to minimize the resistance of the disparate elements it subsumes. For example, in Newton's laws, celestial and terrestrial motions are reduced to two versions of the same thing. Not surprisingly, then, scientific realism has taken the key to science's epistemic power to lie in its explanatory function. The underlying reality implicated in a covering law confers greater power on the subsumed constituencies collectively than they would have individually. As in any political situation, the trick is to make the representative accountable. To revert to philosophy of science terms, one treads the fine line of "corroboration" between portraying the constituencies as pursuing largely *parallel* research trajectories and portraying them as pursuing largely *convergent* trajectories. However, neither option is completely welcomed.

The Scylla that awaits the parallelist is a stack of idle analogies that reveals a common structure only to the impressionable historian or philosopher. Thus, the realist does not want to look like a mere analogy monger. Such would suggest a throwback to the period immediately prior to the Scientific Revolution when man and nature were fraught with mystical "correspondences" that were ultimately underwritten by a Divine Emanator of Forms (cf. Foucault 1970: 17-25). The cognitive scientist may believe that the structure of the *mind* is isomorphic in all of its embodiments (e.g. à la Lévi-Strauss, that the individual mind is the microcosm of some collective mind). Consequently, she typically does not believe that the structure of the mind is isomorphic to the structure of *nature* in all of its embodiments. This belief brings us to Jerry Fodor's (1981) doctrine of *methodological solipsism*: Study the mind as if Descartes' worst suspicions were borne out and the world presented to you is a complete illusion. Admittedly, Rom Harré (1986) is one scientific realist who embraces analogy mongering wholeheartedly, including its Aristotelian implications. But he rejects "cognition" as a proper object of inquiry independent of the environments in which embodied cognizers find themselves. Harré thus turns from Fodor to J. J. Gibson and his ecological orientation to psychology.

Drawing a boundary between what is inside and what is outside the mind is not going to do the trick for the scientific realist. Too many loose analogies are available. Consider this heterogeneous group of people who, on both standard European (De Mey 1982) and American (Gardner 1987) accounts, are said to have been on the same side of the Cognitive Revolution:

- Herbert Simon (political scientist-turned-computer simulator)
- Noam Chomsky (theoretical linguist-turned-psycholinguist)
- George Miller (communications technologist-turned-experimental psychologist)
- Marvin Minsky (mathematician-turned-computer scientist)
- Jerome Bruner (Gestalt psychologist-turned-educational theorist)
- Jean Piaget (child psychologist-turned-genetic-epistemologist)
- Claude Lévi-Strauss (structural anthropologist)
- Thomas Kuhn (historian of physics-turned-philosopher of science)

Given this veritable Chinese Encyclopedia of "cognitivists," how might one characterize—not to mention explain—the relevant sense of resemblance among Simon's problem-solving heuristics, Chomsky's competence grammars, Miller's information-processing stages, Minsky's modules, Bruner's principles of perceptual integration, Piaget's developmental sequence, Lévi-Strauss' cultural maps, and Kuhn's paradigms? This question finally suggests the Charybdis that awaits the convergentist: the image of the Cognitive Revolution as a matter of elaborate collusion. Did the principals listed earlier secretly meet somewhere in Cambridge, Massachusetts, in the late 1950s to concoct the roles they would play in the planned intellectual coup? Then, afterward, did they periodically meet to straighten out each other's lines in the unfolding drama? Clearly, nothing quite like this actually happened. Still the contact that most of these people had with each other in metropolitan Boston during this period was much greater than their disciplinary differences would suggest.

Here are just some of the subtler connections that earned Boston the title of "Hub of the Universe" (an expression presciently uttered by Ralph Waldo Emerson a century before the Cognitive Revolution). Kuhn was inspired to isolate "the structure of scientific revolutions" by Piaget's dynamic structuralism, which Bruner was promoting in the late 1950s and, indeed a decade later when De Mey came to study with him at the Harvard School of Education. An even more direct infusion of French structuralism (including the teachings of Lévi-Strauss) occurred with Roman Jakobson's accession to a chair in linguistics at MIT. Jakobson's principal colleague was Morris Halle, who early befriended Chomsky. (On the psychology side, of course, George Miller was instrumental in converting Chomsky's formal apparatus to testable hypotheses.) Gardner, himself a fixture at the Harvard School of Education, wrote his first book on the French influence on cognitivism (Gardner 1973). Although Herbert Simon's career has been most closely associated with the cities of Chicago and Pittsburgh, Gardner reassures us that Simon (and Newell) was present at Dartmouth College, New Hampshire—a Boston satellite—in the summer of 1956 when the Cognitive Revolution was officially declared. Moreover, the cognitivists were in close proximity to the hub of their opponents, including the behaviorists B. F. Skinner and W. V. O. Quine, both of whom taught at Harvard.

None of these facts, well known as they are, would bother the behaviorist who tried to account for the Cognitive Revolution. The behaviorist, accordingly, would first argue that mutual reinforcement of the revolting parties was crucial to the maintenance of their collective behavior. However, the historian sympathetic to cognitivism would want to avoid any whiff of revolutionary collusion. Consequently, she would prefer to see them as inadvertently running across each other after having pursued parallel courses in which they had managed to survive (what they learn, after the fact, to have been) a common foe.

At this point, Sir James Frazer's early anthropological classic, *The Golden Bough*, may come to our interpretive aid. Of interest is Frazer's discussion of *sympathy* and *contagion* as principles by which "savages" explain change in the world. The historian of the Cognitive Revolution would have us believe that the principals knew of each other's work largely from a distance so as not to be conspiratorial, yet close enough so as to enable them to see their points of commonality. Thus, by striking the right distance from each other, the cognitive revolutionaries can mutually implicate the independent objectivity of their respective viewpoints. The behaviorist would, of course, read these developments less charitably. She would explain a world seemingly fraught with ideational sympathies in terms of verbal contagion. (Indeed that is exactly the kernel of truth in "epidemiological" models of conceptual diffusion: Sperber 1996.) That is, when a piece of language—such as cognitive or information processing—does the trick for someone in one setting, then interested onlookers try to see if the words will work the same magic for them. For example, cognitive continued to work some *negative* word magic in the behavioristically dominated clinical circles long after it had become ascendant in experimental psychology (cf. Mahoney 1989). Whether anything else about the onlookers' research practice changes is an open question, which the Whig historian is inclined to pass over in a tactful silence. Baars (1986: 138-64) implicitly raises this issue by styling an entire set of Cognitive Revolutionaries, including George Miller and George Mandler, as "adapters."

So far I have suggested that some sense exists, either ontological or sociological, in which the likes of Simon and Chomsky are rightly cast as being on the same side of the Cognitive Revolution. But I really want to argue for a much weaker thesis: This unity in arms runs no deeper than the fact that Simon and Chomsky (and the rest of the cognitivists) shared a common foe—the behaviorists. For once the foe was put safely out of dialectical range, the cognitivists roamed wherever they pleased on the conceptual map. Not surprisingly, this practice led the principals to distance themselves from one another in interestingly asymmetrical ways, reflecting their respective senses of how the cognitive should be bounded—now that the coast is clear of The Behaviorist Menace. I confine my remarks mainly to Chomsky and Simon because Slezak focuses on them.

BUT NOW THAT THE COAST IS CLEAR

The indefinite continuation of the "Cognitive Revolution" (Gardner's term) into the "Cognitive Paradigm" (De Mey's term) provides an interesting case study. This study involves consensus formation and deformation, the processes to which philosophers have turned in order to "sociologize" their accounts of science (cf. Laudan 1984; for a critique, see Fuller 1988a: 207-32). For consensus theories, the present discussion's most important implication is that a consensus emerges only if a reason exists (an external force, as it were) to do so; otherwise, the constituent individuals will move off in their own directions, with the consensus language (in this case, cognitive talk) becoming semantically diffuse. Herbert Simon and Noam Chomsky would no doubt be surprised to find themselves fighting on the same side in Slezak's holy war against SSK. Each researcher places quite a different value on continuing the alliance that originally enabled them to vanquish The Behaviorist Menace. Simon, the covering cherub, continues to abstract the form of "intelligence" from as many disciplines as he can just to get the analogies to stack up right. But Chomsky will not let linguistics be co-opted into Simon's scheme so easily.

AI enthusiasts like Slezak tend to presume that the computer models a self-sufficient Cartesian reasoner—perhaps because of the lingering folk association between computers and robots. However, a moment's thought reveals just how alien a program like BACON is to Chomsky's radically Cartesian sensibility. Chomsky (1980: 76), after all, notoriously claimed that humans are endlessly creative creatures, capable of generating new sentences without any obvious prompt from memory or the immediate environment.

In contrast, the "discoveries" BACON makes are not new and are highly dependent on programmer intervention. More in keeping with the spirit of BACON is Simon's (1981: Chap. 3) equally infamous assertion that the complexity manifested in human behavior is entirely a function of the complexity of the environments in which humans manifest their behavior. According to Simon, when we try to solve a problem, we already know in vague terms what an adequate solution would look like. The "problem" lies in realizing that solution within the means at our disposal, broadly construed to include anything we can use in the environment. The human condition, thus, is so inordinately complicated only because our means are typically so ill-suited to our ends that we are forced to concoct backhanded solutions. Progress is made as more efficient means are designed to realize more solutions. Not surprisingly, then, Simon-influenced AI work has shown little interest in modeling the actual cognitive processes of scientists because from Simon's standpoint, these processes are little more than clumsy way stations on the road to completely efficient thought.

Simon's attitude recalls the early behaviorist view of deliberation as hesitation prior to response —a process warranting not further study, but elimination through efficient conditioning. Moreover, Simon's analysis of the sources of human complexity is a more generalized version of what actor-network theorists in SSK have to say about the totalizing tendencies of technoscience. For example, Callon, Law, and Rip (1986) present the technoscientist as establishing her credibility by "translating" the interests of an increasingly large number of others into her own work. Credibility is reflected in the textual constraints placed on journal articles and grant proposals, through which she must pass before being granted her point. In Simon's terms, these intervening interests constitute the "environment" in which the technoscientist must achieve her goals. However, whereas Simon's discovery programs are designed with an eye toward eliminating or simplifying as many of these "middlemen" as possible, actor-network theorists see a trend toward engulfing all of society as middlemen (now called "obligatory passage points") in the production of scientific knowledge. Yet both Simon and actor-network theorists agree nothing inherently complex or special exists about scientific reasoning as a cognitive process: It is simply ordinary strategic reasoning deployed in extraordinarily resistant environments.

Simon—The Covering Cherub

Perhaps a word about the unity of Simon's career is in order (Simon 1991b). Simon (1976) originally introduced the concept of "bounded rationality" in the mid-1940s to account for the adaptive character of corporate decision making or "administrative behavior," which occurred under conditions of significant uncertainty. By the late 1950s, bounded rationality had become the concept that unified Simon's forays into economics, organizational theory, cognitive psychology, and computer science—including computer models of scientific discovery. What follows from Simon's own account, then, is the prototype for Slezak's computer refutation of SSK is something as "sociological" as an account of corporate decision making. (Readers are invited to ferret out the dead metaphors from organizational theory that infest Simon's informal descriptions of his computer programs: cf. Langley et al. 1987: especially 299-300, which describes an "integrated discovery system.")

In the short but synoptic sweep of *The Sciences of the Artificial* (H. Simon 1981*),* we learn that intelligence is inherently artificial. Intelligence, on Simon's account, emerges once an "organism" (understood in that abstract systems-theoretic sense that is indifferent to biology) develops reliable ways of maintaining—and sometimes even enhancing—its identity against the resistance of its environment. Thus, we differ from the thermostat "only" in the variety of productive ways in which we adapt to change in the environment. Simon's main thesis holds that the truly smart organism primarily tries not to seek wins, or avoid losses, or even stay in the game forever, but rather to pursue a bounded version of all three simultaneously. The smart organism, then, tries to get the most from the least for as long as it can. As this thesis goes against the conventional wisdom of virtually every discipline in the social sciences (especially formal philosophical models of rationality), Simon can use it as a pretext for reconstituting all of those disciplines into "sciences of the artificial."

Clearly, in Simon's artificial sciences, the key unit of analysis is not the lone administrator, *but the system of administration.* Here the administrator functions as a major node in the administrative network. Indeed her behavior may well provide the richest symptoms of the system's overall state. But the administrator is only a *part*, not the whole unit of analysis. Unlike Chomsky's competent language user, she is not only embodied as an individual, but environmentally embedded as well. Of course how one goes about individuating an administrative system is not exactly clear. But that very ambiguity renders the "system" a contested terrain, and hence an apt SSK object of inquiry. In particular, whose response to what sort of feedback is relevant to telling one system apart from another? The introduction of computers into Simon's project complicates systems analysis, as well as the ensuing AI–SSK debate, since a computer can be treated in any of the following ways:

7. As a system in its own right, composed of machine parts or functions, depending on one's mode of analysis (cf. Dennett 1987: Chap. 1);

8. As a model of a system (e.g. the BACON computer simulation of the scientific discovery process);

9. As an individual in its own right, like Simon's administrator, embedded in a larger social system.

Although the AI–SSK debate officially transpires at level (8), SSK supporters, in fact, move quickly to level (9), culminating in the recent discussion of computers as *actants*. AI advocates tend toward level (7). Simon buys into this perspective, which permeates what may be called Slezak's "rhetoric of testability." In other words, "intuitive" appeals to the social, as in Simon's administrative systems, remain "mere" metaphors until they have acquired a technology, such as the digital computer. Then these appeals can be used to reproduce the relevant nonsocial fact and thereby explain the fact's persistence in a variety of social settings. Slezak's argument has bite just as long as we focus on the final stage of this operation and ignore all the previous ones, including the embarrassing fact that Simon's intellectual perambulations began with an interest in the work environments of harried bureaucrats.

Chomsky—The Revisionist Historian

As much of Slezak's argument against SSK, and especially Bloor, turns on a folk valorization of Chomsky's significance, something should also be said about what Reber (1987) called "the rise and (surprisingly rapid) fall of psycholinguistics." We have begun to get enough historical distance from the Chomsky phenomenon to understand what actually took place. Chomsky's work, while remaining vital in linguistics, reflects a meteoric rise and fall as a research concern in psychology between, roughly, 1967 and

1980. Why did Chomsky fade so fast in psychology? Two related reasons stand out that have import for Slezak's argument.

First, Chomsky refined his generative grammar only in response to anomalies arising from considerations in theoretical linguistics, such as the simplicity of rules in the grammar and the ability to parse intuitively grammatical sentences. He remained unresponsive to the recalcitrant data raised by experiments on the grammar's psychological validity. Hence, the psychologists pulled out of the enterprise in frustration.

Second, whatever one makes of its fortunes, behaviorism conformed to the "functionalism" implicit in all successful research agendas in experimental psychology since Wundt: Behaviorism attempted to derive principles that explain an organism's behavior as a function of some environmental change. Chomsky's work failed to meet this basic principle. It supposed that linguistic competence remains invariant *in spite of* differences in language training and other environmental stimuli. Indeed this lack of interdependence between what is postulated as transpiring inside and transpiring outside the organism posed a major obstacle to conducting decisive experiments on Chomsky's model. However, the typical AI research environment is quite unlike the normal working situation not only of the human scientist, but even of a computerized expert system. The absence of what experimentalists call "ecological validity" exacerbates the problems involved in computer testing Chomsky's model.

Nevertheless, Chomsky has influenced how historians conceptualize the trajectory of psychology since World War II. In particular, Chomsky portrayed behaviorism as a degenerating research program—one that had to be overturned for progress to be made in psychology, and one that could be successfully overturned only by importing a strong "cognitivist" or "nativist" orientation. In what follows, I begin to deconstruct this conception, the pervasiveness of which approaches that of Kant's division of the history of philosophy into "empiricists" and "rationalists" at the end of *Critique of Pure Reason.*

First, I note that B. F. Skinner and other behaviorist targets were taken completely off guard by Chomsky's relentlessly negative portrait of their scientific status. In speaking of behaviorism's "success" as a research program, Skinner pointed to its applications, many of them in clinical settings starting in the 1960s—*after* Chomsky had begun to sound the school's death knell. Both historians and psychologists overlook this fact because Chomsky managed to persuade psychologists that the epistemic status of their research programs should be judged solely on the basis of their scientifically derived findings, not on the basis of their practical applications. Thus, behaviorism's perceived decline was, simultaneously, the triumph of the academic discipline of psychology over the liberal profession of psychology. In short, Chomsky changed the standards of success in the field to his strategic advantage.

Given Chomsky's subtle historiographic coup, Bloor can only be congratulated for reviving SSK interest in Chomsky's original target, Skinner's (1957) *Verbal Behavior.* Chomsky's review, which launched him into stardom, indicates that he had hardly delved into the work. Lost on Chomsky was Skinner's theoretical comprehensiveness. Skinner incorporated into the behaviorist repertoire a strong audience component (as the selection environment for operants) reminiscent of the early reader–response criticism proposed by I. A. Richards, as well as the emotivist theories of language use found in logical positivism and general semantics. In effect, Skinner had made great strides toward "socializing" the behaviorist bias toward the isolated organism. He transformed the concept of a text's "meaning" into a network of operant responses that has the text as the nodal stimulus and that in turn enables efficient communication between an author and multiple readers at once. Judging Chomsky's review as an uninformed attack by an upstart linguist, Skinner, not surprisingly, deemed it unworthy of timely response (MacCorquodale 1970 is the first sustained behaviorist response). This tactical blunder was admitted too late (Czubaroff 1989).

While Chomsky minimized *Verbal Behavior*'s impact on psychology, the book bore substantial fruit in analytic philosophy. The theory of reference (1960), as developed by Skinner's Harvard colleague W. V. O. Quine, continues to be influential. From behaviorist premises about how one would draw up a translation manual for a language radically different from our own, Quine arrived at the "indeterminacy of translation thesis." This thesis asserts no fact of the matter exists about a speaker's mental states that could determine the correctness of a translation of the speaker's utterances. According to Quine, a translation's correctness is entirely relative to its purpose and fit with the speaker's other translated utterances.

Although controversial, Quine's thesis helped shift the burden of proof among analytic philosophers in the United States to those who would maintain that mental entities such as "meanings" fix the reference of our words. Put more generally, something transpires in a speaker's mind that is the ultimate arbiter of what the speaker means. (Wittgenstein's argument against the possibility of a private language has had the same sort of effect in Britain.) This line of thinking is also strongly represented among cognitive scientists trained in the analytic tradition. Take, for example, cognitive scientists who deny that regularities in human thought or behavior can be specified without reference to environmental variables. On the one hand, Stephen Stich (1983) has argued that the empirical unreliability of appeals to reasons, beliefs, and desires in explaining behavior—items in the very ontology that Slezak holds to be at the foundations of AI—merits the elimination of these entities. On the other hand, Daniel Dennett (1987), a student of the philosophical behaviorist Gilbert Ryle, argued that only the "interpretive stance" that one adopts to a computer always beaten in chess determines whether the computer is a bad chess player or simply a machine designed for some other purpose. Nothing intrinsic to the computer could alone decide the issue. Both Stich's and Dennett's views sit quite well with social constructivist accounts of AI—both support evolutionary biological explanations that would make the constructivists bristle.

Simon and Chomsky: The Fine Art of Strategic Positioning

To co-opt Chomsky, Simon resolved the differences between his own empiricism and Chomsky's rationalism at an appropriate level of abstraction from the phenomena of language. As is well known, Chomsky (e.g. 1980: 136-39) treats language as a self-contained, or "modular," organ whose fundamental workings are little affected by the vicissitudes of the speaker's contact with the environment. However, Simon attempts to soften this line. He argues that what is hardwired in the organism is simply the ability to learn from interactions with the environment. In that case, language may be distinguished by the efficiency with which humans learn it: Small and simple input seems to elicit massive amounts of complex output (Simon 1981: 89-91). The British linguist Geoffrey Sampson (1980: 133-65) has tried to show how this sort of efficiency could have arisen from the evolutionary forces of syntactic variation and social selection.

Notwithstanding Simon's efforts at keeping the old revolutionary alliance intact, Chomsky persists in his wayward course. For example, when explicitly asked to comment on Simon's work, Chomsky admits to having never taken much of an interest. Indeed, for some rather deep methodological reasons, one wonders how the two could have ever been allies at all (Baars 1986: 348). Chomsky believes that Simon overestimates the significance of computers that have the ability to solve certain classes of problems as well as, or even better than, humans. He thinks that, in this respect, Simon repeats the errors of behaviorists who were overimpressed by the success of animal conditioning experiments. From Chomsky's standpoint, both Simon and Skinner, say, seem to focus mostly on modeling the sort of behavior in which humans are unlikely to outperform machines or pigeons (e.g. serial computations, simple motor skills). Consequently, they spend relatively little time on trying to model successful human behavior. Like the behaviorist, Simon is still principally concerned with the prediction and control of behavior (regardless of the relevance of the behavior to what one is ultimately interested in). He ignores the search for underlying mechanisms that would genuinely (e.g. neurobiologically) explain why *humans* have the distinctive capacities that they have—most notably, language. Thus, Chomsky reserves his approval of AI work for people like David Marr, who actually tried to model mechanisms (in this case, for visual perception) and not merely behavior.

To be sure, Simon is on record as claiming that what makes humans so good at science is the variety of imperfect heuristics that we have come to be able to juggle to good epistemic effect (Langley et al. 1987: 7). As heuristics, no one of them is foolproof, and each of them can be found in some combination with the others in all the realms governed by the sciences of the artificial. Skinner's follower, Howard Rachlin (1989), likewise denies any special status to human intelligence. He resolves the “cognitive” experience of humans into the complex networks of operants and reinforcement schedules. Not surprisingly, Simon criticized Skinner mainly for refusing to posit intervening variables—such as “programs”—in the prediction and control of behavior, even though (so Simon claims) such posits are necessary for Skinner's own project to get off the ground (Newell and Simon 1972). Here Simon is reflecting his own commitment to the “purposive behaviorism” of famed Berkeley psychologist E. C. Tolman. His imputation of “cognitive maps” to maze-running rats provided some early clues to how the black box of thought may be scientifically pried open (Simon 1991b: Chap. 12). Again we see Simon trying to blur rather than build boundaries in dealing with his opponents, very much against the spirit of Chomsky's own starkly drawn anti-behaviorism.

In sum, Chomsky and Simon employ very different rhetorical strategies in their pursuit of normal science in the “cognitive paradigm.” Chomsky has managed to embroil himself in over 100 separate debates since first proposing his model of generative grammar in 1957, with virtually all of his research designed to gain dialectical advantage in one or more of these encounters (Botha 1989). He typically argues by shifting the burden of proof onto opponents: Why *shouldn't* language be thought of as a special module, seeing that we understand it so much better than our other capacities? More often than not, Chomsky uses the alleged cognitive superiority of linguistics to other social sciences as an argument for the distinctiveness of language rather than for the role that linguistics may play in reforming social science.

Simon, however, typically argues by juxtaposing a set of relatively simple studies from a variety of fields. No one of these studies is especially impressive, but when presented together they enable the reader to see a heretofore undiscerned pattern of intelligence at work. In principle at least, Simon treats all of the social sciences as cognitive equals. His current focus on AI reflects more the ability of computer programs to serve as a lingua franca for discussions of intelligence than any deep-seated belief on Simon's part in AI's superiority as a discipline. Indeed when Simon (1991a) finally entered the AI–SSK controversy, he freely admitted that his new computerized scientists are just as social as the old-fashioned human ones. With friends like Simon, Slezak does not need enemies!

Language and Thought: Horse and Cart

From where, then, does Slezak get the idea that AI poses a direct challenge to SSK? Here the invocation to Chomsky provides a clue—Chomsky’s own aversion to programmed discoveries notwithstanding. One view held in common by Chomsky and Simon is that there is a *language of thought*—an ideally efficient medium for the transmission of content. Chomsky and

Simon disagree, of course, on how one reaches this ideal. For Chomsky, it is by recovering our innate linguistic competence from actual linguistic performance. For Simon, it is by rendering our environment more tractable to our goals. Either case, however, presupposes a way exists to determine the relative efficiency with which a particular content has been transmitted in, say, speech or a computer program. Such judgments presuppose, in turn, that two texts can have "the same content," with one text perhaps conveying this content more efficiently than the other. However, as Quine originally claimed and experimental psychologists have since shown, no empirical basis exists for such a presupposition: What is counted as having the same content is not only conventional, but also contextually malleable. Given these points, what a language of thought, such as the one embodied in BACON or in Chomsky's generative grammar, supposedly models becomes radically unclear. As a result, what constitutes a proper empirical test of the model is equally unclear.

Given the problems with trying to render his position empirically testable, Slezak's claim that SSK cannot explain the fine-grained detail of scientific reasoning seems bizarre. By this claim, Slezak desires an explanation of why, say, Newton specified the laws of motion as he did and not in some other way. We have just seen that the language-of-thought thesis makes sense only if one can collapse differences in detail to identify alternative formulations of the same content. Only then are Simon and others justified in dropping out the historical specifics from their simulations. Indeed so many details are collapsed that whether BACON is modeling an individual, a collective, or a historical reconstruction remains unclear. By contrast, SSK can use STS resources to explain particular textual selections—namely, in terms of the reading and writing traditions with which the author is familiar and with which she associates distinct audiences (or interest groups) and expected responses. Each of these traditions is transformed as it is combined with others in the course of their being jointly reproduced in a given text. One wonders how much more fine grained Slezak would want SSK to get. My guess, however, is that by building an enormous amount of contextual variation into the construction of scientific knowledge, Slezak thinks that SSK destroys the "realism" or "objectivity" of that knowledge. In fact, all that SSK challenges is science's *universality* and *univocality*.

Although Slezak dismisses the SSK project, his own project does not deserve a similar fate. Unfortunately, behaviorists and SSKers alike are prone to dismiss the entire language-of-thought project that underwrites AI as just so much reification. This project becomes an illicit inference because we agree to the conclusion that something—some common content —exists on which we agree. In turn, this agreement is supposed to be part of some ideal medium for communicating content. However, empirical tests would better show the "asocial" concept of content that he presupposes.

Could two cultures, for example, with radically opposed starting points, given comparable opportunities for collecting data and the like, reach the same correct solution to a common problem? If so, one could show that the initial cultural differences were overcome in the course of looking for optimal ways to relate conjecture to evidence. However, doing this experiment right requires the experimenter to observe at some point that the two cultures have reached convergently correct results. In addition, she must see whether her judgments of convergence match what each culture thinks the other has accomplished, and whether the two cultures can agree between themselves on exactly what has been accomplished. Moreover, during the experiment, the experimenter should keep her own judgments private, but permit the cultures to monitor each other's activities so as to enable them to declare on their own that convergence has been reached. If a language of thought exists, then all these judgments of convergence should converge. Thus, both experimenter and subject cultures should be able to get beyond their particular perspectives and agree on the results of the experiment. My guess is that as the experimental task more closely approximates the rich environment in which science is done, such a harmonic alignment of opinion will be less likely. In turn, the empirical elusiveness—if not downright unfoundedness—of the concept of content on which the language-of-thought thesis is based will be highlighted.

THREE ATTEMPTS TO CLARIFY THE COGNITIVE

In declaring a 10-year moratorium on appeals to the cognitive, Bruno Latour and Steve Woolgar (1986: Postscript) tried to shift the status of the cognitive from an *explanans*—something that can be used to explain human action—to an *explanandum*— something that is in need of explanation. In what follows, I draw on three general STS strategies for providing such an explanation. Given what we have seen so far, these strategies will clearly have to explain the *variety* of accounts that travel under the banner of *cognitive*. The first, grid-group analysis, plots the dimensions of this diversity as a function of social organization. In grid-group analysis, "cognitive" defines what anthropologists call a "sacred space." The second is inspired by Marx's ideology critique. This analysis aims to demystify the "inherent" qualities in things deemed either cognitive or the proper objects of cognition by showing that they are systematically misappropriated features of society. The third strategy returns to my original diagnosis of systematic misunderstanding between AI and SSK. By focusing on the common image of the "black box," one can trace the sources of this incommensurability.

The Cognitive as Sacred Space

The fact that Simon is much more hospitable to Chomsky than Chomsky is to Simon shows that how one defines the "cognitive" depends very much on who one takes to be a proper student of cognition. This choice, in turn, reflects how distinctive (or "sacred") an object one takes cognition to be. Not only is Simon more hospitable than Chomsky to would-be students of cognition, he also operates with a more flexible sense of what counts as a cognitive process. Thus, ontological and sociological space are bounded simultaneously. This sensitivity to the rhetorical character of AI's own history may be captured by a method common to SSK and cultural studies generally (Thompson et al. 1990)—namely, *grid-group analysis.* Grid-group analysis became part of SSK's intellectual armament when David Bloor used Mary Douglas' account of tribal responses to strangers to explain the different strategies that mathematicians used to manage anomalies raised against Euler's Theorem, as portrayed in Lakatos' *Proofs and Refutations* (Lakatos 1978; Bloor 1979; cf. Bloor 1983: 138-45). I briefly sketch, in Tom Gieryn's phrase, the "cultural cartography" of cognitive science based on what I have said so far and offer some suggestions as to how the remaining grid-group quadrants may be interpreted (see Fig. 5.1).

In grid-group analysis, "grid" refers to the internal organization of some body of knowledge-and-knowers. "Group" refers to the external differentiation of that body from other such bodies. A body of knowledge-and-knowers is plotted as either "high" or "low" on both dimensions. Thus, "high group, low grid" would mean that the body in question strongly differentiates itself from other bodies, but manifests little internal

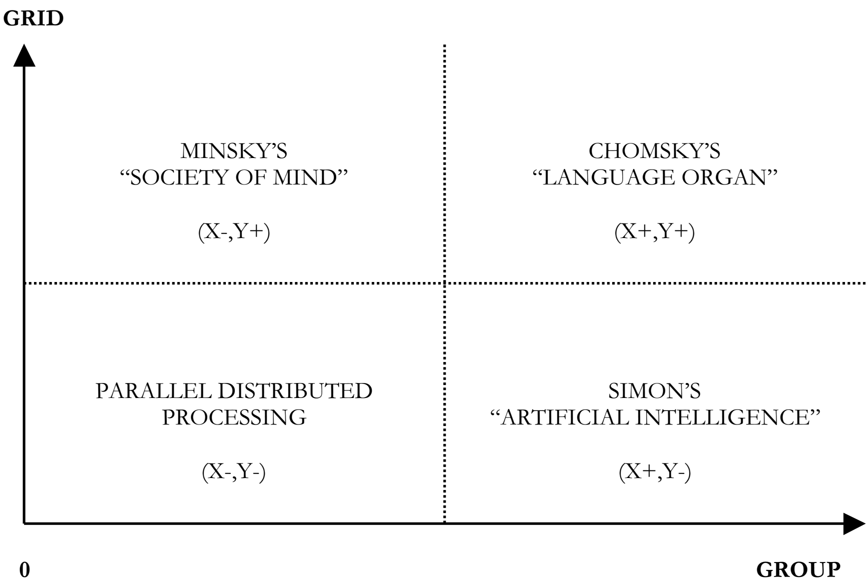

FIG. 5.1 A grid-group analysis of schools of cognitive science.

organization. Here I would place Simon. He identifies the essence of intelligence in the interface between organism and environment, yet then stresses that the mark of intelligence in the organism is its adaptability to change rather than its execution of fixed procedures. Admittedly, the ability of, say, the business firm or the scientific discoverer to adapt to change in its situation is limited. Still, what does not follow is that the firm's or discoverer's response to that situation must be rigid. As we have seen, this attitude also captures Simon's policy toward enlisting allies in the cognitive paradigm. By contrast, Chomsky should be considered "high group, high grid" in his highly formal and rigid manner of demarcating cognitive allies and objects from one another.

A sense is missing of what the "low group" half of the cultural cartography would look like. These people would not postulate a great ontological and sociological divide between the cognitive and the noncognitive. As a follower of Simon might say, the "low group" attenuates the interface between organism and environment. On the "high grid" side, I would locate the "society of mind" approach to AI, long championed by the founder of MIT's AI Laboratory, Marvin Minsky (1986). According to this approach, the mind is a collection of specialized modules that are indexed to situations in which expertise is required in everyday life. Minsky's modules are well defined. Not clear, however, is whether the modules reflect the situation-specific character of social learning or, rather, the biofunctional preconditions for social learning to be situation-specific. Since Minsky's argument draws heavily on metaphors from organizational communication, one might conclude that thought is nothing but a microcosm of social structures in which individuals function. Minsky's (1986: 38-46) basic constructivist tenet—that the "self" is a mythical entity—can be inferred from the fact that the diverse modules cohabit the same body. This view resonates with dramaturgically oriented theories of personhood in philosophy (Dennett, Harré) and sociology (Cicourel, Knorr-Cetina).

On the "low grid" side may be placed the recently popular parallel distributed processing (PDP or "connectionist") models in AI. These models associate particular mental states with the spread of neural activation across the entire brain, thereby obviating the need for functionally specified modules (Rumelhart and McClelland 1986). Connectionism originated as an idea a half-century ago. At that time, Donald Hebb (1949) attempted to provide a neurophysiology able to underwrite the image of the maximally plastic organism presupposed by behaviorism. Since then, several AI researchers designed connectionist models that for some simple motor skills and feature-detection tasks outperformed more orthodox serial processors. However, connectionism did not recognize the strong behaviorist-cognitivist split that fanned the fires of the Cognitive Revolution. Consequently, it remained in obscurity until enough distance had been created from the Behaviorist Menace that cognitivists could afford to reintroduce connectionism through the backdoor—an opportunistic fate for a free-formed, low grid-low group tribe!

The Cognitive as Misappropriated Society

Following Giere, a set of historians and philosophers of science have tried to stake the middle ground in the AI–SSK controversy by proposing a "cognitive history of science" (1988). They support an image of the scientist as a competent, largely self-sufficient human agent. Consequently, they downplay research pertaining to the cognitive limitations of individuals, especially the failure of individuals to appreciate the context dependence, and hence global inconsistency, of their thought and action. Moreover, our cognitivists underestimate the cognitive power that is gained via group communication and technological prostheses. Nevertheless, the cognitivists have brought to light important metaphysical issues that previously eluded philosophers of science. These issues pertain to the bearers of scientific properties: Where in the empirical world do we find *knowledge, theories, rationality, concepts*—to name just four philosophical abstractions hitherto left in ontological limbo? Our cognitivists are clear about arguing for the individual scientist as the relevant locus. Their focus is "cognitive" in the familiar sense of being concerned more with the individual's thought processes than with the products of her thought. Thus, the cognitivists give us a full-blooded sense of what theorizing is like (e.g. a pattern of neural activation), while leaving us with a rather pale, abstract sense of what theoretical output is like.

The cognitive turn in the history and philosophy of science is very much like Marx's on the capitalist turn in the history of political economy. In capitalism, relations among people are mistaken for properties of things. Here Marxists mean that goods do not have an inherent value, or natural price, but only an exchange value determined by the social relations among the capitalist, worker, and consumer. Likewise, I believe that, in its attempt to locate abstractions in the empirical world, cognitivism mistakes (1) *rational reconstructions for actual history*, (2) *properties of groups for those of individuals*, (3) *properties of language for those of the mind*, and (4) *properties of society for those of nature*. I consider each in turn.

1. Consider Margolis (1987), an account of the paradigm shift from Ptolemy to Copernicus in terms of the overcoming of cognitive barriers. Like Piaget's genetic epistemology, this notion makes for better pedagogy than history of science. Teachers could use Margolis to get students to see beyond the shortcomings of their current framework to a more comprehensive one—*but only once that next stage has already been achieved by the larger community*. Margolis provides a method for meeting standards rather than setting them. He fails to see that even

though it makes sense in politics to speak of "failed revolutions," all of Kuhn's revolutions are success stories. In other words, the only cognitive changes that Kuhn recognizes as "scientific revolutions" are the ones that moved scientists closer to our current paradigms. Beyond that, Kuhn has little to say about how such revolutions occur. To do so would involve accounting for a variety of individuals, most with interests quite distinct from those of the original revolutionary, but who nevertheless found that person's work of some use for their own (Fuller 1988: Chap. 9). Thus, Margolis mistakes reconstructed history for the real thing because he typifies in one individual a process that is better seen as distributed across a wide range of individuals.

2. We have just seen that a simplistic sociology informs the cognitive turn. Kuhn deserves more blame here than any of the latter-day cognitivists. He characterizes scientists as having a common mindset or worldview. This idea makes it appear that for a given paradigm, once you've seen one scientist, you've seen them all. Sociologists regard this typification of the group in the individual as a methodological fallacy, the "oversocialized conception of man [*sic*]" (Wrong 1961). This typification renders the social superfluous by ignoring how interaction enables a group to do things that would be undoable by any given individual. Philosophers are prone to an oversocialized conception of humans because of bad metaphysics. The *part–whole* relation is treated as a *type–token* relation. Society is an entity that emerges from parts assembled into a whole, not a universal type that exists through reproduced tokens. Indeed the signature products of cognitive life–knowledge, theories, rationality, concepts—are quintessentially social. They exist *only* in the whole and not in the parts at all. For example, cognitive psychologists commonly treat conceptual exemplars, or "prototypes," as templates stored in the heads of all the members of a culture (cf. Lakoff 1987). In fact prototypes may be better seen as public standards in terms of which the identities of particular items are negotiated. Each party to such a negotiation may have something entirely different running through her mind, but her behaviors are coordinated so as to facilitate a mutually agreeable outcome (Turner 1997, 2002).

3. Parallel distributed processing (PDP) models offer an account of the brain that starts with minimal common capacities and then builds up quite different neural networks depending on an individual's experience. However, the extreme context sensitivity of PDP models implies that whatever sustained uniformity one finds among members of a scientific community is due not to any uniformity in their private thought patterns. Rather, uniformity is found in the public character of their behavior, especially the language in which members of that community

transacts business. (In fact that might be the point of scientific language.) If PDPers are correct about the variety of neural paths that can lead people to say, do, and see roughly the same things, then the nervous system does *not* provide any particular insight into the distinctiveness of science as a knowledge-producing activity. Of course PDP would still say a lot about "how we know the world" in the looser sense of surviving in the environment. Whether the cognitivists can tell a story about scientific communication that says how findings are judged to be normal, revolutionary, or simply beside the point remains to be seen. For if thought proves to be as context-sensitive as PDPers suggest, communication is an unlikely process by which a later scientist reproduces an earlier scientist's thought processes in order to continue a common line of research. This conclusion is especially true if the relevant thought processes are defined in terms of what we now *retrospectively* regard as a "common line of research." Moreover, even if a later scientist wanted to pursue an earlier scientist's work, either her means or her motives for reproducing that work remain unclear (Wicklund 1989). The "concept maps" and other heuristics that cognitivists elicit from scientific texts are likely more formal analyses of scientific rhetoric that conveyed the soundness of the scientist's work than representations of "original" scientific reasoning that readers followed step by step in their own minds. Here I simply wish to put the accomplishment in perspective.

4. Finally, perhaps the grossest sociological simplification behind the cognitive turn may be termed its visually biased social ontology. On this view, social factors operate only when other people are within viewing distance of the individual. If no one is in the vicinity, then the individual is confronting nature armed only with her conceptual wiles. The solitary laboratory subject working on experimental tasks—the source of much of the cognitivists' background psychology—certainly reinforces this image. The biggest offense here lies in the failure to see that cognitive patterns are memories of socially framed experiences that are resistible and replaceable only in socially permissible ways. Altering one's point of view (e.g. adopting a new theory) and making a possible alternative the basis of one's subsequent research involves the simultaneous calculation of what philosophers have traditionally called "pragmatic" and "epistemic" factors. This process binds "the social" and "the natural" in one cognitive package that cannot be neatly unraveled into, respectively, impeded and unimpeded thought processes. Relevant to this point is the Machiavellian Intelligence Thesis (Byrne and Whiten 1987). This thesis argues that cognitive complexity is a function of sociological complexity. In other words, organisms that respond to environmental changes in a less discriminating fashion tend to be the ones with a less structured social existence. One conclusion

drawn by Byrne and Whiten is that the complexity of nature, distinctively uncovered by science, may be little more than a reflection of the combination of people who must be pleased, appeased, or otherwise incorporated before a claim is legitimated in a scientific forum. If, perhaps, more simply organized, then science would reveal a simpler world.

The Cognitive as Black Box

Those who are sanguine about AI's possibilities often regard both humans and computers as "cognizers" at a certain level of abstraction (Pylyshyn 1984). In contrast, skeptics often regard humans as necessary complements of computers. A human, for example, must interpret computer output for it to make sense. This difference may be cast in terms of the two sorts of operations that Piaget (1971) identified as essential to how people orient themselves in the world. The former involves enclosure in logical space (i.e. both humans and computers are members of the class of cognizers). The latter involves separation in physical space (i.e. humans and computers are distinct parts of one intelligent system). These two operations have precedents in the structuralist literary criticism as, respectively, *metaphoric* and *metonymic* modes of linguistic analysis (Culler 1975). Neither operation, as such, implies the superiority of either computer or human over the other. For example, in the metaphoric mode, cognizers can be defined so that either machine computability or human complexity is the norm against which the other is a degraded version. Likewise, in the metonymic mode, either humans may confer sense on computers or computers may serve to discipline human judgment (as with one of B.F. Skinner's programmed learning machines). Nevertheless, fights break out once defenders and opponents of AI enter a prescriptive mode. This mode typically involves treating the computer as a kind of "black box." Consider two ways in which both sides deal with this image.

The image of "closing the black box": AI boosters want to close the computer's black box by trusting its output and adjusting their interpretation of the computer's design. In so doing, they render the output to appropriately follow the historical tendency for instruments to become "cognitively impenetrable." Thus, one trusts the readings from the instruments even if it means discarding the theory one would like to see confirmed by the readings. Skeptics, however, close the black box by evaluating the computer's output by a standard external to the computer's design, such as human judgment. As a result, no intrinsic interest exists in the computer's operation, merely in the extent to which it simulates a predetermined understanding of what human beings can do. Whereas AI boosters adopt Daniel Dennett's "design stance" toward the computer, the skeptics adopt his "intentional stance" (Dennett 1987: Chap. 1). In the design stance, the black box is "closed" because it operates as a final authority on epistemic judgments. In the hands of actor-network theorists in science studies, such as Michel Callon and Bruno Latour (1981), the machine is made to appear to be a cynosure in terms of which many diversely interested parties must define (or "translate") themselves. As in Marx's analysis of commodity, the computer gradually shifts from being a mere medium of exchange to being something consubstantial with the parties involved in the exchange. Thus, as scientists come to rely on the outputs of expert systems to test their hypotheses, these systems come to be endowed with genuine expertise (Fuller 2002a: Chap. 3).

The image of "opening the black box": AI boosters envisage opening the black box as the process of discovering what enables the computer (or the human being, for that matter) to think. The answer, boosters assume, will be given in terms of subsistent and essential properties of the computer mechanism. In contrast, AI critics imagine that the black box will be opened when a history of the interactions between the computer (or human being) and other things has been written. While the concentration of intelligence in one enduring place marks a "cognitivist" orientation toward the computer, the diffusion of intelligence over time and space marks a more "behaviorist" orientation to what would be more properly called a "learning machine." (The cybernetic concept of "system" tried—with decidedly mixed results—to strike a balance between these two.) Thus, for boosters, "opening the black box" means peering inside the machine to see how its hardwired program constrains the range of potential environmental interactions. But for critics, the relevant sense of "opening" involves letting the contents of the box spill out and revealing the sequence of contingencies that have determined the machine's applications. While "opening" in the sense of "peering inside" would be regarded as *intrinsic*, "opening" in the sense of "revealing" would be seen as *relational.* As Herbert Simon first pointed out with regard to firms, what appears at a distance to be a consistent decision-making strategy may, on closer inspection, be seen as a series of ad hoc adaptations to environmental changes. Clearly, then, metaphors such as black box are too fertile for their own good.

AI'S STRANGE BEDFELLOWS: ACTANTS

As a social epistemologist inhabiting the ground common to positivism and constructivism, I believe that our continuing lack of understanding about moods and emotions may result from a lack of agreement over what we mean by "moods" and "emotions." The "mystery" here may be simply logistical and perhaps could be resolved by consensus on a computer application. My open-mindedness on this matter is directed against certain potentially obscurantist tendencies in STS. These tendencies, which I call *practice-mysticism*, hold that science's holistic nature resists any systematic, procedure-based analysis. Practice-mysticism can be traced to Michael Polanyi's (1957) stress on the "tacit dimension" of scientific knowledge. He sought to keep both methodologists (e.g. Popper) and commissars (e.g. Bernal) from holding scientists accountable to publicly scrutable standards.

Ironically, despite their radical patina, ethnographic studies of scientists often reinforce this image of inscrutable competence. Ethnographers presume that scientists do indeed know what they are doing and, further, that this knowledge can be gleaned only by becoming acculturated to their specific habitats, paying attention to what the scientists do, not to what they say. The advantage of computer simulations in this context is to remind us that complexity need not imply ineffability or inscrutability.

At this point, epistemology and ontology start veering into political economy. Is a computer *entitled* to know? Should we confer epistemic status on its outputs? But before broaching this question, the possibility of practice-mysticism must first be brought right to our own doorstep. A principal source for contemporary work in the rhetoric of science has been academic programs in *technical communication*. These programs are designed to enable ordinary people to do or use technical things. (An exemplary text is Collier 1997.) People so trained often produce comprehensible and instructive manuals for people wishing to use particular gadgets. The technical communicator thus assumes that anything can be explained to anyone given enough time. If the time is not always available to articulate all that a particular person needs to know, the problem is regarded as one of economics, not ontology.

From the technical communicator's standpoint, then, the practice-mystic misreads her own impatience as the unskilled's incompetence: One's own need to apply effort becomes a measure of another's cognitive liabilities. The same applies to our own (un)willingness to interpret computers as having done enough for us to attribute certain thoughts and capacities to them. I may lack the time, imagination, or interest to interpret the computer as performing intelligently. Perhaps I reach this interpretation because I have more important things to do and the computer is in no position to prevent me from doing them. Or because I would have to end up interpreting the computer as doing something other than I would have expected or liked it to do. The political implications of this point (which resonate with much in the critical literature on colonialism: cf. Forrester 1985: pt. II) become clearer as we turn to Harry Collins' sophisticated defense of practice-mysticism in *Artificial Experts* (1990).

Collins argues that computers will probably never be recognized as "members" (or "peers") in a scientific community if the community's local standards hold sway. His detailed accounts of computer ordeals remind one that the "computer" often stands for anyone who can pass all the regular examinations, but does not come from the right background. Ever stiffer tests are set—usually ones that members with the "right background" would be hard-pressed to meet—and ever less charitable readings are given to the individual's responses. Indeed Collins signals what, in a more politicized context, would be called a "prejudice" against computers by admonishing that "our" humanity may be endangered by allowing machines too quickly into the fold of intelligent beings. Of course "our" doesn't mean all of humanity, but only those members of *Homo sapiens*. In the unabashed language of the 18th century, *homo sapiens* have the appropriate "taste" or "sensibility" to evaluate others who might lay claim to some humane qualities. Today skill and expertise are the preferred terms of art (cf. Bourdieu 1986), yet whole classes of people are still just as eligible for exclusion as classes of machines. In other words, although Collins (1990) is ostensibly about distinctions between humans and computers, his work is really about distinctions that already exist among humans, but whose coverage, in recent years, has been extended to computers.

Two sorts of strategies uphold the political economy of expertise. The first sort makes one's initiation into a community of experts difficult and hence relatively rare. Were everyone considered competent in some sphere of action, it would probably lose "expert" status. Instead one would probably start assimilating that area to the debased epistemic currency of "habit," "routine," or "common sense." The second sort maintains a double standard of evaluation for "experts" and "novices." Once you are presumed to be expert, the level of scrutiny drops considerably as the extent of your discretionary judgment rises. Thus, actions that might seem anomalous if performed by a novice are allowed to pass and perhaps are even taken as innovative in the hands of an expert. Dreyfus and Dreyfus (1986) presume that the difference in evaluation is due to different properties of the evaluated. I argue, in fact, that the difference lies in the attitudes in the *evaluator*.

Once we regard someone as sharing our lifeworld and behaving within the confines of "civility," then the behavior passes as normatively acceptable. This default standard may reinforce numerous behaviors whose variety can give the impression that people are many splendored things. But in fact we may only reveal the coarse-grainedness of our standards and our willingness to turn a blind eye. In other words, "nuance" and "skill" may be expert *overinterpretations* of behavioral variation that normally escapes the notice of the natives. Thus, human "unpredictability" may be explained as an artifact of an imperceptible, but quite ordinary, shifting of our standards of behavioral scrutiny. Consequently, the context in which we initially predict someone's behavior is typically different from that in which we later evaluate the prediction. (Fuller 1992a provides a critique of pragmatist philosophy from this standpoint.) However, we do not normally extend this interpretive charity to machines, which means that their performance is typically scrutinized under conditions that more closely resemble those of a laboratory experiment. For a human to appreciate what such conditions are like, one would have to imagine the level of suspicion that surrounds being regarded as a stranger to a community.

Flattening the ontological difference between humans and machines has been a strategy pursued on both the AI and SSK sides of the debates. Alternatively, this strategy is called *android epistemology* (Glymour 1987) or the *sociology of machines* (Woolgar 1985). The full range of entities subsumed under these two pursuits may be called *cognizers* (Pylyshyn 1984) or *actants* (Latour 1987). Although some may regard lumping together of humans and nonhumans as dehumanizing, this process really aims to democratize our interpretive sensibilities. After all politics transpiring behind the scenes of interpretation determine whether the utterances and actions of *humans* are to be given the benefit of the doubt or treated with the utmost suspicion. Moreover, most procedure-based theories of rationality, be they derived from economics or from epistemology, work better on computers than on humans. Therefore, if humans are willing to evaluate their own thought and action in light of these theories, then why not credit the computer with *some* measure of cognitive ability? Exactly how much depends on how often we change our behavior in light of what the computers say (Fuller 1989: Chap. 2).

I agree with much of the tenor of AI work that treats the computer not as merely a model, but as a virtual agent in the scientific enterprise. Being an only slightly reconstructed modernist, I believe that as you become more conscious of the mechanisms of conceptual change, you can change your concepts more freely (cf. Dolby and Cherry 1989). This view also reflects the implicit position of most AI practitioners, who want to grant the computer at least some epistemic authority (i.e. there are certain cases in which we should trust the computer's judgment over our own; Faust 1985). Ironically, that much-battered behaviorist B. F. Skinner bears some credit for my enlightened attitude toward computer agency. Skinner's original programmed learning machines were designed to shape the behavior of students who wanted to learn linguistic and mathematical skills by subjecting them to the principles of operant conditioning—the machine doling out the appropriate reinforcement for each student response. In a world where knowledge of, say, mathematics is valued largely for its abstractness and precision, why wouldn't one of Skinner's machines be the ideal entity under which to do one's apprenticeship?

Some SSKers may be disturbed by these developments, but any loss of sleep would be the result of SSK's failing to follow through on its own message. If SSKers generally hold that the meaning of one's actions is what the community takes them to mean, then why should this not also apply to whatever a computer does? Put in terms of the Turing Test, if you can confuse the machine with a scientist, then it simply *is* a scientist. Given the great lengths that SSK has gone toward conventionalizing even the slightest hint of a human essence, it would be only consistent to argue that convention alone causes us to trust human over computer utterance. Indeed we already defer to the epistemic authority of the calculator over our own or some other human's computational efforts. Admittedly, arithmetical computation is not the most esteemed form of cognition, but perhaps that is due precisely to its being a task conventionally delegated to machines. If so, imagine the implications for the ordinary conception of science if scientists routinely trusted the output displays of not only calculators and meters, but hypothesis-testing machines as well! An interesting unintended consequence of coming to accept BACON and its successors as competent prosthetic reasoners may be to remove the cognitive functions that these machines perform from the valorized realm of "science." In short, in trying to understand scientific reasoning, AI may unwittingly end up drastically altering the social definition of science.

THOUGHT QUESTIONS

☙ How is cognitive defined? What aspects of science does cognitive seemingly obscure? How do the various meanings and uses of cognitive lead to incommensurable positions in the debate over AI?

☙ What are the differences between scientific controversies that occur in an academic setting and in the public sphere? What happens when scientific debates move from academic settings to public settings? What does the history of science tell us about the conduct and resolution of scientific debates? Is there a cultural basis on which theory choice can be decided?

☙ In the public debate over the question of "*Can computers think?*" Fuller claims ambiguities regarding the definition of terms like intelligence and computers, and the descriptive or normative direction of questions can be put to "strategic advantage." How? What does the social epistemologist do after diagnosing incommensurable positions in a public debate?

☙ How does describing science as "irreducibly cognitive" or as "irreducibly social" shape questions regarding the acceptance of a scientific claim? Is the incommensurability of claims made by AI or SSK regarding scientific discovery a matter of communication? How?

☙ Does selecting a scientific hypothesis, the outcome of which is known, and then testing it through a computer program (BACON) succeed only in begging the question?

☙ Assuming that scientific theory choice is underdetermined by internal evidence (e.g. experimental results), when do external (e.g. social) factors come into the equation? Is the dispute between defenders of AI and SSK simply a matter of demarcating when or if an account of theory choice should take external factors into consideration?

☙ What rhetorical challenges does the term artificial intelligence pose in the AI–SSK debate? How does Slezak's use of the canonical histories of the cognitive revolution rhetorically shape this debate? What does explanation in the natural sciences achieve, rhetorically, that explanations in cognitive science try to emulate?

☙ Fuller positions Slezak's arguments within the context of the "canonical history" of the cognitive revolution leading to the "cognitive paradigm." Initially, how does Fuller characterize the problems with the canonical history of the cognitive revolution regarding its founders? On what do the "cognitivists" agree and disagree? How is the cognitive paradigm established? How does Fuller position Slezak within the context of the story of the cognitive revolution? What are the rhetorical aims of the canonical history of the cognitive revolution? What are Fuller's aims in recasting the history of the cognitive revolution with respect to the AI–SSK debate?

☙ How do Simon and Chomsky figure in positioning cognitive theory with regard to Skinner and the behaviorists? How does Fuller's "great man" history of the cognitive revolution square with a social epistemology?

☙ Is there a distinction between knowledge that is "universal" and "univocal" and knowledge that is "realistic" or "objective"? Are there rhetorical differences in holding that scientific knowledge is either objective, realistic, universal, or univocal? If so, what are these differences? How might these differences inform the AI–SSK debate specifically or scientific controversies generally?

☙ In adopting the cognitivist position, as Fuller portrays it, what conclusions do we draw about individual scientists and the science they produce? Does Fuller's own characterization of the "cognitivists" and the direction of cognitive science rely on a form of "cognitive history"?

☙ What does Fuller mean by "locating abstractions in the empirical world"? Taking "paradigm" as an example, what comes of empirical observations based on the affect of abstract concepts? What "sociological simplifications" does cognitive science make? Does SSK make "cognitive simplifications"?

☙ What is a black box? How is the term used in other disciplines?

☙ What does Fuller mean by "practice-mysticism"? How does this phenomenon correspond, or run counter to, a sociological account of scientific knowledge?

☙ Are computers "virtual agents" in the scientific enterprise"? Are computers "virtual agents" in social scientific enterprises like SSK? How might one come to understand human agency differently if agency is ascribed to machines? How might one come to understand social aspects of science if agency is ascribed to machines?

6

Excavation, or the Withering Away of History and Philosophy of Science and the Brave New World of Science and Technology Studies

Under Thomas Kuhn's (1970) influence, methodological debates in the humanities and social sciences commonly address where one's discipline is on the road to becoming a "paradigm" and how a "revolution" may be staged to set the discipline aright. However, the remarkable ability of the field of history and philosophy of science (HPS) to establish spheres of influence in other disciplines is no indicator of the fate of the Kuhnian revolution at home. To be sure, historians of science have succeeded in pulling in a few philosophers to examine the details of past science. These philosophers have, perhaps, a greater sense of science's institutional character than before the Kuhnian revolution, but in a way that is still studiously atheoretical and nonprescriptive. Moreover, with the latest revival of scientific realism, philosophers of science have returned to a quasi-transcendental mode of arguing that betrays their roots in classical epistemology and metaphysics (e.g. Leplin 1984). Thus, we have realists proffering just-so stories about what "must have happened" in history to enable science to be so "successful." Instead of raising historical counterexamples, antirealists tell simpler versions of the same story. The homeliness of the scenarios imagined often takes the place of critical historical scholarship. This retreat from the Kuhnian revolution is significant. It reflects an ambivalence on the part of HPS toward breaking new theoretical ground, specifically, *an ambivalence toward making the transition from the humanities to the social sciences*—a reluctance to make the transition from HPS to STS.

POSITIONING SOCIAL EPISTEMOLOGY IN THE TRANSITION FROM HPS TO STS

How can a social epistemologist accuse HPS of dragging its heels along the inevitable path toward STS? In brief compass, my answer is this: Like HPS, social epistemology also starts off "humanistically" in using the language of science as the entry point for understanding the nature of science. What becomes immediately evident, however, is that the descriptive and prescriptive functions of scientific language are in tension with one another, and hence require rhetorical management. In short, certain things appear in the world only because certain other things have been made to disappear.

Only once the normative conditions enabling this rhetorical management are uncovered can one then envisage alternative normative conditions that would produce alternative forms of knowledge. Thus, the social epistemologist quickly moves from deconstructing texts, to surveying the material bases of power relations, to designing experimental utopias—from humanities to social science! Now let us look at this transition a little less breathlessly.

Ordinary language is ill suited to any of the usual philosophical conceptions of epistemic progress. The relatively unscrutinized level of ordinary usage serves more to maximize a sense of group identity and historical continuity on the part of the language users and less to establish the exact extent or even presence of some common objects of agreement. This phenomenon, called by the American rhetorician Kenneth Burke (1969) the "consubstantial" quality of discourse, enables numerous people to move in a common direction without ever having to reach explicit agreement on a label for that direction. For example, a call to "patriotism" may unite many citizens in war, even though, if asked, they would probably give divergent opinions of what they are defending when they defend their "country." The positivist account of language as tool, typified in A. J. Ayer's (1936) emotive theory of ethics, is sensitive to this point. That is, unless special institutional arrangements are made—say, the introduction of a verificationist semantics—language functions primarily to move people to act, speak, and feel in certain ways. Nothing as fine-grained as the distinction between truth and falsehood is required for these functions to be performed. Here the positivist parts company with the pragmatist, who holds that instrumental success and long-term survival are prelinguistic surrogates for truth found throughout the animal kingdom. In siding with the positivist, I admit that the search for truth is quite an artificial inquiry directly tied to the regimentation of linguistic practice. Such an inquiry cannot simply be reduced to brute pragmatic utility.

From the scientific standpoint, the consubstantialist tendencies of ordinary language foster miscommunication and cognitive stasis by minimizing the opportunities for expressing latent differences. Such opportunities are presented once an utterance is held to stricter standards of accountability, even if that means simply asking more follow-up questions. The Socratic dialogues illustrate this move, whereby two people who originally assented to some seemingly simple proposition are asked to articulate the reasons for their opinion. These explanations turn out to reveal a deep disagreement that then requires philosophical assistance for its resolution. Applied systematically, such assistance aims to reconstitute ordinary linguistic practice into one that can be regularly scrutinized and, thus, rendered an appropriate vehicle for epistemic progress. In this way, truth and falsehood become institutionalized as properties of utterances. However, this institutional arrangement, often called "representation" or "reference," is rather expensive to maintain and goes against the efficiency of language as a prod to action. For a variety of procedures and products—repeatable experiments, canonical methods, final examinations, pure samples—must be established to which specific utterances can then be shown either to correspond or not. This variety embodies the process of "standardization." We are able to say that standards are subject to "determinate" readings because of the control that is exerted over who can speak for them. In turn, an "objectivity" is conferred on the utterances that are held accountable to those standards. What I have just described is the verificationist theory of meaning expressed as a piece of sociology.

In everyday life, one presumes an utterance moves its audience unless explicitly challenged. Once the utterance is challenged, the speaker will often justify it by invoking standards. These standards would test the validity of the utterance if construed representationally. However, under normal circumstances, invoking standards simply serves to terminate discussion of the issue and to move the conversation to some other topic. Hence, one must distinguish the *representational function of language* from the *rhetorical function of representation*. The representational function of language involves a vast deployment of human and material resources for what are, essentially, surveillance operations. In contrast, representation's rhetorical function involves that one grants an utterance the same warrant for action as one grants surveillance operations (that would ideally stand in the utterance's place). This representationalist rhetoric commonly occurs whenever one scientist incorporates another's results into her own research without feeling a need to reproduce the original study.

Here is a piece of philosophical shorthand that epitomizes the way in which the social epistemologist combines views on the nature of knowledge that are typically seen as antagonistic. Am I a *scientific realist*? A *logical positivist*? Or a *social constructivist*? The answer is that I am all three. My realism is predicated on positivism which in turn is predicated on constructivism. The difference between the three positions is that the social dimension of knowledge is least evident in realism (which, as in Peirce, always alludes to the theoretical language of a community in the indefinite future), somewhat more evident in positivism (which makes the possibility for knowledge relative to a currently available language), and completely self-conscious in constructivism (which relativizes knowledge still further to an extension of the language currently in use).

In a sense, the social epistemologist wants to beat the positivists at their own game. The social epistemologist envisages implementing the positivist account of language. The whiffs of Burke and Foucault are meant to vivify a point that can be traced to Wittgenstein and Carnap: Truth and falsehood are properties of sentences in a language designed to represent reality; prior to the construction of such a language, neither truth nor falsehood exists. But unlike the positivists and their logical forebears, I take account of the diachronic dimension of language. Most speakers of a language will have interests and understandings quite different from, and often at odds with, those of the originators of the language. From that I infer that, once routinized, verification practices become more susceptible to consubstantiality effects as similarly trained individuals come to take for granted that others mean what they mean when they say certain things. Thus, although routinization bespeaks a certain efficiency of practice, it also opens the door to incommensurable conceptions that rise to the surface only during a round of critical inquiry, as, say, happens during a "crisis" in one of Kuhn's paradigms. In that sense, the success of scientization (i.e. routinization of scientific standards) sows the seeds of its own destruction.

One can also see this point in terms of what marks the conceptual transition from an instrumental to a representational approach to language. In this instance, one's personal ends are no longer sufficient to justify the linguistic means used in their pursuit. Once enough misunderstandings, deceptions, and failed ventures have been acknowledged, people will realize their interests are best served by making their usage first satisfy some mutually agreeable end—a standard—before it can satisfy more personal ends. As a result, one's pursuits are less direct, but also less arbitrary. Everyone will have an interest in catching violations before they contaminate activities of the entire community. The first systematic effort to make this transition occurred during the Scientific Revolution of 17th-century Europe.

Although generally seen as transforming attitudes toward the natural world, the Scientific Revolution is better taken as having introduced a new attitude about *ourselves*—namely, as *imperfectly embodied standards of knowledge.* Thus, Francis Bacon expressly presented the experimental method as a form of self-discipline designed to counteract cognitive liabilities or Idols of the Mind. The evolution of experimental method over the next 350 years is likely the most important contribution that *psychology* has made to social epistemology; namely, a series of proposals for institutionalizing inquiry so that the whole of human knowledge may consist in something more than the sum of the participating human knowers. This notion of inquiry is one that I endorsed as *naturwissenschaftlich* (i.e. natural-scientific) in *Philosophy of Science and Its Discontents* (Fuller 1989). Contrary to how such matters are normally understood, the experimental method can be seen more as a means for *macro*reproducing the lab in the world than for *micro*reproducing the world in the lab.

Historians and anthropologists often note the cultural distinctiveness of the Scientific Revolution. Nevertheless, they disagree about what exactly constituted the epistemic "takeoff" that led the West to surpass China, India, and the Islamic world in knowledge production after 1700. What crucial "factor" or "idea" was absent in the East that was present in the West? Although a historical dispute of such magnitude and complexity lies beyond our scope, social epistemology's sense of the history of science offers up an answer that may be pursued as a hypothesis. Rhetorically speaking, the Scientific Revolution enabled the translation of theoretical speculation into experimental practice. To give this point a more perceptual spin, Western scientists came to see experiment as not merely an instantiation of theory, but as a test for the well-foundedness of theory. The trick here was the realization that because we are inherently imperfect knowers our reasoning processes are unlikely to reach the truth simply of their own accord. Experimental techniques and apparatus, then, become both prosthetic devices to extend our cognitive capacities and standards against which those capacities are evaluated.

The computer's dual role as the extension and the measure of rationality in the modern era is a clear case in point. By contrast, although the East had the technology and the speculation, the two were pursued independently of one another. No matter how certain or fallible our reasoning processes were taken to be by the Eastern philosophers, those processes were treated as self-contained or at least not enhanceable or revisable by technological mediation. Generally, this condition arose because the human soul was held to already contain the essential ingredients of reality, much as Plato and Aristotle had thought in Western antiquity. In the end, then, the difference between Occident and Orient, *circa* 1700, boiled down to ontology: The Occidental philosopher portrayed humanity as an incomplete part of the natural world, whereas the Oriental philosopher portrayed humanity as a micro-instantiation of the entire world order. Only the former was suited to modern science (Fuller 1997: Chaps. 5-7).

The long prehistory of Mill's Methods of Induction testifies to the existence of experimental ideas in the West before the 17th century. However, earlier attempts to isolate necessary and sufficient conditions were speculative, and hence had largely consubstantialist effects. Thus, medieval arguments about some factor's being the *sine qua non* of some state did more to elicit a sense of group identity between author and reader than to open the claim to empirical scrutiny. Indeed this rhetorical appeal to thought experiments is very much alive today. The definitions of knowledge proposed by analytic philosophers, for example, turn on test cases so well rehearsed in "the literature" that they enjoy the status of "intuitions" among the cognoscenti (cf. L. J. Cohen 1986). Also in this category are most historians' narrative attempts to isolate causes. In this instance, what remains unclear is whether one is persuaded by the general familiarity of the historian's plotline or its particular relevance to the case under study. This approach to knowledge, or *geisteswissenschaftlich,* is subject to criticism (Fuller 1989).

As long as historians present themselves as knowledge producers, the principles providing the implicit warrant for their causal analyses are open to social scientific scrutiny. The business of the social sciences is, after all, generating and testing such principles. These principles consist of the assumptions that historians make about people's motivations, collective tendencies, and cognitive horizons. The positivists, in advancing the "unity of science" thesis, were reminding inquirers investigating similar subject matter that they are accountable to each other's epistemic standards. From these standards, inquirers may then negotiate a common standard of confirmation and explanation (Hempel 1965). Thus, historians open themselves to criticism by psychologists if they borrow outmoded theories, just as psychologists open themselves to attack from historians who question the generality of their experimentally derived principles. This dynamic, in short, is the implicitly scientific character of historical explanation.

THE PRICE OF HUMANISM IN HISTORICAL SCHOLARSHIP

History is both an admirable and an atavistic discipline. It is admirable for courting a wide public readership that extends beyond the academy. However, history is atavistic in continuing to publicly portray itself as a relatively neutral resource for finding out what actually took place. Moreover, history suggests its claims can be assessed simply in terms of their conformity to the available evidence, not in terms of their conformity to general explanatory principles put forth by, say, the social sciences. Both philosophers and sociologists of science frequently err in their use of history when insisting on isolating certain decisive "factors," be they "internal" or "external" to the knowledge enterprise, that are responsible for determining the course of science across a variety of sociohistorical settings.

Consider the infamous exchange between philosopher Larry Laudan (1977) and sociologist David Bloor (1976) over the *arationality assumption* (sociology explains only the arational parts of science) vis-à-vis the *symmetry principle* (sociology can explain both the rational and arational parts of science). Historians have been inclined to look on the dispute as purely ideological, a mere cross-disciplinary turf war (J. R. Brown 1984 recaps the debate). After all, hasn't the historical record shown that *both* sorts of factors are at work all the time? Moreover, doesn't the inconclusiveness of the Bloor–Laudan debate prove the bankruptcy of any attempt to infer generalities from the history of science? A good case in point would seem to be the chilly reception given to Laudan's attempt to stage a "crucial experiment" between the two viewpoints (L. Laudan et al. 1986; R. Laudan et al. 1988).

Under the rubric of "normative naturalism," Laudan (1996: pt. 4) proposed a research program whereby the central claims made by internalists can be put to the historical test on, so to speak, a case-by-case basis. The fruits of this project would earn Laudan a place alongside Francis Bacon, Jeremy Bentham, Francis Galton, Pitrim Sorokin, and the other great tabulators of our times. In theory, a bureau or institute could be entrusted to collect the data, periodically publishing the latest tallies in a handbook (e.g. "Do scientists wait for a new theory before giving up an old one plagued by anomalies? In 10 cases this happened, but in 8 cases not"). A good way to look at this project is as an empirical test of Laudan's "arationality assumption." This assumption offers that norms exist, of sufficient transhistorical purchase, to count as rational grounds for theory choice in science. Laudan deliberately chose enumerative induction—the idea that each confirming case counts in favor of a hypothesis—as his guiding metanorm. Enumerative induction is a method that virtually every philosopher (Popper being the exception) has taken to lend credibility to a knowledge claim. Laudan hoped that the project would come up with less intuitive ones as well. However, if none or very few of the 300+ norms under consideration were decisively accepted or rejected by the annals of science, then that would indirectly lend support to Laudan's externalist foes. They argue that theory choice is primarily determined by local social factors in which methodological appeals figure willy-nilly.

Despite Laudan's care in formulating the norms and to justify his inductivist testing procedure, his project has been resisted from all quarters. Is this resistance simply a case of theorists not wanting to see their pet theses falsified? The main objection that has been voiced so far to the Laudan program is this: Even if every philosopher has endorsed enumerative induction as a necessary part of the scientific method, certainly no philosopher has endorsed it as the entire scientific method. Indeed the various other methods proposed by philosophers have generally been designed to *counteract* the irrational consequences that would follow from the strict pursuit of inductivism (cf. Nickles 1986). Moreover, Laudan's historical appeals notwithstanding, his selection of enumerative induction as the project's method reveals more of his true, *nonhistorical* interests. In effect, Laudan has abstracted a lowest common denominator from the views of various philosophers of science and then reified it as the essence of the scientific method. Thus, he has mistaken what metaphysicians call an "accidental universal" (i.e. a feature common to a set of particulars that fails to define their real nature) for a "natural kind."

But this essentialist strategy is not unique to Laudan or even to philosophers. When philosophers and sociologists debate the merits of internalist versus externalist histories of science, they are really contesting a point of *ontology*: Does science have a transhistorical essence ("Is science *sui generis*?" as Durkheim might have asked), or is science reducible to a historically persistent combination of some other, specifically social, sorts of essences? But first we need to diagnose and evaluate the reluctance of historians to take sides on this issue.

Some historians erroneously believe that an ontological debate of the sort just described could not be adjudicated by historical means. In a positivist spirit, these historians conclude that the debate should be terminated. True, a simple reportage of history "as it actually happened" will not do the job. But then historians are not the only ones who make use of historical evidence—everyone does. In fact the territorial claims historians make over historical evidence bear an unfortunate resemblance to traditional philosophical claims to expertise over what is rational. When speaking in this territorial mode, one acts as if an intruding discipline has only two courses of action. In the case of historical turf, the intruder has the option of either submitting to the scrutiny of historians or admitting that she is using historical evidence in a (probably less literal) way that evades the standards of historical scholarship. In neither case is the intruder made to feel like she is doing something intellectually worthwhile. Thus, history's autonomy results from xenophobia; specifically, a fear of being held accountable to the claims made by other disciplines that draw largely from the same body of evidence.

In trying to alleviate their xenophobia, some philosophers make their theories of rationality accountable to the findings of economists (especially rational choice theorists) and psychologists (especially cognitive scientists). Still historians remain more reluctant to admit officially that the validity of their research is affected by the findings of other disciplines. However, the practice of historians reveals a less consistent stance, one captured by the following observations: (i) historians maintain that they range over a distinct intellectual terrain while philosophers and social scientists periodically wander into the historian's turf; (ii) historians reinvent aspects of other disciplines in their studies even though they are more clearly articulated, and tested theories on those aspects are readily found, in the disciplines themselves; and (iii) historians, when it suits their purposes, will sometimes rely on the research of other disciplines. But when disciplinary research does not suit their purposes, historians either ignore or criticize it on methodological grounds—even if the methodology employed (e.g. controlled lab experiments) was the same as that of research on which, on another occasion, they had relied.

Yet historians are no different from other specialists. One of the social epistemologist's primary academic functions is to compensate for these liabilities. They, nevertheless, tend to be especially trenchant in the humanities, where the presumptive generality of "human nature" traditionally licensed casual sampling from the literatures of the special sciences. Indeed the "liberality" of the humanist's general education is supposedly displayed in such bibliographic forays.

The inconsistency noted in the historians' behavior suggests that a double standard is afoot—a double standard seen in a recent version of the internal history of science, "cognitive history" (briefly discussed in the previous chapter). Much cognitive history consists of intellectual biography, a reconstruction of the thought processes of great scientists, usually from private notebooks and with the aid of the conceptual apparatus of cognitive psychology. This work tends to be done by people who have had substantial training in "cognitive psychology" broadly construed (i.e. including not only recent lab and computer work, but also Gestalt and Piaget) and who openly support the HPS movement (e.g. Holton 1978;

Gruber 1981; Nersessian 1984; Tweney 1989). These histories have an uncanny tendency to provide "independent corroboration" for internalist theses. But if double standards are afoot, this so-called corroboration should be traceable to the suppression (or ignorance, as the case may be) of countervailing considerations located in the psychological literature on which these historians draw. Let me now turn to three obvious instances in which this event occurs: (1) *the fixation on genius*, (2) *the presumption of scientific competence*, and (3) *the analytic significance of individuals*.

The Fixation on Genius

Historians influenced by developmental psychology are prone to argue either that geniuses (e.g. Einstein) achieved a sixth stage of cognitive development after having exhausted Piaget's normal run of five stages, or that near geniuses (e.g. Poincaré) failed to make the big discovery (e.g. relativity) because they were stuck at stage five (cf. A. Miller 1986). In effect, this line of reasoning supposes that the relative significance of individual scientists to the scientific enterprise is an implicit acknowledgment (or at least a reliable indicator) of the relative quality of the scientists' minds. On this account, the principle of scientific progress is that the entire community should try to approximate its most intelligent member.

This principle may well have functioned as a regulative ideal during the Heyday of Humanism, the 16th-century Renaissance. Then scholars saw themselves as recovering the pristine wisdom of the ancients that had become vitiated through repeated cultural transmission. Still to portray the spread of relativity physics in the first three decades of this century as a matter of scientists playing catch-up with Albert Einstein would be anachronistic. Even on her own terms, the cognitive historian would have a hard time explaining how rank-and-file physicists could come to reproduce routinely a discovery that originally took incredible mental powers (although perhaps she could argue, with a little help from scientific realism, that Einstein just needed fewer clues to arrive at relativity, which then enabled him to set down the additional clues that the rank-and-file needed). Even reasonably sophisticated inquirers interested in improving the social conditions of knowledge production (e.g. Root-Bernstein 1989) focus more on how individuals may generate better ideas than on how ideas may circulate better in the scientific community. Moreover, systematic psychological studies of scientific discovery suggest that quality of mind is *not* what separates the geniuses from the also-rans of science (Langley et al. 1987).

We know enough about the psychology of intellectual reception and appropriation to suppose that scientists would *try* to understand Einstein only as their own purposes determined. Specifically, scientists would regard the theory as an incomplete or partly erroneous view of things, which their own contribution aimed to correct (Wicklund 1989; cf. Fuller 1988a: Chaps.

5-6). A wide range of studies suggests that a group working on a common set of problems is much more effective than any single member in eliminating error and almost as good as its most insightful member in arriving at correct solutions. Together these traits make groups consistently better than individuals in most forms of problem solving (Clark and Stephenson 1989). These findings already start to explain why relativity theory was adopted and extended in quite a variety of ways. Many of these conceptions were unexpected and even unsatisfactory to its creator, although better than if relativity theory had been merely transmitted intact in its original conception. This reception also suggests a tradeoff exists between *doing* (writing) good science oneself and *recognizing* (reading) good science in others.

The Presumption of Scientific Competence

Even when the cognitive historian does not treat the great scientist as a genius, he (sic) can seemingly do no wrong or, at least, does wrong for only a short time. Cognitive scientists are now generally agreed that heuristics are liabilities on borrowed time—mental shortcuts that work well in a limited domain but disastrously outside of it (Fuller 1989: Chap. 3). We should expect, then, that a heuristic-based account of a scientist's thinking over a span of several years would illustrate a great many cases of cognition run amok, perhaps never to be resolved properly in the scientist's own mind. Unfortunately, cognitive historians tell us that, say, Michael Faraday just so happens either to access the right heuristic at the right time or to correct a misapplied heuristic by the time the story is over (cf. Tweney 1989). The probability that this rendering would capture the thinking of a real human being, given our best theories of cognition, is minuscule.

The humanist demand for a well-told story—one where the hero wins in the end—often undermines the scientific credibility of the cognitive historian's account. Not surprisingly, the Faraday case is greatly aided by meticulous notebooks that Faraday deliberately kept to assist himself in developing a continuous line of thought. However, our cognitive historians make reference to this fact apparently without realizing that Faraday's meticulous "metacognition" may render his notebooks a *more*—not less—opaque, overwritten record of what he did in the lab. The next issue is the reliability of Faraday's memory, the first thing that a psychologist would question, given the time lag between the events in his lab and Faraday's recording of them in his notebooks. Indeed "protocol analysis" (cf. Ericsson and Simon 1984), an entire subfield of psychology, is devoted to this issue, one routinely raised in the manuals on historical inference published a century ago (Dibble 1964).

The Analytic Significance of Individuals

Exactly what is wrong with making the individual scientist, genius or otherwise, the unit of historical analysis? After all, in the interest of thoroughness, the historian will be forced to take in the scientist's cultural context and thereby sweep up ambient social factors. The casualness of this response reveals that historians still think of themselves as akin to novelists for whom the choice of subject is largely a matter of personal taste. However, much more is at stake here. A remarkable point of convergence among ordinary language philosophy, experimental cognitive psychology, and cross-cultural anthropology is that our concepts are normally calibrated to fit our visual horizon. It thereby becomes the default level of ontological analysis. If people tend to regard a freely moving, foregrounded object as causally determinative of its surroundings (Kahneman 1973), then the historian's choice of subject plausibly represents an implicit causal judgment about the events and entities that have made a difference in history. Thus, even when the cognitive historian portrays Einstein and Faraday as "socially situated reasoners," this phrase serves only to frame the portrait. The historian makes sure that the scientists are portrayed as having transformed their contexts in ways that are interesting for the subsequent development of scientific thought (i.e. a Kuhnian "exemplar"). By being invested with such self-possession, the great scientist is conceptualized as an "agent" (cf. Harré and Secord 1979). Rare is the cognitive history that locates a scientist's most distinctive contributions in a misunderstanding or some other sort of error, perhaps on the part of influential readers. (One controversial example is Fuller 2000b, itself on Kuhn.)

Why do historians of science continue to focus on great individuals as causal agents (in the manner of political historians) and not on more aggregate notions of institutions, cycles, and trends (in the manner of economic historians)? The reason is certainly *not* because more detailed historical work has shown that individuals matter more than groups in determining the course of science. On the contrary, each new sophisticated history of science seems to uncover crucial social factors that change one's entire sense of what transpired. However, these social factors—such as Max Weber's triad of class, status, and power—have an ontological diffuseness that renders them unwieldy tools with which to think about the mechanics of historical change in science. Unsurprisingly, then, historians who freely wield these Weberian entities are often criticized for endowing them with agent-like qualities, as if a class, say, were itself a kind of purposeful superindividual who presses ordinary individuals into its service. If a clear case exists of our natural modes of thought (or "cognitive biases") working against what we are trying to think, our attempts to overcome the fixation on individuals by treating social factors as new individuals (Tilly 1991) would be it.

One solution is to tell the history of science as the history of a *distributed object*. Copies of this artifact can be mass produced and inserted into many situations, thereby generating a dispersion of effects (for related projects, see Daston 2000; Hacking 2002). The obvious candidate is the *book*—taken not as the captive essence of a great mind, but as a commodity whose value is negotiated in a variety of local exchanges (including the exchanges it took to concentrate the capital and labor needed for producing the original copies: Fuller 2002a: Chap. 2). If vestiges of authorial intent are not illicitly introduced as an invisible hand, we should be presented with a rather chaotic history of the book. The result is a diffuse pattern in which the artifact shaped behavior quite differently in different settings, with many parallel and interactive effects in tow. The pattern's unwieldiness would probably lend itself more to spatial than to linear presentation: messy tree structures more than neat lists. In any case, the result would be to disrupt the mnemonic compulsion to collapse the history of science into a sequence of great discoveries by great people through which the World-Historic Spirit has happened to pass. For only sheer memorableness lulls historians into continuing to center their narratives around individuals often in spite of what they know about how the history really works.

Nevertheless, the legitimatory function performed by telling the history of science as a series of heroes should not be underestimated. The sequence of Aristotle, Copernicus, Galileo, Newton, Laplace, Maxwell, and Einstein betrays the hand of the textbook tradition. The principal aim of this tradition is to present the welter of past discoveries in a pedagogically tractable form even if one cannot say for sure how one hero "laid the groundwork" for the project of the next hero (although the impulse is diagnosed next as symptomatic of an "overdetermined" historical sensibility). If more self-conscious about the pedagogical psychology that makes the trail of geniuses such a convenient way to envisage the history of science, cognitive historians would probably not as quickly associate the *mnemonic* and the *causal* significance in their selection of subjects.

A SYMMETRY PRINCIPLE FOR HISTORICISM

The three points just examined show how cognitive historians of science remain locked into a humanistic frame of mind. For all its pretense to being scientific, cognitive history unwittingly serves to reproduce the biases of the "prescientific" internal history of science. This atavistic feature of humanism is revealed in the strategic use that historians make of *historicism* as a methodological doctrine. One can take historicism to be a family of positions that involve the claim that the epistemic differences between times and places are more important than their similarities for understanding why people think and act as they do. Such differences may matter for various reasons depending on the version of historicism that is endorsed. On the one hand, Auguste Comte and G.W.F. Hegel are historicists who take the radical epistemic differences between times and places to constitute a directed sequence of changes. On the other hand, Wilhelm Dilthey and Karl Popper are historicists who take these differences to preclude the possibility of any such sequence. (Ironically, after flagging this distinction in historicisms in 1957, Popper went on in the next 15 years to become the sort of teleological historicist he originally condemned; Fuller 1988a: Chap. 2.) Nevertheless, historicism has been the most fruitful philosophical strategy for getting skepticism's critical edge without suffering skepticism's self-debilitating consequences.

However, historians, by applying historicism *asymmetrically* to the past and the present, often undercut their own critical advantage. The historicist is supposed to demystify the tendency of today's philosophers and scientists to stress superficial continuities with the past that serve to suppress deep differences. Once revealed, these differences generally show just how little we contemporaries understand about our own historical situatedness. Yet the historicist is also supposed to recover the self-understandings of past figures. Equipped with a keen sense of their historical situatedness, these figures can seemingly transform the available traditions in arch ways by investing even the most ordinary of objects with scads of "cultural meaning."

Much in the spirit of Renaissance Humanism, this asymmetrical application of historicism makes the people of the past our cognitive superiors. These are people the historian strives to understand largely because they understood themselves better than we understand ourselves. Of course the *prisca sapientia* that today's historian valorizes in the "ancients" is not quite the same as was valorized in the 16th century. Some historians of political theory, for instance, seem to think that John Locke and his predecessors had a better grip on human nature than any of his successors. Still most historians of science defer to the great scientists on the more modest grounds that they had a culturally (or at least cognitively) integrated understanding of their inquiry, next to which today's scientists seem either alienated or simply shallow. One wonders: How could we have fallen from such an Age of Heroes?!

To their credit, thoughtful humanists have been dissatisfied with the temporal asymmetry exhibited in this application of historicism. Unfortunately, in moving toward a more symmetrical historicism, many of these "postmodern" humanists and semioticians have been led to overcharitably read the thoughts and actions of members of our own culture—as if the solution lay in elevating the present to the mythic levels of the past. Thus, an ideologically diverse group of inquirers, ranging from radical social constructivists like Karin Knorr-Cetina (1981) to reactionary followers of Michael Polanyi (1957), hold that unspeakable amounts of expertise are built into routine laboratory practices. The entire scientific workplace appears an enchanted realm of deep meanings. If the reader, however, winces at the implausibility of a world superabundant with competence, she should save some of her cringing for analogous claims that are normally accepted without notice when the lab in question is that of Faraday or some other ex post facto notable.

One might achieve the desired temporal symmetry by a social scientifically informed cognitive egalitarianism. This strategy brings people from the past down to the realistic level of shortsightedness that both historicists and experimentalists have been so good at detecting in people from the present. In addition, this strategy drives home the point that knowledge is *necessarily* a social accomplishment that cannot be completely understood by adopting the perspective of any one of society's members—hence the need for a *social* epistemology (Fuller 1988a). Without attempting to evaluate their specific claims about the history of science, several models for this sort of historiography exist. Structural Marxism and Freudian psychohistory are perhaps the most explicit in their symmetrical historicism, largely because false self-understandings, as either ideologies or ego mechanisms, are granted powerful roles in explaining what historical figures do. However, historiographies that postulate the inability of agents to predict what other agents will make of their work will do. In this regard, diffusionist accounts in the history of *technology*—in which an artifact takes root in ways unanticipated by the original inventor—would stand as good models for the history of science (cf. Basalla 1988).

But why do even cognitive historians of science resist a social scientifically informed historicism? Part of the reason must lie in many historians continuing to imagine that the configuration of academic disciplines has not changed since the late 19th century—just before the emergence of the social sciences. As a result, they regard history as the final authority on human affairs. In this respect, the debate between philosophers and sociologists over the ontology presupposed by the history of science counteracts the historian's inertial tendency to think that all they need are good archives, common sense, and some opportunistic reading in other disciplines.

HISTORICISM'S VERSION OF THE COLD WAR: THE PROBLEM OF ACCESS

There is a deeper disciplinary motivation for the historian to be fearful of the sort of essences that philosophers and sociologists argue about. The historical method demands an exhaustive study of the documents of the time and place about which one is writing. Very often more time and energy are devoted to this task than to preparing an analysis of the materials for publication. Under these work conditions, the most psychologically satisfying thing for the historian to believe is that the causal significance of the things discussed in these documents is proportional to the amount of time and energy spent in wading through them. After spending several years in an archive, a historian would be hard pressed to admit that her conclusions about the workings of some episode are similar to someone who never visited the archive and, in fact, conceived of an alternative account by bouncing off some secondary sources.

The challenge posed by essentialism is precisely that the quality of evidence is not proportional to its quantity. Consequently, much of the actual historical record may be incidental to what has really mattered in the course of history. Presumably, then, someone with a higher sense of cognitive efficiency than the historian—a Comte or Hegel perhaps—could penetrate the surfeit of texts to glean the defining patterns of history. Call this challenge—that, after all, something like a faculty of intellectual intuition might exist—the *Platonic Plague.* The Platonic Plague is the historian's biggest epistemological nightmare. It is realized on both sides of the Laudan–Bloor debate. And it is realized in the experimental method of science; that is, in the possibility that an abstract system of interacting variables can model the complexities of the phenomenal world. Drawing some explicit connections between "historicism ontologized" and the experimental method, one can also outline the best way for the historian to counteract the Platonic Plague.

This counterargument starts with the classical nominalist account of our knowledge of universals. The nominalist account still has some psychlogical validity—namely, what Platonists and others have wanted to cast as our ability to intuit universals is really our *in*ability to remember the manifold differences among particulars. However, armed with the canons of the inductive method, even nominalists have believed that this adversity can be turned into a virtue. Thus, we learn to focus our forgetfulness by retaining only those differences that we think might be necessary for bringing about interesting results. Experimental controls provide one environment that enables this mental discipline to work. Now notice the highly pragmatic character of all this talk—as if the pursuit of knowledge were only a matter of carving out an epistemic niche from within the welter of unmanageable data. Such a pursuit might involve systematically ignoring and compensating for data that our minds are incapable of handling. As a result, we could well be left with a seriously skewed picture of the nature of reality, one that would perhaps never be penetrated *unless we explicitly set out to do so.* Herein lies the epistemic importance of the historian's practice not to leave any page unturned in the archives—as an antidote to the modes of convenient and pragmatic thinking that the search for universals invites. However, to fully realize this role, the historian must embrace the symmetrical historicism advanced earlier so as not to succumb to the Platonic Plague. In other words, she must be open to the possibility that what is least suspected (or recalled) turns out to be most significant.

The upshot of the prior argument is to recommend that historians exchange their familiar posture as keepers and dispensers of practical wisdom for the more alienating one as archaeologists of knowledge (cf. Fuller 1988a: chap. 6). In this game of epistemic bluffsmanship, the savvy historian can now issue her own counterthreat to the Platonic Plague. This counterthreat would come in handy in dealing with philosophical and social scientific attempts to ontologize historicism, as in the case of the Laudan–Bloor debates. Dubbed the *Idiographic Incentive*, this counterthreat effectively answers the question: Why should we bother sifting through all the data of history if what really interests us is discerning long-term trends and other such essential notions?

The answer is based on the now classic experimental findings of Tversky and Kahneman (1974), which showed that people tend to take the *availability* of a memory as a sign of its statistical *representativeness*. In other words, the easier one can recall an item, the more likely one will take the recollection to be typical of the class of items to which it belongs (the relevant class here being dictated by what the experimenter asks the subjects to recall). By calling this tendency the *availability heuristic*, Tversky and Kahneman underscore the fact that it works enough of the time so as to discourage people from investigating the many other times in which it fails to work. The heuristic has also been aided by the captivity of common sense to Aristotle's wax tablet view of the mind. On this view, more frequent encounters with an object leave a more lasting mental impression. However, given the ease with which the structure of human memory can be altered by seemingly incidental factors, there are no good psychological grounds for thinking that the statistical and mnemonic qualities of things are so directly correlated.

A version of the availability heuristic is also at work when accessing historical evidence. For various reasons, some planned and others not, it is easier to get at certain kinds of evidence than other kinds. For instance, getting access to a scientist's journal articles is usually easier than her private notebooks. Although if (as arguably happened in the case of Darwin) the notebooks are widely publicized and celebrated as literary works in their own right, they may become more readily available than the works originally designed for public consumption. Among the diverse factors that affect one's cognitive access to historical evidence are the availability of translations in one's own language, the substitutability of original sources by glosses, as well as the historical figure's sensitivity to the chance that future generations might want to eavesdrop on her conversation (cf. Fuller 1988a: Chap. 12). If Tversky and Kahneman are right, the tendency should be to think that most of the story is told by the evidence that is readily available. Less available evidence would not appreciably alter the story (although it would undoubtedly fill in the details). No doubt such a tendency may be found among such ontologized historicists as Bloor and Laudan. After all, don't we already know enough to decide whether (or under what circumstances) scientific theories are selected on the basis of "internal" or "external" criteria?

By contrast, part of historians' professional training is to unlearn the availability heuristic. In so doing, historians would take seriously the possibility that the next bit of uncovered evidence may radically reconfigure all the previous evidence—hence the incentive to pursue the idiographic method. In this way, historians inhibit the economizing tendency of the heuristic, which presumes that a principle of diminishing marginal utility exists for the epistemic value of evidence. Now, clearly, when speaking of the "professional training" of historians, I am idealizing somewhat. Historians are likely unaware of the relevant biases in human psychology that their inquiries are useful in counteracting. Accordingly, they do not counteract their own biases as often as they might if they were made to see the psychological significance of their practices. Indeed an important research project could be undertaken to drive home this point in the history of science (or of anything else): To wit, a *Critical History of Access*. One could trace how the documents on which historians most heavily rely (including translations and secondary sources) came to be made so readily available. Moreover, one could establish the effects that this ready availability has had both on the facts that figure prominently in the histories written and on the historians' search for other documents. Such a history would reveal the biggest fallacy plaguing humanistic thinking—namely, *the unwarranted inference from the disposition of the evidence to causal dispositions*. This fallacy appears in many guises, the subtlety of which is a tribute to their commonplaceness. Here are just three.

1. *The amount of evidence available is often taken to be a measure of the causal significance of the thing evidenced*. For example, if most of Newton's manuscripts pertain to theological matters, then the humanist is prone to conclude that theology was the driving force in Newton's work. However, some causes may be documented well out of proportion to their significance either because of the literary conventions of the time or because of the survival patterns of the documents over time. Nevertheless, the humanist is motivated to commit this fallacy to avoid admitting wasted effort in poring over the archives. It shows a failure to pass one of the classic tests of the distinction between science and superstition. Although the scientist sees no *a priori* reason why the mere presence of a piece of evidence will have significance and readily concedes that most of the data gathered will be trivial or misleading, the humanist cannot quite face this possibility. Consequently, the humanist more likely invests her findings with spurious significance.

2. *The self-referential features of the evidence are often taken to indicate the type of causal role played by the thing evidenced*. Thus, the fields of history in which most of the evidence is personally signed—for example, political (i.e. treaties) and intellectual (i.e. treatises) history—are said to be about a succession of personalities. Yet the fields in which most of the evidence is left unsigned—namely, economic and social history—are said to be about impersonal forces. Of course a hospital's accounts and health records are done by particular people, and the pronouncements of a politician or an intellectual are subject to linguistic constraints beyond her control and awareness. But these truisms are easily forgotten when the humanist insists on being so evidence-driven.

3. *Historical events often become exclusively associated with the canonical locations for finding evidence about them.* This outcome shows that the deceptiveness of historical access is, in a sense, a by-product of the scarcity of the material world: To wit, the present and future are recycled versions of the past. Yesterday's events are reconstituted and preserved as tomorrow's archives. This scarcity may be seen as a form of the Platonic Plague: Every particular is typecast for posterity as one of the universals that participated in its production. Put less metaphysically, while any event is clearly part of the intellectual, economic, political, and other currents of its time, after the event has transpired, traces of these currents are distributed to various archives, only one of which becomes typically linked to the event. Consequently, the historian does not really make a "free choice" in her selection of facts for interpreting the event because she will be immediately drawn to the stereotypical archive. For example, if the historian is interested in the proceedings of an academic conference, she will probably be drawn to an academic library and completely neglect the receipts that were taken in funding the conference, since that evidence is probably located—if at all—in some obscure place like a university's business office. The difficulty in obtaining this alternative source of evidence is then unwittingly taken by the historian to indicate its diminished relevance for understanding the event. In short, if you store the intellectual and economic records of an event in separate locations, then the two locations will soon be taken to symbolize separate causal "factors" that combined to bring about the event.

The depth and subtlety of the prior fallacies ensure that it will not be easy for the humanist to adopt the mindset that is appropriate for doing a Critical History of Access. However, the experimental psychology literature cited earlier is not the only source of refuge here. Feminist historians routinely incorporate a Critical History of Access in whatever they write about because of the long-standing systematic efforts to alienate this half of the world's population from recorded knowledge. Moreover, the form of oppression involved here is distinctive: Women usually have been excluded without any of the oppressor's rhetoric of evil and mystery that typically accompanies, say, racial discrimination. Rather, women have simply been passed over in silence as men render women's lives and works part of the taken-for-granted background conditions of everyday life. Thus, students of women's knowledge are professionally alert to potential discrepancies between the amount and the significance of evidence. In this way, feminists also tend to be especially sensitive to the major ontological and epistemological issues surrounding incommensurability, which more traditional historians might be inclined to underestimate. The ontological issue surrounding incommensurability is how conceptual differences arise from communication breakdowns; the epistemological issue is when a failure to communicate implies a conceptual agreement or disagreement. If the former captures the process by which autonomous bodies of knowledge emerge, the latter captures the process by which these bodies are related to one's own. These may be regarded as the founding principles of social epistemology (Fuller 1988a).

Feminism has anticipated social epistemology's two-pronged probe of incommensurability. On the epistemological side, feminists have counteracted the bias imparted by Leopold von Ranke's historiographical maxim of recalling the past "as it actually happened." As the cornerstone of professional history, this maxim encouraged a document-driven inquiry, in which causal significance was assigned on the basis of the size of the paper trail one left, where "one's" identity was determined by the signatures left on the particular pieces of paper. It was thought, quite in line with Ranke's empiricist-inductivist sentiments, that any truly important event would be recorded. Not surprisingly, on this view, a major historical event transpired whenever a few heads of state met in the same room long enough to sign a prominently placed piece of paper. Once historians moved away from the national archives to less obvious repositories for documents, other sorts of people—merchants, priests, scientists—started getting their due. However, causal significance was still measured by the ability to leave permanent traces, usually written ones, to which the historian, at least in principle, could gain access. Given that women both did not write and were often not written about, any comprehensive history would have to transcend the historian's standard interpretive techniques—indeed perhaps to the point of entertaining the possibility that those techniques are complicit with the male-dominated culture that the historian studies (Scott 1987; Nielsen 1990).

I have labeled this potential for radical critique in our understanding of the past the *inscrutability of silence* (Fuller 1988a: Chap. 6). But once the critique has been made and women's voices are heard, will they sound much different from men's? This is the ontological side of incommensurability: Is there anything *more* to conceptual difference than communication breakdown? If not, then maybe the articulated voices of women should sound like those of men. This is certainly the hope of Enlightenment liberals like Jürgen Habermas who equate increasing the sphere of freedom with enabling the disenfranchised to air their views in the open forum. In that case, feminist appeals to a specifically nondiscursive "intuitive" orientation to the world may simply be an artifact of the traditional prohibitions on women's speech.

But this is not the only possibility. For even if communication breakdown is all that is involved in alienating women from the public sphere, it does not follow that emancipation will come when women are brought into the open. After all men are equally alienated from women, which explains the peculiar form that masculinist domination has taken—in particular, a tendency toward radicalizing the difference between a self-contained active self and a passive nature that can be understood only in terms of its responsiveness to the self. Plato, Aristotle, Bacon, and Kant cloak this form of domination in rather different metaphysical trappings, but the metaphors that seep through their abstractions strongly suggest that the male–female relation is the analogue through which the self–world or subject–object relation is understood (Keller 1985). This is not the place to proffer new metaphors; my only point here is that, unlike the usual explanations of conceptual difference in terms of a creative leap or a normative infraction, the communication breakdown account supported by social epistemology implies a mutual loss and a mutual gain that squares with feminist ontological sensibilities.

UNDER- AND OVERDETERMINING HISTORY

Historians do not simply provide a foil for the Platonizing tendencies of philosophers and sociologists by transcending considerations of material scarcity and cognitive limitations in the course of inquiry. The issue is more complicated. The Platonic Plague and the Idiographic Incentive are ultimately alternative viewpoints about the amount of *evidence* needed before making historical inferences. However, this debate is often made to stand in place of an important subterranean debate about historical *causation* which is often fought between rationalist philosophers of science and constructivist sociologists of science. Whereas rationalists tend to presuppose that historical events—at least the exemplary ones that interest them—are causally *overdetermined*, constructivists presume that they are causally *underdetermined*. The distinction simulates the two sides of the metaphysical debate on how tightly the world is held together. The overdeterminationist simulates *determinism*, whereas the underdeterminationist simulates *voluntarism*. Although the latter often advertises itself as more "empirical" than the former, we shall see that they both appeal to what can only be regarded as occult notions of causation. Indeed both historiographical stances are primarily *normative* positions, evincing certain attitudes that philosophers and sociologists have toward history.

An *overdeterminationist* view of history postulates that there is only one world order, which consists of certain nodal events through which all possible histories would have had to have passed, although not necessarily as a result of all the other events that actually fed into these nodes. The "nodes" in question are, for a rationalist philosopher of science like Imre Lakatos (1979), the sequence of correct theory choices in the history of science: They had to have happened in a certain rational order, although not necessarily in the length of time it actually took. In particular, the sequences could have transpired more efficiently. The computerized discovery programs of Herbert Simon and his followers, discussed in the previous chapter, also follow this line of thought. Overdeterminationism is also consistent with the idea that history could have transpired *less* efficiently, yet nevertheless transpired in the requisite order. The Anglo-American philosopher Stephen Downes proposed a striking hypothesis that raises this possibility.

Contra the logical positivists and Quine, Downes argues that formal logic was not necessary for the development of science, in that, had logic never been formalized, the same sequence of theories would still have been chosen in the history of science, although perhaps the formulation of these theories would have been somewhat inelegant. Downes' thesis can be subjected to the test of counterfactual history.

Going back to the latest period prior to the formalization of logic (clearly one needs to specify whether Aristotle, the Stoics, the Scholastics, Leibniz, Boole, or Frege is meant here), imagine that formalization had not occurred, and then see whether the crucial events in the history of science would have still taken place, assuming that the absence of formal logic had the *smallest* possible collateral impact in the course of history. The idea would be to presume that history is generally overdetermined so that other factors could have brought about events close to the actual history by compensating for the factor removed *ex hypothesi*. Such a presumption makes sense if we further suppose that every event can be identified primarily in terms of the *function* it served in bringing about some other event. Hence, the overdeterminationist would search for a "functionally equivalent" combination of events (Elster 1984: Chap. 1; McCloskey 1987: Chap. 4, on the use of this principle in econometric history).

I cannot pretend that an adequate test has been made of Downes' thesis, but a couple of points are worth mentioning. First, the truth of the counterfactual thesis hangs on the relevance of formal logic to the conduct of inquiry remaining obscure, not that logic would fail to be formalized. (For example, formalization may be introduced as a pedagogical technique, much as how Descartes and Hobbes regarded experimentation.) Second, if it turns out that Downes is right and the history of science is overdetermined with respect to formal logic, then that would be a good empirical argument for denying that science has an *a priori* component.

In contrast, historical *underdeterminationism* says that a given event need not have occurred but once it did occur everything that followed did so by necessity. Thus, instead of the Lakatosian account of inevitable progress, the constructivist sociologist paints a picture of the history of science governed by "turning points," such as Robert Boyle's successful exclusion of Thomas Hobbes from membership in the Royal Society—a triumph of the new experimentalism over the old scholastic rationalism, according to Shapin and Schaffer (1985). Indeed it would be hard to cast constructivist accounts as involving "free choice" by the historical agents if the agents' perceived options did not turn out to have significantly different consequences over the course of time. If experimentalism would have become the most esteemed form of knowledge even with Hobbes' admission to the Royal Society, then his actual exclusion could hardly have been a "turning point" (Lynch 1989). In that case, we would have an overdetermined history of experiment's epistemic ascendancy: several alternative trajectories with the same endpoint. Economists call this "equifinality."

Thus, in matters of causation, the sociologists economize just as much as the philosophers—especially if, as Shapin and Schaffer imply, the experimental method of today is little more than the latter-day reenactment of a decision that was originally made in the 17th century. Shapin and Schaffer seem to think that the 17th century continues to "act at a distance" on 20th-century science, as if nothing occurred in the intervening 300 years to sublimate the resolution of the Boyle–Hobbes debate. It would seem, then, that the Royal Society was able to construct its reality only by constraining our reality. In history, as in physics, underdeterminationism presupposes an occult sense of causation. To be sure, it is a different sense of the occult from the overdeterminationist account, which presumes that the robustness of the sequence of theory choices in the history of science, under a variety of counterfactual conditions, implies that there is some hidden logic, or "method," that orders the choices, which in turn suggests a quasi-deductive structure to history.

Perhaps both the promise and the peril of underdeterminationist historiography is best captured in Immanuel Wallerstein's (1991) "world-system" approach. It stipulates that some fairly local changes in the organization of agriculture in medieval Europe triggered a series of dispersed effects that have since stabilized as the capitalist world system. The point, then, would be to locate the next chaotic episode—the functional equivalent of a revolution—that will destabilize the existing world system and ultimately reconfigure a new one.

The occult causal sensibilities of the two historiographies betray that their real interests lie elsewhere. In the case of overdeterminationism, the implicit normative agenda, "Whiggism," is fairly evident: The sequence of theory choices in the history of science is no fluke, but destined to triumph despite the variety of ways it may be locally resisted or even temporarily delayed. However, the normative agenda of underdeterminationism is considerably subtler, yet still present. Shapin and Schaffer, for example, express disappointment that Hobbes did not persuade more natural philosophers because, had he succeeded, the scientific community today would probably be conducting its activities in a much more dialectically responsive environment. Two features of this attitude are noteworthy.

First, underdeterminationists typically ground their sense of historical regret in exactly the same set of "internal" norms of science—valid reasoning, true premises, and the like—that the overdeterminationist espouses. The difference between the two historiographies turns on whether the "good guys" really won. Whereas Lakatos would say that the history of science has borne out the correctness of Boyle's position, Shapin and Schaffer strongly suggest that, once properly understood in its original context, Hobbes had "better arguments" than Boyle, but Boyle had greater political and rhetorical savvy.

Second, historians who confer great significance on relatively rare turning points tend to underestimate the causal efficacy of their own sense of goodness. They are likely to veer between a feeling of relief (when the good guys win) and regret (when they lose). Correspondingly, Whig historians overestimate the efficacy of people who share their values, and hence straddle hope (when the good guys are losing) and triumph (when they win). Thus, in terms of their normative sensibilities, an interesting analogy between the histories of science and politics may be drawn, with overdeterminationism corresponding with political utopianism and underdeterminationism with political realism. On this basis, I have increasingly contrasted the overdeterminationist's Whiggishness with the underdeterminationist's latent *Tory* historiographic sensibilities (Fuller 2000b: Introduction, Fuller 2002b).

WHEN IN DOUBT, EXPERIMENT

Our paradigmatic practicing historian has been engaged in a battle of wits with other historicists over the proper use of historical evidence. However, the Critical History of Access conjures up a spectre that could make all this thrust-and-parry beside the point. The spectre is inspired by the Cartesian Demon of classical epistemology, but with a more restricted scope and, hence, with a more realistic chance of being true. Call it the *Diltheyan Demon*. Instead of a superhuman intelligence with the ability to create a world with all the evidential cues needed to cause humans to hold a seamless web of false beliefs, imagine a quite human intelligence with the ability to plant all the evidential cues needed to cause future historians to believe exactly what she would have them believe regardless of its correspondence to what really happened. Of course, historians are alive to the efforts that people have made to perpetuate a certain image of themselves and their accomplishments. But often historians unwittingly contribute to this perpetuation by focusing on people who have already succeeded in self-perpetuation by making their work indispensable for our own.

For example, no matter how many books are written revealing Galileo's counterfeit experiments and philosophical bluffsmanship, Galileo's demonic wiles worked long enough on the generations immediately following him to make it very difficult now to write a history of science that gives him a diminished role. It no doubt could be done, but it would require a fundamental reassessment of the significance attached to the most readily available evidence as well as a concerted search for evidence that has not already been focused through Galilean lenses. To what end? Here I do not mean to understate the importance or even the ultimate feasibility of disentangling "what really happened" from "what they would like us to think happened." However, historians are just as susceptible to mystified conceptions of their own history than the rest of us. The role of "case studies" in historical scholarship—especially the historiography of science—is a good case in point.

There is an increasing tendency to run the history of the case study approach through the idiographic tradition in the human sciences—namely, hermeneutics, ethnography, and other methods that let the specificity of the case dictate the methods appropriate to its understanding. Unfortunately, the idiographic tradition is rather alien to the sort of people who originated the history of *science*—namely, philosophers and physicists. Even George Sarton, the person most responsible for institutionalizing the history of science as a field by founding the journal *Isis*, advocated Henri Berr's positivistically inspired "synthetic" history (Stern 1956: 250-55). Yet these proponents of principles, norms, and laws were not insensitive to cases either. Buxton and Turner (1992) unearthed this alternative tradition of case studies by examining its diffusion through the faculties of Harvard University from the late 19th to the mid-20th century.

At the near end of this alternative tradition is Harvard's president James Bryant Conant (1950). Conant pioneered the case study approach to teaching science by recapitulating the design and interpretation of historically important experiments, examining how each major scientist continued and departed from his predecessors. From this approach came Gerald Holton's (1952) famous physics text, which introduced the central concepts of the field in rough historical sequence. Yet the ultimate result was Thomas Kuhn's (1970) portrayal of science as an activity in which innovators must struggle with how much of the textbook tradition (or "paradigm") needs to be carried over in solving an outstanding problem. But Conant and his associates in science education were only latter-day converts to an approach that had flourished in the Harvard Law School for over a half century. The use of case studies to teach law is quite familiar, but its point is radically different from what one finds in today's idiographic appeal to cases. The law professor wants to display how an exemplary judge tailored a general principle or precedent to fit the case at hand, thereby revealing something of how the legal mind ideally works. This practice presupposes that there are indeed general principles of legal reasoning, but that one needs to survey a wide body of cases to discern their overall pattern. In this way, the law student gets a sense of the limits on the applicability of principles to cases and the degree of flexibility one has in interpreting previous cases and statutes. Thus, the cases are interesting only as illustrative devices, not in themselves.

Conant (1970: 438-40) endorsed endorsed Wallace Donham's adaptation of the case study method to the Harvard Business School by having students examine the consequences of introducing innovative management techniques into a variety of workplaces. Here we have a sense not only of generalizable norms, as in legal case studies, but also of norms being prescribed so as to increase the likelihood of some preferred outcome. Whereas in the law any decision by an authorized judge is ipso facto a potential exemplar of legal reasoning, the soundness of management thinking is ultimately borne out in the marketplace, where slight differences in technique can make substantial differences in productivity and sales. Thus, case studies of business successes and failures are of equal pedagogical importance. The moral of Donham's adaptation of the case study method—which Conant carried over to scientific cases—was that the more one is inclined to alter the normative structure of a situation, the more the validity of the new norm should be judged in terms of the consequences of that intervention.

Following from Donham's example, then, the problem with traditional philosophy of science is that its treatment of historical cases straddles between the law school way and the business school way of using case studies. On the one hand, like the law professors, philosophers want to confine their attention only to exemplary episodes in the history of science—whether they believe such episodes to be rare or frequent. On the other hand, like the business professors, they want to take seriously the possibility that even the best science could have been done better. The tension between these two tendencies issues from the philosopher's desire to meddle in the conduct of inquiry with impunity. Indeed "meddling with impunity" may neatly capture the legal-economic conditions under which the Platonic Plague can take place. A philosopher can seem to be extracting, rather than merely imposing, the normative structure of some situation if she can intervene in that situation without leaving a trace of her presence (i.e. the scientists can't fight back). This invisible philosophical hand produces an inversion of appearance and reality, the kind of which led Nietzsche to demystify the hidden power structure of abstract ethical systems, the model for later deconstructionist projects (Culler 1982: Chap. 1).

Let us consider this general conclusion in light of the histories of science that Laudan and his philosophical followers have written. In one sense, they start from the scientist's standpoint. As they keep one eye on the future, scientists tend to say in their official writings what they think they should have done to get the right result. These statements not only tend to look like philosophical dicta, but they also make for better method than the mess the scientists actually made in the lab, which more charitable historians might dignify as exhibiting "implicit norms." This perspective suggests that Cartesian solitude—away from the maddening lab—is needed to think through a scientific conclusion from first principles. Moreover, the Cartesian image of the scientist carries certain strategic advantages for the normative philosophical project.

First, it makes a virtue out of the social constructivist charge that the scientific research report is a rationalization detached from the original scene of activity. Second, it nimbly avoids the need to judge the reliability of, say, Faraday's memory when he writes of what transpired in his lab: If the events reported *would* have led to an epistemically desirable outcome, then that is good enough for the Laudanian historian. Third, by not relying on the actual practice of scientists, the Laudanian historian also avoids the "cult of science" mentality associated with followers of Michael Polanyi, who would make the scientists the final authorities on how science should be done. What stands out from these strategic advantages is its tacit concession that it is more important that a methodology *would* have worked, had it been used, than that it was used frequently or perhaps even ever. In other words, the historical character of Laudan's project is really quite incidental to the normative conclusions he wants to draw. In that case, a better way to get the sort of evidence he needs may be *psychological experiments* (cf. Fuller 1992b).

Historians are not the only ones who retard the development of HPS by sticking to humanistic approaches. Philosophers indulge in their fair share of methodological backwardness when they openly embrace "rational reconstructive" approaches to the conceptual (Carnap, Reichenbach) and historical (Lakatos, Laudan) aspects of science, but then avoid functionally equivalent empirical approaches: respectively, ethnosemantic surveys of scientific discourse and controlled experiments on the efficacy of various methodological norms. Elsewhere I have defended the importance of the social history of language to a systematic understanding of knowledge production (cf. Fuller 1988a: pt. II). Now I limit my discussion to the role of experiments.

Consider the case of falsification as a methodological norm of science. Experiments show that subjects taught to falsify hypotheses are better problem solvers. This should enable clearer thinking on the counterfactual questions that typically concern rational reconstructionists, such as whether (or by how much) the introduction of a falsificationist strategy would have hastened some major scientific discovery (Gorman and Carlson 1989). In designing experiments to test the efficacy of falsificationism, psychologists are forced to come to grips with issues that philosophers manage to sidestep because of the level of abstraction at which they normally pitch their claims. These are some of the issues: What is the measure of methodological efficacy, and how is it to be operationalized (e.g. how does one count "solved problems")? Is the norm meant to govern each individual's practice, group practice, or some other unit of analysis? Is the norm meant to be representative of ordinary scientific practice, or is such a concern (i.e. for "ecological validity") beside the point because the norm is meant to improve, not merely reproduce, ordinary scientific practice? In this light, rational reconstruction is best seen as a first pass at a proper experimental design, one in which relevant variables are isolated for further refinement and testing in controlled settings.

A significant advantage of philosophers' having recourse to experiments in testing their normative claims is that they are forced to clarify the object of their enterprise. To say, as many realists and positivists often do, that we need to explain the "remarkable success" of science is to be tantalizingly vague about the terms in which we are to judge this alleged success. Attempts to historically specify such a standard quickly face resistance from the evidence. In the fullness of time, all scientific theories are eventually shown to be false. Arguably, the longer entrenched theories are the ones that turn out to have had the deepest flaws (which, in part, explains why the replacement of these theories is so long in coming). Whatever "improvement" can be discerned in a sequence of theories is usually because it is assumed that they are all trying to solve roughly the same set of problems. However, as Kuhn and his successors have emphasized, that set is subject to change often because the problems simply lose their urgency, which makes any overarching sense of progress elusive. Even the technocratic criterion of success in terms of enhanced prediction and control will not sustain scrutiny as a metric for the success of *science* because any technique can be explained by a variety of incompatible scientific theories, which makes it impossible to credit any of those theories with the technique's success.

Nevertheless, those who question the generalizability of experimental results will wonder whether having groups solve problems in artificial settings will bring us any closer to determining the sense in which science is "successful." My response is that, at the very least, reflexively speaking, the deliberations required for designing an experiment will enable philosophers to become more self-conscious about the sorts of situations and effects they are prone to term successful.

Given both the pro-science stance of philosophers who back an internal history of science and their acceptance of the experimental method as definitive of science, it is ironic that they have been among the most vocal opponents to experimental approaches to the study of science (e.g. Shapere 1987; Brown 1989). Perhaps even more ironic is that their grounds for objection are essentially the ones that Aristotelians raised against the legitimacy of generalizing the experimental method throughout the natural sciences—the very objections that had to be overcome before the Scientific Revolution could take off (Harré and Secord 1979). These objections are raised and answered in detail elsewhere (Houts and Gholson 1989; Fuller 1989: Chaps. 2-3). For purposes of the argument here, I merely stress that the need for experiment arises naturally from the sorts of questions that philosophers (and their sociological interlocutors) tend to pose, which involve the extent to which a given factor (usually a method) contributes to a generalizable outcome, which may (or may not) be conceptualized as an "essence." I have argued that history as normally practiced is ill suited to dealing with these questions; hence they never are resolved.

Critics of experimental approaches, although ostensibly sophisticated in their attitudes toward science, seem to have a stereotyped view of the possibilities for experimental design, one modeled on, say, Galileo's (alleged) inclined plane experiments—this despite the fact that the biggest innovations in experimental design have come from psychology precisely because of the tricky nature of its subject matter— namely, human beings (e.g. Campbell and Stanley 1963; Campbell 1988: pts. I-III). The debate over the viability of experiments as a testing ground for the philosophy of science has been continued by Kruglanski (1991) and Tweney (1991), taking the pro- and anti-experiment stance, respectively.

However, there is a conceptually deeper worry lurking in the critiques of experimental approaches. It is the Scylla and Charybdis that awaits the internal history of science once the experimental study of science is granted legitimacy. In their search for generalities, philosophical defenders of internalism are methodologically compelled to turn from the anecdotal evidence of history to the nomothetic approach of experimental science. However, once they agree to experiment, these philosophers will probably find that if the methods of science are generalizable, they can also be characterized in fairly abstract terms–that is, without having to make reference to the content of particular sciences. (Of course such abstractness also becomes a practical necessity when the pool of experimental subjects is confined to undergraduate students.) It may even be that these methods can solve any of a wide variety of problems or facilitate any of a wide range of social actions. It should come as no surprise, then, that philosophers who have been attracted to the "problem-solving" model of science, such as Dewey, Popper, and Laudan, have also been quite liberal in what they will countenance as a science or "intellectual practice." But with such liberalism comes the threat that sociologists like Bloor may be right after all, in that there is really nothing *epistemically* distinctive about science: If certain methods seem to make science work better, that is only because they would make any social practice work better. Thus, science would be shown to have no essence of its own. Were philosophers to stick to internal history of science, they would never be under an obligation to compare the workings of science with that of some other institution, and thus not be tempted to rise to the level of abstraction at which the distinction between "internal" and "external" to science no longer makes a difference. Experimentation, by contrast, imposes just such an obligation.

STS AS THE POSTHISTORY OF HPS

If interdisciplinary fields rarely become disciplines in their own right, that is only because their central problems continue to be defined in terms of the old disciplines. Take the difference between HPS and STS. Whereas the HPS person tends to blur positions that emerged *after* the Strong Programme in the Sociology of Scientific Knowledge (and hence all the sociologists sound like David Bloor), the STS person tends to blur those that had existed *before* (and hence all the philosophers sound like Larry Laudan). In this respect, HPS belongs to the prehistory of STS.

As we have seen, HPS failed to make substantial progress because it became embroiled in disciplinary turf wars between philosophers and sociologists who argued (subject to historical arbitration) about the relative contribution made by "internal" and "external" factors to the growth of knowledge. By contrast, the central problems of STS are not principally defined along such disciplinary lines. Rather, there are signature problem areas on which inquirers with a variety of disciplinary backgrounds work, whose differences of opinion are no longer predictable simply on the basis of those backgrounds. These include the thick interpretation of experimental practice, the mapping of the circulation patterns of scientific artifacts and interests in society, and the deconstruction of artificial intelligence programs. Consequently, the Laudan–Bloor debates, pitting "*the* philosophical" against "*the* sociological" perspective, seem uninformative to current STS practitioners.

Even identifying STS with the triumph of "sociological" approaches can be misleading if the term is meant to suggest the discipline of sociology. For the training and work of most of the leading STS researchers bear slight resemblance to what is normally found in professional sociology journals. Rather, STS researchers are *sociologistic*, which is to say they tend to presume that an ontology of social entities is needed for explaining science. This commitment should be taken as analogous to the traditional scientific presumption of *materialism*, which is primarily a metaphysical position that is neutral with regard to the particular theory that has the best grasp on the nature of matter.

Despite STS's professional distrust of disciplinarity, it must be said that the *Realpolitik* of academic survival dictates that STS move toward becoming a discipline or die. This leaves open the question of which sort of discipline STS should become. In particular, should it acquire some of the trappings of the liberal professions and applied fields? We have already seen that the normative motivation of the philosophy of science can be fruitfully understood as straddling that of law and business. Indeed to prevent STS from losing its radical potential by becoming just another "normal science", perhaps it should be housed in a professional school—alongside the clergy, law, education, business, engineering, and medicine—rather than in the liberal arts division of universities. Although more traditional defenders of the university are loath to admit this, professional schools have been much more "liberal" in the types of relationships they have forged both inside and outside the academy than the so-called *liberal arts*. For starters, most professionals work in environments where their financial survival depends on enlisting the support of lay people, who usually rely on their expertise. Although I do not wish to endorse the cult of expertise, neither do I want to lose sight of the public-spiritedness that accrues to a field defined in terms of members whose practices are oriented more toward nonmembers than toward each other. As a matter of fact, the conference program of any annual meeting of the main STS professional association, the Society for Social Studies of Science (4S), reveals that STS researchers have already found their way into virtually every kind of knowledge-producing site.

Unfortunately, they have remained as nonobtrusive in their participant–observation status as possible, packaging their insights in ways that could make sense only to other STS practitioners.

To be sure, a conception of STS centered on social epistemology would require such institutional liberality so as to allow intervention in already existing knowledge practices. However, this intervention would not be in the form of second-order pronouncements worthy of a philosopher king. Rather, a more apt model is the participant observer approach of the ethnomethodologist—that is, an *a posteriori* Socrates (i.e. a Popperian), who is both open and strategic in her probes. In that case, the social epistemologist would be required to spend much of her time not as a studious scholar in a department of her own, but as a catalytic agent in someone else's department—or, better yet, *between* departments. Research would consist of recording and analyzing the results of these interventions. At professional meetings, social epistemologists would trade techniques that worked (or didn't) in various interdisciplinary (for cases of science vis-à-vis science), interagency (for cases of science vis-à-vis government), or interconstituency (for cases of science vis-à-vis the public) settings. Tenure would be granted to practitioners who succeeded in reorganizing research agendas and perspectives in profitable ways. Analogous criteria could be developed to evaluate the social epistemologist's attempt to disrupt the institutional inertia of science funding or folk attitudes toward the public impact of science.

Clearly, I envisage social epistemology as the successor subject to philosophy of science in the STS constellation. As it stands, philosophy of science exists only as what may be called a "vulgar sociological formation." In other words, the field exists only in the sense that there are journals claiming to publish work in that area. However, the work to be found within the covers of such journals, while predictable, does not have any obvious integral unity (Fuller 1989). Some philosophers are essentially doing internalist history of science, others are conceptual underlaborers for the special sciences, and still others are playing the endgame of ancient debates over confirmation and explanation (postpositivists read: rationality and realism). The Popperians were probably the last to attempt to forge an integral whole out of these ever more disparate parts, but little has happened in that vein since the Laudan–Bloor debates. The missing philosophical glue is an overarching normative perspective that addresses the ends of science in addition to its means: a perspective unafraid to suggest how science might change and improve. (Indeed, the best way nowadays for a philosopher to take up these issues is by participating in a funding panel of a research council.) Such were the origins of philosophy of science in the 19th century when the likes of Comte, Whewell, Mill, and Mach constructively intervened in the scientific process.

Yet equally needed is a rhetoric whereby philosophers can see their own interests addressed by social epistemology. For example: Will logicians be allowed to join the club or will they first need to ply an entirely different trade? Although it is now commonplace to say that the social sciences lack their own Newton, it would probably be more correct to say that there are too many pretenders to the Newtonian throne *and not enough Maxwells*—that is, too few outstanding talents who see enough of their own interests represented in someone else's project to devote their energies toward developing that project. Thus, social epistemology needs to attract a few good Maxwells. Perhaps the best way to think about this task is in terms of ways in which the social epistemologist can recontextualize what philosophers of science normally do. In other words, how does one significantly alter the *point* of philosophical activity without having to change its conduct very much? I end here with four possibilities.

1. Philosophers skilled in formal theories of rationality and logic can design machines whose handling of scientific tasks is easily confused with that of a competent human. The social epistemological point would be to see whether public reaction to computerizing the task brings the machines closer to being seen as scientists or whether computerization serves only to distance the task from the realm of the scientific.

2. Discussions of the ends of science would at once help break down any artificially maintained science–society distinction as well as the equally artificial distinction between epistemological and ethical concerns. Ironically, an "externalist" perspective on the nature of science—one that keeps "What is science *for*?" an open question—might serve to reunite increasingly disparate branches of philosophy.

3. Philosophical interest in the history of science need not be "for its own sake" or played out exclusively by the rules of the historians. On the one hand, philosophers can take a cue from Hempel and uncover the hidden social scientific assumptions (i.e. generalizations about human behavior) that historians of science take for granted. On the other hand, philosophers can take a cue from Ernst Mach's critique of absolute space and time in *The Science of Mechanics* and use the history of science to undermine philosophical legitimation of a currently dominant research program.

4. Philosophers trained in the conceptual foundations of a special science should not continue to work with members of that science, but rather be placed as catalytic agents in a different science to break down artificial disciplinary divisions between the two fields. Instead of *handmaidens*, philosophers would thus be *matchmakers* of the sciences!

THOUGHT QUESTIONS

☙ Fuller opens with a potted history of the "retreat" of history and philosophy of science from its more radical implications. What is the history of HPS as Fuller portrays it? How does Fuller distinguish the aims of Kuhn and the aims of HPS?

☙ What might be the rhetorical conditions by which scientific language is managed? How do social epistemologists establish the grounds on which rhetorical management might be based? What is in it for the scientists to allow for the management of their discourse? How might a social epistemologist convince a scientist or group of scientists that their discourse needs managing? Reflexively, in what ways might the rhetoric of social epistemologists need to be managed?

☙ What is the "consubstantial" quality of discourse? How do these tendencies lead to cognitive stasis? How might a social epistemologist diagnose and change linguistic practices achieve "epistemic progress"? Generally, what might count as "epistemic progress"?

☙ What kinds of resources are needed for language, as representation, to work? What kinds of resources are needed for language to work rhetorically? In what ways does scientific experiment affect, and in what ways is scientific experiment affected by, the representational and rhetorical functions of discourse?

☙ What is "internal" history of science? What is "external" history of science? What difficulties does the social epistemologist face in adjudicating claims made in internal or external histories? How does Fuller characterize the behavior of historians with respect to practitioners in other disciplines?

☙ How does the "fixation on genius" by historians provide psychological corroboration of historical claims? How does the emphasis on scientists' competence lead historians to privilege practitioners memories and accounts of their own work? Ultimately, what does Fuller see as the problem with the focus on individuals in the history of science?

☙ What is historicism? How does Fuller characterize individuals', whom historians choose to study, sense of their own historicity? What is the symmetry principle for history that Fuller espouses? Do you agree with Fuller's premises regarding historians working assumptions and conceptions of knowledge?

☙ What fallacies do historians commit drawing inferences from the "disposition of the evidence to causal dispositions"? As a result, how does Fuller suggest that historians should be trained? Do you agree?

☙ What are the ontological and epistemological consequences of communication breakdowns? What are the ontological and epistemological consequences of silence?

☙ What is the overdeterminist view of history? What is the normative agenda of overdeterminist history? What is the underdeterminist view of history? What is the normative view of underdeterminist history?

☙ What does it mean to "do history better"? What advantages or disadvantages are found in using historical case studies? What normative impulses seem to guide the actors being studied and the historians who are doing the studying?

PART III

OF POLICY AND POLITICS

7

Knowledge Policy: Where's the Playing Field?

As that archdeconstructionist Jacques Derrida might say, science policy is captive to the "metaphysics of presence." In other words, science policy is treated as something that occurs only when traces of intervention are left (e.g. added funding or regulation), but not when such traces are lacking (e.g. allowing science to continue as is). Yet policy is always being made even when nothing is changed (Bachrach and Baratz 1962). Refusing to steer the course of science policy is a very potent form of science policy. One reason why this axiom of policy science is rarely given its due in science policy is that both the public and its policymaking representatives regard science as something that proceeds in a relatively autonomous fashion. Science policy is, therefore, something that intrudes, for better or worse, on this ongoing enterprise. In much of my earlier work (Fuller 1989: esp. Chap. 1, Coda), directed at the *internal history of science*, I wanted to deconstruct a bad pun that had been masquerading as a sound argument, to wit: If the trajectory of scientific research is subject to *inertial motion*, then the trajectory of science policy should be subject to *institutional inertia*. Even if the antecedent of this conditional were true, which it is *not*, only an inductivist of the naivest sort (or, in political terms, a traditionalist of the most conservative cast) would accept its consequent.

My original deconstruction had two immediate targets that will surface again in this chapter. The first target is the tendency of scientists (often under the influence of philosophers of science) to calibrate desires to match expectations so as to appear to be able to get what they want. This strategy usually involves an *adaptive preference formation* (Elster 1983). In this instance, scientists end up defining anything outside their sphere of control, such as funding and research prioritization, as "external" to the scientific enterprise and, hence, a drag on the scientific spirit. This strategy serves no one in the long run. Scientists look like what Marxists have traditionally seen them as—namely, benighted slaves for whom "freedom" is little more than the awareness that their masters can exploit "only" their bodies, not their souls.

The second target is the more general tendency to neglect the material consequences of satisfying intellectual needs. A way of trenchantly making this point is to observe that the maintenance of "free inquiry" normally entails the ability to pursue false leads with impunity. This capability materially involves the freedom to waste resources, which, in an age of increasingly expensive science, means channeling more funds away from other public and private interests. Here, too, we see what Marxists would call alienation of the scientist from both herself and her fellows. After all, what joins scientists to other human beings is the space and time they take in the material world as expressed in the media of social relations. Moreover, an increasing portion of a scientist's energies is spent on activities that look more like the work of entrepreneurs and managers than that of “scientific professionals.” Nevertheless, the scientist continues to believe that she is really in her own element only during the vanishingly small period in which she works with test tubes and formulae.

In what follows, I use a locution of my own coinage, *knowledge policy*, where one would expect to find “science policy.” Part of the reason is to remind the reader that, even when the examples are taken from the natural sciences, the range of fields included for policy scrutiny include all the *Wissenschaften*. Ultimately, I argue that claims to funding and attention made by the natural sciences need to be evaluated alongside those by the social sciences and humanities in contemporary democracies. But, in addition, using the phrase “knowledge policy” sustains the point that once cognitive needs are taken in conjunction with their material realizations, the standard policy decisions associated with funding and accounting become *de facto* epistemological ones.

SCIENCE POLICY: THE VERY IDEA

The refusal of policymakers to steer science policy is nicely captured in one of the many tacit maxims codified by Harvey Averch (1985), former staff officer at the U.S. National Science Foundation. In contrast to other social programs, scientific research is held not to experience diminishing marginal returns on investment: *Any* research funded for *any* length of time will yield *some* benefit. This maxim could easily be regarded as a call to institutional inertia; the tendency to continue a policy, regardless of opportunity costs and rate of return, unless it has obviously negative effects that impinge on a politically sensitive constituency. As an instrument of knowledge policy, social epistemology is designed to address the sorts of issues that would otherwise be decided by institutional inertia.

At the outset, the social epistemologist needs to persuade policymakers that they do not already know enough about the production and distribution of knowledge to make intelligent decisions. This task, especially in the United States, is easier said than done. The bulk of funded research appears as line items on the budgets of agencies that are officially devoted to addressing the public's medical, environmental, energy, or defense needs. Such an occluded accounting procedure reinforces the idea that scientists are sufficiently self-regulating to be inserted comfortably into any politically sensitive environment. Moreover, this procedure impedes collecting the evidence needed to reveal the dysfunctional character of this distribution of scientific effort.

Not surprisingly, then, the American *science policy advisor* is defined as a conduit between science and the government. These two institutions are presumed to work reasonably well by themselves, but can do more for the public at large by extended periods of cooperation (Guston 2000). The actual job of the advisor is to communicate the range of public needs to the scientists and the state of scientific research to the politicians. Furthermore, the information required for this two-way exchange is presumed to be fairly accessible if one is an "insider" in the relevant scientific and political circles (D. K. Price 1965). Thus, science policy has been institutionalized to rely almost exclusively on scientists' folk understanding of how knowledge production works. These intuitions rely more on a few anecdotes than on systematic study, let alone sustained criticism or experimentation with a course of action that goes beyond a mere extrapolation of "current trends."

The institutional inertia currently gripping science policy reflects the policymaker's relative satisfaction with both our current knowledge of how science works and the policy ends toward which that knowledge is put. This coupling of factual and normative satisfaction is, in turn, indicative of what Daniel Bell (1973) characterized as our "knowledge society" (Stehr 1994). Presuming that the workings of science are substantially understood, the knowledge society takes the uses to which science ought to be put as dictated largely by the very nature of science. Thus, among the foremost items on the science policymaker's agenda is the conversion of the amorphous problems that emerge in the public sphere to ones that are tractable by scientific means. Whatever escapes the categories of science is then relegated to a residual irrationalism, pejoratively called (in Bell 1960) "ideology" and euphemistically called "politics."

Yet for all their interest in scientizing the public sphere, policymakers in the knowledge society still operate with what is properly seen as a "folk theory" of how science works. "Folk theory" means something like common sense: a set of beliefs that reinforces the "normal" or "natural" character of some phenomenon in the course of explaining it. Thus, strictly speaking, a folk theory is "ideological" in that ideas about how science works—well founded or not—are constitutive of science's identity (cf. Fuller 1988a: Chap. 2). Policymakers typically despair of identifying any principled (as opposed to "merely political") grounds for shifting science funding priorities because they believe that scientific research never exhibits diminishing marginal returns. As to be expected of a folk theory, this belief is subject to considerable anecdotal support. It comes mainly from cases in which a line of inquiry led to many long-term beneficial products that had little to do with the original conception of the inquiry. But there are no attempts to submit this belief to rigorous tests. The "naturalness" of the policymaker's understanding of science is traceable to a metaphysical presupposition of folk theories. That is, one does not explicitly court challenges to their beliefs because whatever errors they contain will be revealed in the normal course of events.

But even when suspecting their own folk wisdom, science policy practitioners take the sheer pursuit of science, regardless of its palpable consequences, to be an activity that morally elevates society. Of course so-called free, nonutilitarian inquiry has traditionally promised some major long-term cultural benefits. A short-term indicator that this promise is being met has been the spread of higher education to larger segments of the population. Even if most college students never directly contribute to the production of scientific knowledge, they are nevertheless exposed in the classroom to exemplary lives in action. In scientists, students meet people regarded as apt replacements for the religious and aesthetic icons of more superstitious and elitist times. (It is a short step from this political sensibility to one historically tied to the German university system, which uses science as a rallying point for cultural identity and national unity.) For this reason, the increasingly obvious disparity between the value that the academy and the public places on teaching has engendered a public relations crisis in higher education that is unprecedented even by America's traditionally skeptical lights. By severing teaching from research, academics are now in the process of undercutting the most persuasive case for free inquiry in a democracy.

Investing cultural significance in the pursuit of "pure research" or "basic science" says nothing about how many people, of which sort, should be doing what, where, or when. Indeed the arguments surrounding such pursuits make clear that the "freedom" of free inquiry lies largely in its alleged spontaneity or *unmanageability*. This sense of freedom has profoundly affected the American conception of scientific inquiry. Indeed the expression "scientific community" does not enter American English until the early 1960s, with the roughly simultaneous publication of works by Thomas Kuhn, Warren Hagstrom, and Don K. Price (Hollinger 1990). Before that point, little discussion occurred regarding the decision-making process by which science could function as a self-governing—let alone externally governed—enterprise. Even the need to establish internal accounting mechanisms had been obviated by the allegedly spontaneous fair-mindedness of scientists. Like Rousseau's "noble savage," each scientist freely follows wherever the path of inquiry leads, using up as many resources as she needs, but never so much as to deprive her colleagues of a similar luxury. Even the four principles that Robert Merton advanced in the 1940s under the rubric of "the normative structure of science" failed to specify any mechanisms for their institutionalization.

Nevertheless, imagining that flesh-and-blood "free inquirers" would behave like noble savages is difficult. Using capital expansion in the marketplace as our benchmark, progress is measured by the supersession of past products and processes. Still progress can be artificially accelerated by manufacturing goods with "planned obsolescence." This idea applies no less to transitory fields of inquiry whose sole purpose seems to be to maintain the visibility of researchers until (if ever) something intellectually more substantive comes along. In that case, fully realizing the ideal of free inquiry would produce a system modeled on the convenience foods industry, aptly called *Fast Science*, which would maximize waste by ever quickening cycles of resource use and disposal. (See De Mey 1982: Chap. 9. The economics of this phenomenon has been analyzed from Marxist [Agger 1989] and neoclassical [McDowell 1982] standpoints.)

Once researchers are rewarded for this mentality, one can easily see how they would start to loathe teaching. Teaching has always seemed attractive to *teachers* because they have regarded the stuff taught as worth *preserving*. Once preservation is no longer valued in the knowledge system, then teaching seems to offer little more than partial, transitory snapshots from the frontiers of research. Thus, if Fast Science continues uninterrupted, we should expect the continued devaluation of teaching.

The question of values gets to the heart of science policy's inertial character. Perhaps the two most important issues normally resolved by institutional inertia are the relative value of the research produced by academic disciplines and the means by which a discipline may produce more of value. Does molecular biology, for example, "pack more bang for the buck" than high-energy physics? What may be done to address whatever discrepancies exist? The vast disparity in the costs and benefits that disciplines have to offer would, one might think, be an area for systematic knowledge policy research. However, the contrary is often the case. The suggestion that we might be spending too much money on, say, high-energy physics is typically treated as exemplifying a "know-nothing" attitude toward science. Yet the underlying motivation for this suggestion may be a desire to apply science to science itself, specifically, to determine the best projects in which to invest given certain short- or long-term goals (cf. D. de S. Price 1986; Irvine and Martin 1984). In this regard, the STS practitioner is the soulmate of the "philistine" government economist who fails to see why science cannot be subject to cost–benefit analysis, just like every other federally funded social service (cf. Chubin and Hackett 1990: Chap. 6). Perhaps some of the philistinism may be removed by looking at science funding through the eyes of a historical counterfactual.

Suppose it were 1870, and I were a knowledge policymaker interested in promoting an atomic view of reality. Up to this point, scientists had been reluctant to think of the quest for "ultimate reality" in terms of getting at the smallest unit of matter because no techniques existed for isolating and analyzing such units. This impasse was (and still is) discussed as "logico-conceptual" in nature. But why not regard it instead as "techno-economic"? Thus, the impasse could have been discussed in terms of a lack of relevant mechanical devices—something on the order of a dynamo or a digital computer—to stimulate the experimental imagination into proposing testable hypotheses about such micro-units. By calling the impasse "logico-conceptual," the would-be knowledge policymaker is left to the whims of scientific creativity with no clear sense of how to focus funding. However, by calling the impasse "techno-economic," the policymaker can call for the manufacture of certain gadgets that will enable scientists to hang their abstractions on something concrete. The scientist would need to visualize the analogical implications of mapping properties of a theoretical construct onto those of a material object. If a "technological determination" of thought exists, it more likely vindicates McLuhan than Marx: Technology determines less the content than the form that thought takes. A striking piece of technology may even determine that the thought takes a form at all, and not simply criss-cross various levels of analysis. A historical case in point is the focus that the introduction of first the mechanical clock and then the self-regulating steam engine gave to 17th- and 18th-century discussions of governance in the natural and human worlds (Mayr 1986).

Science policy research, however, tends to be problem-centered. Consequently, this research often deliberately avoids recourse to the more systematic cognitive interests fostered by social epistemology. Interestingly, this problem-centeredness has been justified from opposing ideological directions. On the Right, science policy researchers try to solve the problems of their clients in government or industry who are usually interested in manipulating their access to knowledge to serve their own ends. For example, funding for research into the health of factory workers has rarely been done to advance the frontiers of medicine—although it sometimes has had this effect. The more immediate goal has been to prevent worker illnesses from slowing down production schedules. On the Left, science policy research has often been prompted by problems that have reached mass media visibility as instances of science "impacting" on the public.

AN ASIDE ON SCIENCE JOURNALISM

Science policy research plays hardly any role in *discovering* or *constructing* the problems it tries to solve. Unfortunately, this situation also applies to *journalists* who rarely track down stories about science with the same investigative zeal that they would a story concerning a politician. (Greenberg 1967 is the locus classicus for this complaint, which was revisited in Dickson 1984.) Except in cases of scientific misbehavior sufficiently grave to threaten public heath or coffers, journalists tend to print watered-down or mystified versions of scientists' own press releases. This practice only ends up increasing the public's confidence in science without increasing its comprehension (Chubin and Chu 1989: Chap. 3). This state of affairs is an especially curious turn for the "in use" epistemology of journalism to take because the modern journalistic commitment to "objectivity" has much the same constructivist bent as STS research. Both aim to present as many sides of a story as possible, so as to let the reader decide for herself (Stephens 1988: Chap. 13; cf. Mulkay 1985). Just as the public rarely trusts a politician to give the last word on a topic in which they have a vested interest, why shouldn't a similar skepticism (politely put: "open-mindedness," "neutrality") be instilled in the public's understanding of scientific pronouncements? Short of generating alternative facts and theories journalists have done little to raise the public's consciousness about science.

Moreover, journalistic objectivity becomes complicated once besieged scientists openly court the press in quest of a "fair hearing." Here journalists have often brought larger political and economic angles into the disputes that start to give science a public face comparable to that of other institutions. Sometimes (e.g. in the sociobiology controversy) this strategy ultimately benefited the besieged scientists, whereas in others (e.g. the "cold fusion" controversy) it did not. To some extent, this process is a step in the right direction, although in these episodes the press rarely operates with a sophisticated sense of the methodology of science (Nelkin 1987).

In particular, journalists often presume that theory choices are winner-take-all contests that turn on some crucial fact or event (i.e. a news item) that will be decided within a limited time frame (i.e. before boredom sets in). Generally, the more provocative the disputed theory the more likely journalists will champion it. As a result, the burden of proof shifts onto the opponents (typically, the scientific establishment) to design the relevant "crucial experiment." In the case of cold fusion, such experiments were designed and the underdogs lost. But in the case of sociobiology, its distinguished opponents (e.g. Stephen Jay Gould and Richard Lewontin) could offer only more talk to E. O. Wilson's original talk. In that case, boredom soon set in, and the press declared Wilson the winner by default. Some think that, given their role in shaping public opinion, science journalists should be more scientifically literate. Perhaps, however, sociobiology's opponents should take a few lessons in democratic rhetoric (Segerstrale 2000).

Independent science journalism also contributes to a subtler phenomenon—an increased public impatience with the pace of scientific progress. Two images are worth keeping in mind here. The first is the supermarket tabloid, the public's primary source of information about the latest developments in science. The second is the growing pressure on government agencies from both industry and the public to limit the period of testing on scientific products before making them generally available. Clearly, we are ready consumers of science. But we would be wrong to believe that each dominant knowledge system excels by the standards set by its own culture. It is certainly *not* true of our own scientific culture. The problem here is that philosophers—not only relativists—fail to register the effects that publicity for scientists' initial expectations have on the standards used to evaluate subsequent scientific achievements. Promises of impending breakthroughs, strategically made to muster funds from Congress, may come back to haunt the scientists if, on delivery, the goods are late or somewhat less than promised. Discoveries that would have counted as clear cases of progress by an earlier standard now come to appear as disappointments because they fall short of current expectations. Moreover, one scientist's ill-fated boast may unintentionally set the pace for subsequent researchers, who are then forced to contribute to inflated standards of achievement (Klapp 1991).

I do not bemoan the fate of science journalism once it decides to pursue an independent course of investigation. Journalists' instincts are often good. Scientists may complain that journalists take their arguments and announcements "out of context," but often that simply means that they are being taken *literally*. After all, scientists claim universality for their message. What difference *should* public eavesdropping make on promises made originally for the ears of Congress? By simply taking the scientists at their word, the press believes their word is uttered for all to hear. If, however, the press did not so often believe the specific promises of scientists, it might be able to help scientists realize the situated, and hence rhetorical, character of their utterances. However, journalists exude a certain vulgarized positivist sensibility, which sees science as theoretical debate punctuated by crucial experiments. When experiments fail to be forthcoming or crucial, the press simply gets bored, whereas the positivist declares a lack of "cognitive significance" to the proceedings. Yet both attitudes partake of the mythos of "the spectacle"—that combination of "put-up-or-shut-up" and "seeing-is-believing" which dominates both political *and* scientific imagery in a democracy (Ezrahi 1990). Since, historically, a free press has been democracy's most characteristic medium of expression, the pursuit of the spectacular moment should be seen as an attempt less at debasing scientific thought than at reaffirming democratic values. Here then begins the tension between science and democracy that will figure increasingly in this and the next chapter.

MANAGING THE UNMANAGEABLE

In January 1991, the American Association for the Advancement of Science filed a petition. It called on the U.S. federal government to double the science budget over the next decade. However, from a supporting piece that appeared in *The Atlantic* around that time, the call clearly was for more *unmanaged* money to be put into science (Crease and Samios 1991). Perhaps even the current amount of money will do, but with a smaller portion eaten away by such "costly" accounting procedures as grant renewals and program evaluations. Taken at face value, this subtext is much less persuasive: Is that most scientific of standards, efficiency, abhorrent to the conduct of science itself? More pointedly, we might ask: What exactly is supposed to be the difference between administering science "for its own sake" and running it as a profit-making venture judged according to business values? The issue runs deeper than a facile contrast between long-term, market-insensitive investment in "basic research" and short-term, market-sensitive investment in something called "applied research."

Philosophers in the Popperian tradition recognize the similarities between scientific innovation and the best features of entrepreneurial capitalism. For example, Imre Lakatos (1979) distinguished his own position from the "naïve falsificationist" who would immediately disown a program that was subject to many refutations. Lakatos advised holding onto a currently unsuccessful research program until it either attracts enough people to capitalize on its strengths or the needs of the scientific marketplace are restructured so as to favor the program over its competitors. Here Lakatos echoes the entrepreneurial strategy of jumping in early and staying the distance with a new product. He eschews the kind of short-run thinking typically used to sell goods that represent only a slight improvement on those already on the market. In this latter, naïve falsificationist case, once the market for the product dries up, one simply tries to latch onto the next fad. However, if all business enterprises ran on this principle of moderate gains at low risk, no major innovations would ever be made (Brenner 1987).

Historically, the basic-applied distinction was an artifact of U.S. federal government accounting procedures. They were designed to prevent as much science as possible from being implicated in the manufacture of instruments of destruction (Hollinger 1990). Currently, the distinction conjures up differences in both the motivation and the content of science. Put simply, the presumption is that "basic" research has more impact on the conduct of academic science than "applied" work. Nevertheless, "basic" research may be rendered "applied" under two conditions: (i) once the sphere of accountability is extended to include consumers who are themselves not producers of science (e.g. bureaucrats); and (ii) once the frequency with which scientists need to give accounts is increased.

Conversely, if the frame of reference for evaluating the outputs of putatively applied research was made solely by other applied researchers and such accounts were rarely required, the outputs would start to look like basic research. In principle, then, writing up any research project as either basic or applied should not be difficult. Indeed bold defenders of basic research have tried to use this convertibility to their rhetorical advantage:

> The suggestion is that large scientific projects unfairly monopolize scientific capital, squeezing out the little guy who might make valuable innovations if given a chance. But the analogy is false. Knowledge generated by large scientific projects, unlike the profits of large corporations, becomes the property of the entire community and restructures the scientific background against which research teams large and small execute new ventures. (Crease and Samios 1991: 83)

The prior argument trades on what philosophers of language, after Quine (1960), call *referential opacity*: The same thing identified in two mutually exclusive ways (e.g. the expressions "Morning Star" and "Evening Star" refer to Venus but not to each other). In the case of business bigness is condemned, whereas in the case of science bigness is praised. Big Business monopolizes capital, and is therefore bad. But what does monopolizing capital mean other than to have enough clout in the market to force all potential competitors to orient their activities towards one's own? Yet this very consequence of large scientific research projects is then praised! The fact that reference to Big Science as Big Business remains opaque testifies to another missed opportunity in the journalistic portrayal of science. Instead of reporting science like the serious side of the entertainment industry (fluctuating between its own kind of dazzle and scandal), the press should accustom people to follow the short- and long-term trends in the public investment of their tax dollars. After all research is the largest expenditure of federal agencies, and education is premier among local and state authorities. Why not, then, be concerned with performance records? An itemization of projects funded, the proportion of the budget they consume, and their track records at various points would dissipate some of the mystique of unmanageability that Crease and Samios continue to promote. (This idea would appear more attractive if citizens could reinvest their taxes in other public projects, as they see fit, just as they can with their untaxed moneys.)

Philosophers of language generally cast referential opacity as demonstrating that the same reality can be *described* in multiple terms. However, idle description is hardly the only reason why one might want to identify or refer to something. Rhetorically, seeing the multiple identifications of an object as alternative ways of *prescribing* for the future of the object suggests different ways to treat it. These choices, depending on how they are made, could subsequently change the character of the object. (The politically correct term for this process is *performativity*.) Thus, Big Science is untouched if its practices resist the predicate "monopolistic," but they are likely to change if the predicate sticks because of the different evaluative standards invoked by calling an activity monopolistic. Sociologists will recognize this point as following from W. I. Thomas' concept of *definition of the situation*: "If men [*sic*] define situations as real, they are real in their consequences" (Thomas and Thomas 1928: 572).

Referential opacity is often compounded by ambiguous connotations. Generally valued above applied research, basic research more explicitly engages the creative intellect of the scientist. She goes beyond merely enhancing what is already known to make a genuine discovery, the significance of which may remain unknown for some time. However, creativity does not always carry a univocal rhetorical advantage. Sometimes less creativity pays off if the scientist wants to claim credit for something she has done. Patents are a good case in point. The basic researcher may ideally want to distance her work from applied research by claiming that her equipment enabled the manifestation of a phenomenon that would have existed, albeit undiscovered, even without the introduction of any special equipment. After all, the phenomenon's ontological independence

—"realism" in the philosophical sense—makes for a genuine discovery, an insight into nature that merits the engagement of the basic researcher's intellect. Unfortunately, a scientist who fails to stress the necessity of her equipment will be unable to acquire the legal rights and economic power that accrue to patents. Indeed the task of securing a patent may require a role reversal between the ends and means of research. The scientist, for example, may portray her discovery as a demonstration of the equipment's ability to work according to set instructions (Miller and Davis 1983).

The ease with which scientists switch between regarding their work as discovering new things and as extending old ones indicates the rhetorical convertibility of the basic-applied distinction to meet specific needs. This convertibility serves social epistemology's assignment to democratize the intellect. As the flexibility of legal rhetoric suggests, the first step involves acknowledging that all attributions of "creativity" and "genius" are dependent on the reception given to a piece of work and are necessarily made in retrospect (Brannigan 1981). Such works are, in the first instance, anomalies and, as such, may be ultimately diagnosed as the product of either creative genius or foolish effort. The determination depends on whether the anomaly manages to change the disciplinary norms or falls victim to them. So Einstein's 1905 papers marked their author as a revolutionary physicist rather than a harmless crank because of the network of people who came to support, or otherwise rely on, the Special Theory of Relativity. The strength of the network caused the norms of physics to bend to the theory. To claim otherwise is to be faced with the embarrassing question of why a scientist's genius varies directly with the extent of her impact, over which she exerts relatively little direct control.

Notice that this social analysis of genius does not actually involve proposing that someone other than Einstein could have come up with Special Relativity. While perhaps true, relying on a counterfactual makes something about the theory—its "content" perhaps—appear marked as a work of genius no matter who came up with it first. A counterfactual question truer to this analysis is whether mobilizing some comparably extensive network, at roughly the same time, would lead to the revolutionary overthrow of Newtonian mechanics. The idea that scientific creativity can be fruitfully subjected to this kind of network analysis is hardly new to social science (Rogers 1962, updated in Latour 1987).

The point raised here may be cast in the vocabulary of evolutionary epistemology (cf. Campbell 1974). Darwinian evolution requires two sorts of mechanisms, one for genetic variation and one for environmental selection. The traditional epistemological fixation on creativity, genius, and the generation of theories—a focus retained in science policy thinking—stresses variation at the expense of selection. Consequently, policymakers attempt to construct environments that foster creativity before clearly understanding the selective aspects retained in the history of science. For example, although physics was clearly revolutionized by the

Special Theory of Relativity, we still do not know what activities associated with the theory's introduction led to its success (a.k.a. "selection"). The answer would lie in professional gatekeeping practices (especially Max Planck's), prior expectations and interests of potential allies, and competing research agendas. This alternative counterfactual focuses thinking on selection mechanisms that are relatively independent of the context of Einstein's discovery. Nevertheless, these mechanisms determined the theory's survival in the scientific community.

A variant of referential opacity—*strategic opacity*—is increasingly important in philosophical contributions to the public understanding of science. The idea is based on the classical trope of *catachresis*, or the misuse of names. If a situation can be described in alternative ways so as to motivate alternative courses of action, then surely one of those ways could be a literal misdescription that is nevertheless necessary for the audience to act in a normatively desirable manner. At first glance, this simply sounds like manipulation. But "manipulation" presupposes that the audience is being made to act against its own interests or beliefs. Yet in this case the audience has yet to form any clear views on an issue that requires prompt action. If strategic opacity succeeds, then in the long term the world comes to resemble more closely the strategic misdescription.

My example is the role of philosophers of science as expert witnesses on the nature of science. In a trial involving the teaching of Creationism in public school biology courses, philosopher Michael Ruse was asked to demarcate science from nonscience (La Follette 1983). He responded by giving Popper's falsifiability criterion, knowing full well that his answer failed to do justice to the serious objections that philosophers and others have raised to the criterion's plausibility (although it could be turned easily to exclude Creationists). However, had Ruse attempted to represent the complex battles that are waged over even the intelligibility of the demarcation problem, he probably would have undermined the philosophers' credibility as authorities on the normative character of knowledge production. Thus, Ruse's charge was to represent both his opinions and the opinions of others who may be called to testify on similar matters in the future. Even if these future witnesses would oppose Ruse's theory of science, they would probably object even more to being preempted from offering an opinion. Rhetorically speaking, even if philosophers of science have abandoned Popper, Ruse may still be right that it would be to the advantage of both our knowledge enterprises and the public at large—at least in this case—to act as if falsifiability were true.

The social function of strategically opaque accounts of science has been long familiar to philosophers. For example, John Herschel's *Preliminary Discourse* presented the scientific method to the lay Victorian audience as systematically applied common sense. Herschel wanted to normalize science's relations with a public that marveled at the spectacle of experimental demonstration, but remained skeptical of its relevance to the "humane" knowledge mastered in the British liberal arts curriculum. Herschel's strategy was to transfer the "technical" character of science from the construction of apparatus to the design of nomenclature: a conversion of experiment to rhetoric. He deployed oversharp distinctions in the stages of scientific reasoning—such as the contexts of discovery and justification—that were illustrated by homely examples. These stock cases are used when philosophers of science argue about the nature of science. This last point is important because, in the 20th century, philosophy of science came to be practiced more by professionally trained philosophers than by scientists. Thus, Herschel's rhetoric was crucial for philosophers to convince *themselves* that they could opine significantly on the nature of science after having mastered some scientific vocabulary and syntax but without laboratory training. Although this "shallow," "merely philosophical" view of science has received criticism, it nevertheless kept alive a publicly accountable image of science throughout this period of increased disciplinary specialization.

Referential opacity is just one tactic by which public attention is diverted from the more encumbering social consequences of Big Science. The most effective tactic involves the biggest ruse: It is to believe that the only consequences of research are the officially intended ones. For example, social science research, given its focus on the human and on the applied, likely has more socially dislocating consequences than, say, basic research exclusively designed to study the abstractions of microphysical reality. This particular myopia follows from overlooking the material character of intellectual needs. Specifically, *that even unintended consequences need not be unexpected*. A careful empirical study into the social effects of different lines of research might enable the prediction of outcomes that researchers did not intend. This result would be a valuable tool for prying open scientists' *ex cathedra* pronouncements on what their research can or cannot do. Policymakers evaluate any practice, including an intentionally scientific one, in terms of the groups most likely affected by that practice's consequences in an appropriate expanse of space and time. For instance, although a series of high-energy physics experiments is intended to affect the community of high-energy physicists, presumably in a positive manner, the experiments' biggest impact may turn out to be on another disciplinary community even more impressed by the results. More to the point, lay people conceptually unconnected to science may find themselves the indirect recipients of subsequent experimental applications.

The typical high-energy physics experiment offers an especially vivid example of the strategic conflation of intention and expectation. What is tested in such an experiment? The intended answer, of course, is some range of hypotheses about the nature of microphysical reality. But given the material conditions needed for realizing this intention, we should come to expect that other hypotheses will also be tested at the same time—not in physics, however, but in political economy. These social experiments, no less than their "natural" counterparts, involve the enforcement of *ceteris paribus* clauses. That is, the experiments are designed to exclude all factors from the test site other than the ones that are thought to bear some responsibility for the phenomena under investigation. In this way, scientific research appears subject to its own kind of inertial motion. For example, current high-energy physics experiments commonly pool the financial and human resources of several countries based on an international agreement. The agreement's wording constitutes instructions for converting the physics experiment into a test of a certain theory of international relations. The experiment also tests a certain scheme for redistributing income and personnel. After all the physicists' freedom to manipulate variables as they see fit rests on the ability of governments, universities, and other scientific support agencies to coordinate labor and capital over vast spaces for long periods that might otherwise move in disparate directions. Indeed large-scale natural science experiments are both the most powerful testing ground for hypotheses about social interaction and potentially the biggest source of large-scale social dislocation during peacetime.

The tendency to conflate intention and expectation is ultimately a Platonic conceit. Having one's mind in harmony—or in "reflective equilibrium" as students of John Rawls (1972) like to say—is a matter of knowing what one wants and wanting what one knows. However, the Jesuit moral casuists foresaw the hazards of this conflation four centuries ago and tried to reestablish a distinction between the *epistemic* (i.e. the expected) and the *ethical* (i.e. the intended) sides of action with *The Doctrine of Double Effect* (Harman 1983). The Jesuits, unfortunately, formulated the doctrine accordingly: You can expect things you didn't intend, and, therefore, you can knowingly do something without being culpable—a convenient moral psychology for the religious warrior! In contrast, one can invoke the doctrine to demystify the idea that, say a physics experiment is only—or even primarily—about physics. Consequently, it may empower nonscientists (including policymakers), rather than excuse scientists. In short, then, scientists should be held accountable for what can be expected to follow from their hypotheses, regardless of their intentions.

Regrettably, the distinction between the expected and the intended remains clouded regarding the context in which unintended consequences are most often discussed—economic prediction. Economists postulate an idealized rational agent who, although not a Platonist, always seems to intend in proportion to her expectations. When she does not, the consequences are generally beneficial as in invisible hand accounts of economic order. If the economic agent is not omniscient, she at least remains *blissfully* ignorant. Yet the evaluative asymmetry between basic and applied research creeps into how even this conflation is handled by the defenders of pure inquiry. Only basic research is portrayed as having positive unintended consequences (usually in opening up new lines of inquiry, but often in the applied realm as well). Applied research is seen as having primarily negative consequences especially in terms of foreclosing opportunities for pursuing basic research, but also in its unwitting production of instruments of mass destruction. The positive unintended consequences of basic research supposedly flow "serendipitously" from the unconstrained pursuit of inquiry. However, the negative unintended consequences of applied research appear to be opportunities that ideologically inspired ministers of science are all too eager to exploit.

From a strictly scientific viewpoint (the viewpoint from which one might think scientific rhetoric should be judged), all the anecdotes that may be cited as evidence for the beneficial by-products of basic research, and the destructive capabilities of applied research, are the stuff of which superstitions, rather than careful policy, are made. As the cognitive psychologists say, the privileged anecdotes contribute to a *confirmation bias*. To claim that basic research unwittingly courts good and avoids evil better than applied research consider the following possible questions:

1. If a large enough expanse of space and time is examined, might not the effects of basic research turn out to be just as deleterious as the consequences of applied research?

2. Or, rather, might not the effects of applied research turn out to be just as beneficial?

3. Even granting the serendipitous consequences of basic research, might not applied research have reached the same conclusions sooner and more efficiently?

4. Even granting that serendipity reaches those conclusions more efficiently, might not more desirable conclusions have been reached by replacing a particular line of basic with applied research?

Questions (1) and (2) ask the historian to manipulate the parameters within which she examines the actual consequences of applied and basic research. Questions (3) and (4) call for counterfactual historiography. This practice, having established its credentials in economic and social history, has yet to take root in intellectual history. The general strategy would be to go back to the latest point in time when the alternative trajectory in question could have been pursued. Then one would estimate the probable consequences of pursuing that trajectory, instead of the one actually pursued, assuming that little else of the actual subsequent history would have changed. The goal would be to show that, given the chance, applied research could perform at least as well as basic research without disturbing too many historical background assumptions.

A burgeoning sphere of litigation exists where the manipulation of possible pasts and futures makes a major practical difference. The cases turn on the liability of scientific research for unwanted environmental change. The battle between Big Science and the Ecologists is often portrayed as a disagreement over matters of fact and levels of risk, but behind it all is a dispute over one's sense of history. Ecologists typically suppose that the trajectory of scientific research will not veer enough off its current course to preempt or resolve any long-term environmental disasters. Yet Big Scientists presume that most of the potential for disaster will be contained or addressed by potential research breakthroughs. Given this contrast in historical vision, Big Scientists, on the one hand, have a fairly short-term conception of liability (since significantly new factors may intervene in the future to confound any current tendencies). Ecologists, on the other hand, project their legal concern on the long term, wanting to hold scientists responsible for the remote consequences of their actions. In these cases, the courts adopt a third-party standpoint—the involuntary stakeholders, if you will—who are the potential beneficiaries or victims of the scientists' actions. In a fully democratized knowledge enterprise, the effects of unsuspecting third parties might serve as the sociological surrogate for the check of an "independent reality" or "external validity." Thus, the judge in an environmental damage case would revert to the Greek origins of her office, *kritos*, the "tester" of alternative causal accounts (Kelsen 1943).

The basic-applied distinction is truly clear only in government accounts of science funding. However, the philosophical history of the distinction has aimed to keep basic research beyond accountability. Consider the *pragmatist* vision of science, especially as articulated by John Dewey, vis-à-vis the *positivist* vision articulated by the members of the Vienna Circle. Pragmatists saw the epistemic authority of science as resting in its ability to transform nature in the interests of humanity (Procter 1991: Chap. 3).

Dewey saw no sharp distinction between basic and applied research and had no desire to make value neutrality a virtue of science. In contrast, the Vienna Circle traced the epistemic authority of science to the logically valid and empirically testable terms of its theories. Whereas for Dewey "instrumentalism" indifferently referred to a position in epistemology and ethics, for Vienna Circle eavesdropper A. J. Ayer such indifference was the height of philosophical folly. What lay between the pragmatist and the positivist was World War I. The German scientific community—generally regarded without peer on the world scene—openly accepted responsibility for the military hardware that led to the most devastating war in history, culminating in a humiliating defeat for Germany. This admission unleashed an antiscientific irrationalism in the 1920s. Intellectuals supporting science adapted. They promoted a science that the public could see either as irrationalist or as conceptually independent of its destructive technological capability. The indeterminacy thesis in quantum mechanics—which denies causal determinism for microphysical reality—is an outgrowth of the irrationalist tendency (Forman 1971), logical positivism an outgrowth of the independence tendency. To put science beyond reproach, positive value connotations were reintroduced by emphasizing that some of the best consequences of basic research may come in unexpected quarters. The much publicized service of basic physics researchers in the Allied cause in World War II performed the function effectively for the popular imagination. These events were celebrated in Vannevar Bush's *Science: The Endless Frontier*, the ideological statement behind the founding of the U.S. National Science Foundation.

But why should scientists, and their favorite epistemologists, resort to these backhanded rhetorical maneuvers to avoid accountability? What have scientists to fear from subjecting themselves to greater public scrutiny? Nothing, except a stereotype of what being accountable means. That stereotype reaches back to the primal moment of accountability, the academic exam, in which an individual's merit is judged on the basis of an externally driven standard. For example, the inquisitorial style of courtroom accounting procedure that characterizes Continental European legal systems arose from the practice of university examinations in the Middle Ages (Hoskin and Macve 1986). As Foucault suggests, both society and the individual are "co-produced" in the process of inquisition. But accounting need not be a process for measuring the fit of individual cases to general rules. It can, rather, be a diagnostic procedure that treats cases as symptomatic of the overall state of the rules. Thus, wayward scientists need not fear having their PhD's revoked if their deeds fail to match up to their words. Instead the scientists' incentive structure may be altered so as to get them to work in a different way or in a different field. Moreover, accounting for science may act to award compensation to affected third parties, especially when the consequences of research stray significantly outside the academy. A polluting laboratory, for example, might be required to devote substantial research to cleaning up after its messes or, perhaps, developing technologies that improve the well-being of the affected parties.

THE SOCIAL CONSTRUCTION OF SOCIETY

Revering each of these distinctions—basic-applied, invention-discovery, genius-error—stands the folk wisdom of science policy on its head. Doing so is central to the doctrine of *the social construction of facts and values*, the philosophical cornerstone of most STS research. This doctrine maintains a sharp separation between determining *what the norm is* and *when the norm applies* and, in turn, distinguishes the *script* from the *scene* of action. A norm is any pattern of social action that is scripted. Consequently, a distinction can be drawn between right and wrong ways to perform the action. So far social constructivists and philosophers of science agree regarding how scientists justify their research to each other and to policymakers. These justifications are taken generally to work and, hence, continue to keep the scientists in business. All would cite chapter and verse of the hypothetico-deductive method and other positivist scripts as examples of scientific norms.

But now ask the constructivist and the philosopher *why* the script works. Whereas the philosopher will focus on properties of the script (e.g. its logic), the constructivist will turn to the scenes where the script is typically enacted. These scenes provide access to the specific mechanisms that enable the verbal performance to elicit the desired effects. Moreover, the constructivist need not presume that these mechanisms will be the same across situations. Instead she may simply believe that the script must be performed somewhere at some point. Indeed I am such a *script transcendentalist*—someone who believes that arguments and claims concerning the valued form of knowledge or "science" are necessary for the possibility of society. But I leave open to empirical investigation (of the past and present) and normative negotiation (in the future) the exact backdrop against which such arguments and claims can be successfully made (Fuller 1988a: Chap. 7).

Philosophers of science are no strangers to the study of scenery. However, this study is shrouded in Latin, the *ceteris paribus* clause, and shoved into the background of philosophical analyses (Fuller 1988a: Chap. 4). Philosophers suffer from the physicist's prejudice of undervaluing in concrete what can be so easily done in abstract, as in the case of deriving the laws of motion from a world of frictionless planes. For their part, constructivists realize that heavy transaction costs are incurred in moving from the abstract to the concrete. Human and material resources, for instance, need to be strategically situated (including things that were prevented from getting in the way) for "all other things to be equal." The folk wisdom of science policy is symptomatic of a metalevel version of the same prejudice. Just as the physicist regularly forgets to consider exactly how one would materially construct a frictionless plane, likewise the policysmaker forgets to consider what it would take to construct environments to enable future physicists to arrive at their abstractions. The script is thus fallaciously made to do the work of the scenery.

The more locally one considers the construction of scenery needed for enacting a script—say, one laboratory that agrees that certain evidence supports a certain hypothesis—the more social constructivism appears to be a species of dramaturgy. Indeed the followers of Erving Goffman and Harold Garfinkel who have introduced a microsociological perspective in STS have conveyed this impression. Consequently, STS practitioners espouse a bias toward *localism*, or the ontological privileging of the "here-and-now" over the "there-and-then." Sometimes localism is little more than a politically correct way to talk about what positivist philosophers of science have called "the observable." Other times localism is simply a nominalist (i.e. a negative) stance toward the reality of such macrosocial entities as institutions and classes. In either case, the STS practitioner takes herself to be showing how various localities interlock to produce the dispersal of effects that characterizes today's technoscience (Ophir and Shapin 1991; Shapin 1991). Such a research agenda presumes that the places where scientific work is done are "indexes" for various sorts of knowledge. In a more rhetorical vein, these places serve as reminders of the skills that are called for on particular occasions.

Indexes also trigger what art historians call *iconographic* associations. These associations supply the observable foreground with an affectively charged conceptual background. For 20th-century art historians like Erwin Panofsky and Ernst Gombrich, iconography effectively documented the *Weltanschauung* or collective memory of a culture as a set of ubiquitously cueable and applicable symbols (cf. De Mey 1982: Chaps. 10-11). These associations are verbally elicited when people are asked to explain or excuse their behavior. Thus, engaging in a routine lab technique involves a certain attitude toward the activity that the participant–observer tries to access conversationally. In effect, this attitude toward one's place—one's "station" as it were–is the manner in which the scientist embodies her community's ethos (Polanyi 1957). It suggests a more expansive sense of indexicality. What then is the appropriate binary contrast to "local"? After all, the very existence of iconographic memory concedes that some of the most important things that happen and matter locally come from the outside. In short*, the nonlocal is always already inscribed in the local.*

This last point finally allows us to talk about knowledge policy from a constructivist standpoint. Can interlocking enough locales together ever produce the sort of "global" picture of knowledge production that would enable a policymaker to set priorities, anticipate outcomes, and adapt to changes in "the system"? Both positivists and Marxists, bureaucrats and activists, are skeptical of the constructivist attempt to eliminate such macrostructures as "power" and "objectivity." These macrostructures often slip between the cracks of locales, yet give scientific knowledge its distinct sense of independence from much else that happens in the social world (Fuller 1988a: Chaps. 2, 10). Here is a strategy for explaining knowledge production that attempts to respect both local and global sensibilities:

1. The translocal uniformity of a piece of knowledge is largely an artifact of the restricted channels—the standardization of words and objects —in which knowledge must be officially communicated.

2. Nevertheless, one wonders why a wide range of independently and diversely managed laboratories find themselves communicating roughly similar, if not downright identical, messages.

3. The answer is that each such message should be treated as the predictable outcome of a decision procedure that, although differing across labs, has precedent as the decision procedure used in other sectors of society, with which lab members would have had some contact.

4. Thus, the apparent independence of the knowledge that emerges from multiple labs is due to a concatenation of individually predictable events that are then rendered uniform by the restricted channels mentioned in (1).

Consider the convergence of the physics community on the existence of neutral currents. Pickering (1984) has shown that the different labs involved behaved in ways that could have been predicted based solely on their particular social arrangements, even if the existence of neutral currents were not at issue. Just because the labs *end up* agreeing on the existence of a particular entity, it does not follow that their agreement is *due to* the existence of that entity. For example, one lab may come to believe in neutral currents because it always follows whatever the research director thinks. Another lab may come to the very same belief as a result of a weighted average of what the entire research team thinks. If operating in customary fashion, each lab's convergence in beliefs could have been predicted simply on the basis of knowing its decision-making procedures, without knowing anything about the content of the belief on which they converged.

The natural conclusion to this line of thought is that the convergent belief in neutral currents is an epiphenomenon of the diverse social processes that issued in assertions of that belief. This view runs contrary to such official communications as journal articles, which give one the impression that the various labs reached the same conclusions for largely the same reasons. The decision-making procedures that distinguished the labs earlier—deference to a superior and the weighted averaging of peers—are found in other, nonscientific sectors of society. Indeed these procedures go to the very heart of how modern society is organized and maintained.

THE CONSTRUCTIVE RHETORIC OF KNOWLEDGE POLICY

We have stressed the *social* over the *construction* side of social construction. However, the construction side brings us into the heart of the rhetoric of knowledge policy. The rhetoric of knowledge policy covers the construction of individual rationality out of beliefs and desires and the construction of collective rationality out of facts and values—the rationality of the *researcher* and of the *research* as it were. I call these the rhetoric of *rationality attributions* and *fact-value discriminations*. Let us consider each in turn.

The Rhetoric of Rationality Attributions

Rationality is the rhetorical balance sheet for our budget of *beliefs* and *desires*. Be it ordinary common sense or rational choice theory, beliefs and desires tend to be emphasized at the expense of each other. Desires and beliefs are the mind's metaphorical movers and moved. Only a small imaginative leap is required from here to David Hume's aspirations for a “mental mechanics” to parallel Newton's physical mechanics. Thus, a picture of the mind containing mobiles and mobilizers, passive reflectors (beliefs), and active resisters (desires) of nature would support some epistemologically sharp distinctions. For beliefs and desires are usually held to be irreducible to anything else, including each other. For example, beliefs are tempered by evidence, whereas desires are often strengthened by evidence to the contrary. But more fundamentally, can we clearly say when something should count as a belief rather than as a desire?

At stake here are the criteria used to evaluate the rationality of someone's actions. We normally explain behavior by appealing to a configuration of beliefs and desires. However, we tend to lean more heavily on beliefs when evaluating actions by criteria in the agent's immediate vicinity that are not necessarily of her own creation. In contrast, desires bear a greater burden when the evaluative frame of reference is expanded to cover criteria of the agent's own creation although often not in her immediate vicinity. Thus, to answer why Mary walked out into the rain without an umbrella, we can say either that she did not think it was going to rain or that she wanted to get to the office quickly. In the former, belief-driven account, Mary appears to have simply erred. In the latter, desire-driven rendition, Mary is portrayed as having taken a calculated risk. Clearly, the two accounts are compatible, yet the first Mary is a victim of misinformation, whereas the second Mary deliberately suffers short-term losses to achieve long-term goals. Our evaluation would not change had Mary gone out into the rain *with* the umbrella. On the one hand, her correct belief would correspond to a reality (i.e. that it was raining) that did not require that belief for its existence. On the other hand, her risk would have appeared still more calculated, thereby enabling her to minimize even short-term losses.

Generalizing from the prior example, we see that an agent's rationality can be rhetorically enhanced by giving desires the upper hand over beliefs in the explanation of her actions. Desire-driven accounts can mitigate the surface irrationality of an isolated episode by making it comply to a more extended life plan. This point has been invoked by behaviorists in reinterpreting the many cognitive psychological studies that make people out to be incompetent calculators of expected utility. Rachlin (1989) observed that these studies focus exclusively on the performance of subjects in the experimental task. Accordingly, these studies portrayed the subjects as possessing false beliefs about their immediate situation, rather than relating their response to some long-term goal not directly represented in the situation. To avoid such “instant rationality” judgments—without having to postulate a “deep structure” to the mind—behaviorists assessed the rationality, or “efficiency,” of animal response on the basis of a *series* of trials and usually not until a stable pattern of performance was detected.

Ironically, then, the rigid protocols of cognitive psychology have compelled behaviorists to recover some of the phenomena associated with the very mentalism whose existence they have traditionally denied. These mental phenomena include foresight, hindsight, and any other form of inference that forces the organism to adopt a historical perspective toward its own behavior.

The Rhetoric of Fact-Value Distinctions

When knowledge policymakers argue for either maintaining or changing a line of research, a strong distinction between *facts* and *values* can play a strategic role in the argument. If the policymaker wants to stick to a research trajectory despite resistance from the environment, she can appeal to the "value" of pushing onward. But if she is looking for an excuse to abandon the trajectory, an appeal to the countervailing "facts" of experience will typically figure in a winning strategy. The strategy outlined next is largely an elaboration of this point.

The practice of replicating experiments is central to science's self-image, especially to its image of having a firm database. Thus, Harry Collins' multi-pronged challenge to the feasibility of the norm was bound to prove controversial. According to Collins (1985), professional disincentives exist to performing replications (i.e. they were rarely published). Still even in cases where replication was crucial for continuing a line of research, important details of the original experiment could be gleaned only by personally contacting the experimenter since the published text turned out to be singularly uninformative. Collins' empirical finding calls enough of the policymaker's natural understanding of science into question to force her to take a stand on whether replication is part of the "is" or the "ought" of science. These recalcitrant cases may be seen either as refuting replication as a fact about science or as violating replication as a norm governing science: One interpreter's falsification may be another interpreter's infraction. It all depends on how one manages the anomalies.

The foregoing line of reasoning builds on David Bloor's (1979) attempt to use Mary Douglas' anthropology of cultural boundary maintenance to make sense of Lakatos' (1978) theory of anomaly management. Lakatos identified four strategies for handling counterexamples to mathematical arguments: monster barring, monster adjustment, exception barring, and Popperian falsification. Drawing on Douglas, Bloor argued that a society's preference for one or another of these strategies will depend on so-called *grid-group factors* (i.e. the society's internal stratification [grid] and its external differentiation from other societies [group]). I propose that *episodes of anomaly management bring into existence the occasions that warrant making the fact-value distinction.*

Initially, I present a grid-group analysis of possible policy reactions to Collins' counterinstances to replication as a norm of science. Then I offer a grid-group analysis of a science policy issue. Running through the Collins case helps show that grid-group analysis speaks not only to the management of "dangerous objects"—Douglas' original concern and subsequent direction (e.g. Douglas and Wildavsky 1982)—but also to more abstract threats, namely, to one's implicit theory of how the world (or some part of it) works (cf. Thompson et al. 1990). With that in mind, grid-group analysis suggests that Collins' counterinstances can be examined along two dimensions as epitomized in the following questions:

(X) Is replication judged against the counterinstances (i.e. descriptively [X-]) or is it the standard against which the counterinstances are judged (i.e. prescriptively [X+])?

(Y) Are the counterinstances representative of a more general tendency (Y-) or restricted to just those cases (Y+)?

The (X)-axis captures the "group" character of the judgment. Here the policymaker must decide whether the practice of replication will be opened to correction from the counterinstances ("low group") or whether the counterinstances will be banned to uphold the integrity of replication as a norm ("high group"). The (Y)-axis captures the "grid" character of the judgment. The policymaker, in this instance, must decide whether there is likely to be a difference between the instances that Collins reports and those that have yet to be observed in the relevant population. "Low grid" suggests no substantial difference between the seen and unseen cases; "high grid" suggests more heterogeneity. Thus, combining the possible answers to the previous two questions, the following interpretive possibilities emerge:

(X-,Y-) *Falsification:* Replication is an empirical hypothesis about how science works. The hypothesis may be rejected in toto in light of counterinstances.

(X-,Y+) *Exception Barring:* Replication is a principle whose empirical breadth may be adjusted in light of the counterinstances. These counterinstances are thus rendered irrelevant to a proper test of the principle.

(X+,Y-) *Monster Barring:* Replication is a normative standard that may be used to discount all counterinstances as cases of scientific malpractice.

(X+,Y+) *Monster Adjustment:* Replication is a principle whose normative depth may enable a charitable reinterpretation of the counterinstances. These counterinstances are thus rendered less contrary than they first seem.

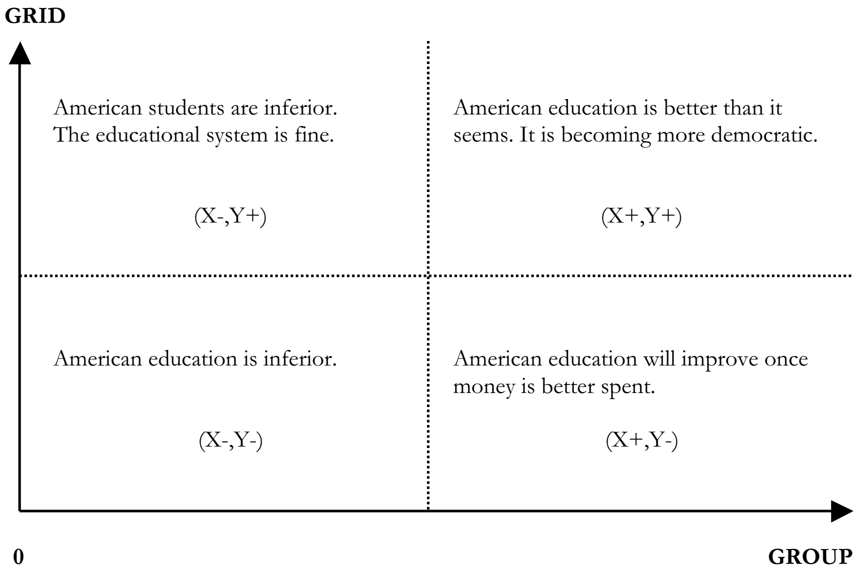

FIG. 7.1 A grid-group analysis of responses to the crisis in american education.

We can easily apply this anomaly management scheme to a major knowledge policy issue: the long-term decline in the academic performance of American students when compared with their counterparts in other countries (see Fig. 7.1). This decline comes in spite of the large and increasing funding for education. Moreover, the decline *prima facie* challenges a piece of policy folk wisdom expressed by the following maxim: "Academic performance will improve in proportion to the amount of money spent on education" (cf. Averch 1985: Chap. 4).

Commentators on this anomalous state of affairs have occupied every position on Bloor's scheme. Corresponding to (X-,Y-) is a frank admission that American education is inferior, and that clearly current funding patterns are not improving matters. These critics take the trend as symptomatic of a need to radically rethink our educational policy. Being low on both group and grid, these commentators are receptive to the educational initiatives taken in Europe and Japan. Representing (X+,Y-) are those who find the trend relatively superficial, suggesting simply a problem with the accounting procedure used to evaluate education funding. Perhaps moneys are being used to renovate buildings, for instance, when they would be better spent on raising the salaries of the best teachers. Tighter scrutiny would presumably remedy such poor managerial judgment. The strategy is to locate "the enemy within" who can be scapegoated and ultimately exorcised. As a result, balance is apparently restored to what is essentially a sound educational policy. Critics occupying position (X-,Y+)

might alter the terms of the argument by pointing out that, although American nationals continue to decline academically, a larger number of foreigners are matriculating in the United States, where they form an ever-increasing percentage of the excellent students. This account suggests that the problem is more contained than first appearances indicate. America, then, is becoming more of a world educational mecca, thereby vindicating the folk wisdom. Unforeseen, however, was that relatively few Americans would thrive in this competitive environment. Thus, these commentators advise a continuation of the same policy, but with revised expectations about the policy's exact beneficiaries. Finally, the rosiest picture is painted by (X+,Y+). Here the decline in test scores is symptomatic of the relative democratization of education in this country vis-à-vis other parts of the world. People from all walks of life now go to school in this country, for a variety of reasons, few of which can be satisfactorily evaluated by standardized test scores. Accordingly, an apparent sign of failure is reinterpreted as a success in disguise.

Philosophers who insist on a "real" fact-value distinction would interpret our grid-group analysis as suggesting that the "is" and the "ought" pull in opposite directions. For example, the maintenance of replication as a norm of science rests on marginalizing new information about scientific practice. Giving that information its empirical due, however, would undermine replication's normative status. Yet if we focus on policymakers' natural understanding of how science works, then its specifically "empirical" and "normative" features are revealed only in cases where its naturalness is challenged. As long as replication is regarded outside the context of problematic cases, the policymaker will unlikely feel any need to decide whether it is descriptive or prescriptive of science. But once the counterinstances are conjured up, policymakers are forced to take a stand in terms of the four options outlined earlier. By the logic of this argument, then, a strong sense of the fact-value distinction should arise in periods of severe challenges to a long-standing natural understanding of things, or what a positivist might regard as genuine tests of a set of beliefs.

ARMED FOR POLICY: FACT-LADEN VALUES AND HYPOTHETICAL IMPERATIVES

The sociologist Max Weber has been most closely associated with modern concerns about separating "is" from "ought" or distinguishing "fact" from "value." Weber's position is normally caricatured as wanting to protect factual inquiries from being tainted by value commitments (i.e. the *value-freedom* thesis). But Weber was more inclined to the opposite thesis: that values needed protection from facts (Proctor 1991: Chap. 10). He did not want to make our value aspirations hostage to the fallible and partial forms of knowledge represented by the latest scientific trends (i.e. the *fact-freedom* thesis). Weber's training in economics can explain his existentialism.

Economists believe that we are saddled with too many possibilities for action and a scarcity of the knowledge needed to eliminate all but the best of them. Personal commitments and social conventions must therefore compensate for the uncertainty of this situation. A third reading of Weber suggests that we value certain social practices and their products—scientific ones, in this case—only because we presume that certain things are true about their role in society as a whole. But if these presumptive truths were shown to be false, then the value of the practices and products would be thrown into question. In short, I claim that the *fact-laden* character of value commitments is more rhetorically revealing than the *value-laden* character of facts. This additional Weberian thesis is often held responsible for stalemating rational discourse (Fuller 1988a: Chap. 12; Fuller 1989: Chap. 3).

My view may be usefully contrasted with the pragmatist analysis of the fact-value distinction classically presented by John Dewey (1958, 1960) and, more recently, and specifically in the context of science, by Larry Laudan (1990, 1996). The pragmatist analysis also emphasizes the fact-ladenness of values, but only *after* the fact-value distinction has been already made. It does not explain how the distinction first gets constructed. From the social epistemologist's standpoint, this prior move is crucial for pragmatism's much-vaunted "instrumentalism" remaining a tool for critical, and not merely technocratic, rationality (Fuller 1994). The pragmatist argues that norms are really hypothetical imperatives for reaching a certain end by the most efficient means. The imperatives are experimentally derived regularities for which any ordinary human action is potentially a test case. The primary role of the social sciences is to discover and codify these regularities, evidence for which has been accumulating since the dawn of civilization.

But how does one decide on which end to pursue? According to the pragmatist, each end can be regarded as a means to some other end. Each end may then be factually judged by the extent to which it enables the higher end to be achieved. For example, a typical hypothetical imperative would be (assuming that it is true), "If you want to expedite the growth of knowledge, then pick theories that explain the most data by the fewest principles." But why might we want to expedite the growth of knowledge? Is this an end that we must embrace or reject unconditionally? No, says the pragmatist. We may regard expediting the growth of knowledge as a means toward improving the quality of human life. Whether it actually does so is an empirical question. Distributing currently existing knowledge more widely may turn out to be a more efficient means for improving the quality of human life than encouraging the production of new knowledge that only elites can use. In that case, if we want to expedite the growth of knowledge mainly because we thought that it would best promote the quality of human life, then we should stop expediting and start redistributing instead.

The pragmatist analysis starts by treating as an open question which of several courses of action one ought to pursue. Thus, the normative inertia that ordinarily engulfs the policymaker has been interrupted by the time the pragmatist enters the picture. Given Dewey's definition of intelligence as one's ability to "react to things as problematic" (Dewey 1960: 224), the pragmatist is understandably reluctant to admit the robustness of normative inertia among intelligent beings like policymakers. In contrast, my own analysis addresses how the policymaker's inertia might come to be interrupted. One can show that even an unproblematic course of action presupposes an account of how the world works that makes the action appear natural. However, once these presumptive facts are challenged, then the policymaker is forced to sort out explicitly facts from values. Consequently, she must choose from among a variety of means and ends in the manner that the pragmatist suggests.

But that is not the end of the story. My analysis can be applied to the pragmatist's, leading to the following question: What does the pragmatist's very strategy of constructing hypothetical imperatives presuppose about how the world works, and what if those factual presuppositions turn out to be false? Even avowedly pragmatist accounts of knowledge may contain empirically dubious premises that need to be ferreted out if the accounts are to prove truly practicable.

Unfortunately, this story offers an additional wrinkle because policymakers often implicitly rely on pragmatist principles to frame their own inquiries. Ironically, policymakers may have been misled into thinking that pragmatism is more practicable than it really is!

The idea I have in mind is that the track record of a hypothetical imperative consists of multiple cases of single individuals or groups (and pragmatists are crucially indifferent between these two possibilities) who have tried to achieve their ends by using a stipulated means. This seemingly innocent assumption is built into the form that a hypothetical imperative typically takes. The form is a statistical correlation between indefinitely many *independent* events of two types, one type covering those who pursue a given end and another type covering those who use a given means. Accordingly, certain features of human pursuits are not represented in this analysis: How many people are attempting to pursue a given end or use a given means *at the same time?* With what *other ends and means* are these people pursuing the end and means stipulated in a particular hypothetical imperative? These two questions remind us of the commonplace that no one follows a hypothetical imperative in isolation from other people and other imperatives. A good example of this shortcoming in pragmatist thinking was discussed previously regarding Laudan's attempt to test 300+ philosophical norms of scientific change against a set of historical case studies.

The pragmatist misses here what has traditionally been regarded as the source of the normative dimension of such imperatives. Following from the

Scottish Enlightenment tradition, I take it that the feature distinguishing norms from ordinary statistical regularities is that norms enable many agents to pursue diverse projects at roughly the same time by drawing on a common pool of resources (Hayek 1973). According to this tradition, norms emerge out of a concern that agents may unwittingly interfere with one another's pursuits, thereby leading to counterproductive results for all involved. A norm, then, is rarely the most efficient means by which any given agent could pursue her ends. Rather, the norm offers a relatively efficient means by which a diverse group of agents can pursue their ends with a reasonable chance of success. Therefore, to assess the normative range of the pragmatist's hypothetical imperatives, we need to know the social environments in which these imperatives were operative. Here the force of pragmatism as an "experimental" approach to knowledge and value may be felt, but in a way that goes beyond the pragmatist's own analysis.

In a laboratory, the experimenter can control the interactive effects of competing subjects or competing ends and means to whatever degree she deems appropriate. In so doing, she approximates the social conditions presupposed in the construction of the pragmatist's hypothetical imperatives (Fuller 1989: Chaps. 2-3). But the same cannot be said of the historical track records on which the pragmatists actually wish to rely. Yet it is methodologically naïve to think that the fate of a given means to achieve a given end is unrelated to other means and ends pursued at roughly the same time. Recall our original example of a hypothetical imperative: "If you want to expedite the growth of knowledge, then pick theories that explain the most data by the fewest principles." In each supporting historical case, few competing principles may have explained a range of disparate but relatively well-defined data. Therefore, if too many scientists follow the announcement of this hypothetical imperative, then the resulting proliferation of principles and data domains might undermine it as an efficient means to expedite the growth of knowledge (Ackerman 1985). Natural science would start to look like sociology, literary criticism, or even pre-Socratic philosophy.

The pragmatist's failure to see these consequences of her position is revealed in Dewey's easy recommendation that the hypothetical imperatives be made available to the public at large. Dewey (1946) presumed that human welfare would be best promoted by involving as many informed people as possible in the knowledge enterprise. Yet the success of many, if not most, of the hypothetical imperatives that can be inferred from the history of science has crucially depended on restricting access to the knowledge enterprise. Whether these imperatives would work in environments more democratic than the ones in which science has been normally conducted is unclear. This argument is not against democratizing science, but a cautionary note about the complexities in using history as a basis for making science policy. In our own day, feminists are probably the most alive to this point, especially in their deliberations over whether, in the long term, the influx of women into science will change how and why research is done (Harding 1986: Chap. 3; Harding 1991: Chap. 3). In this regard, pragmatist intuitions match those of "liberal feminists" who do not envision that a massive change in personnel will radically alter the character of the enterprise.

While drawing lessons from history is tricky, we cannot solve *all* our problems by laboratory experiments on groups of scientists working under various conditions. After all my critique of pragmatism ultimately rests on pragmatism's insensitivity to the *frequency* and *distribution* of a given norm across society. In short, pragmatism lacks a theory of *power*. Seen in this light, the standard methodologies for studying science have some striking shortcomings. On the one hand, histories (and ethnographies) tend to overestimate the pervasiveness and, hence, the constancy and even the "naturalness" of a readily observable pattern. On the other hand, experiments (including computer simulations) commit a complementary sin. Experiments take their circumscribed ability to produce alternative results, by changing initial conditions, as a sign of the malleable and even "artificial" character of the norms that are currently in force outside the lab. If exclusive reliance on the historical method engenders a conservative politics of science, a similar reliance on controlled experimentation should issue in an impracticably radical politics of science: Mannheim's (1936) ideology and utopia revisited!

However, something more positive may be said as well. Recall Collins' studies of experimental replication: Instead of concluding that replication is either an unfalsifiable norm, or a falsified hypothesis about scientific practice, the policymaker may reason that replication seems to be, in principle, an effective way to ensure quality control in the scientific enterprise. If so, then she should ask *not* whether individual scientists do it or even whether they can do it. Rather she should ask: *At what level or unit of the scientific enterprise does or can replication occur*? One way to look at this new question is as a version of the (X-,Y+) interpretation of Collins' cases. Even if Collins is right that individual scientists do not replicate experiments, that may show only that replication is not the sort of thing that *individual scientists* do. For example, replication may be a collective unintended consequence. Priority concerns typically make scientists quite secretive in their dealings with colleagues, and the resulting lack of communication may be the main source of multiple discoveries (Brannigan and Wanner 1983). From a policy standpoint, selfish considerations may apparently lead to a wasteful duplication of scientific effort. Yet this "wasteful duplication" enables an unwitting replication of discoveries.

This take on the issue recalls our previous discussion of the original context in which "ought implies can" was made. Currently, the slogan implies that it is unreasonable to compel people to do something that is not within their power to do. In that case, a would-be norm may be invalidated simply by showing that the norm is not humanly realizable. However, Kant first argued "ought implies can" to quite different effect. Kant wanted to show that if we have principled grounds for believing that a certain course of action is the one we ought to pursue, then there must be some faculty (indeed one we may have yet to discover) that enables us to do it. On this basis, Kant claimed that there must be a special "noumenal" aspect to our being—a "rational will"—that is subject to the moral order since, clearly, the ordinary physical aspect of our being is swayed amorally by the passions. While perhaps striking the modern reader as perverse, Kant's reasoning nevertheless serves to underscore the inherent ambiguity in using "human realizability" as a constraint on the acceptability of norms. Yet the ambiguity is not an unhappy one. Experimental psychologists, for instance, have shown that *individuals* are cognitively ill disposed to follow virtually every norm that has been proposed for rational inference in economic and scientific matters. Taking a cue from Kant, instead of scrapping all the proposed norms as just so many falsified hypotheses, and thereby concluding that "man [*sic*] is an irrational animal" (Stich 1985), we may need to turn from the individual to other "units of rationality." For example, norms may have more bite as sketches for computer programs or as blueprints for the organization of cognitive labor (Fuller 1989: Chaps. 2-3). Likewise, the policymaker needs to broaden her imagination as to what might count as humanly realizable.

Some Kant-intoxicated philosophers claim the existence of "unconditional" norms—norms that bind people in all situations, no matter their ends, and even if the immediate consequences are not particularly salutary. These norms are *categorical* imperatives as opposed to the condition-bound *hypothetical* imperatives we have been discussing so far. But do such things exist? If norms govern the activities of real people, then shouldn't the norms reflect differences in people's situations, which means that all norms will be hypothetical imperatives? No, at least if there is more to a norm than merely a strategy that regularly gets you what you want. As an act of legislative will, a norm is designed to govern an entire community such that one's status in the community does not affect the norm's efficacy. This idea is the signature modern method for deriving principles of justice, immortalized by John Rawls (1972) as the "veil of ignorance" from which one operates in the "original position" of constitution-making. Apparently, then, an important goal of any normative inquiry is to sort out the categorical from the hypothetical imperatives: Which courses of action can be recommended to anyone no matter what others do? Which can be recommended only after a survey of what others are doing?

But wouldn't the prior exercise involve more than merely sorting imperatives? Wouldn't the normative inquirer be compelled to issue norms of her own? These questions are especially controversial when applied to science. Most of the hypothetical imperatives that philosophers invoke as "rational criteria for theory choice" emerged without legislation as individual scientists took advantage of situations that they realized would remain unexplored by most of their fellows. Surprisingly, historians of science have said little about this self-selection process. One possible reason for the self-selection has to do with the nature of theorizing itself—at least the sort of theorizing discussed here, which Popper believes would engulf science in a "permanent revolution" if often practiced. Revolutionary theorizing of this sort is defended in this book. Such theorizing can reconfigure entire fields of inquiry by dialectically overcoming existing disciplinary differences. The import of successful theorizing in this sense—as in the cases of Newton, Darwin, Marx, and Freud—is to reorient the research of one's colleagues, and perhaps even to threaten their livelihoods altogether, if they are unable to adapt to the proposed change in milieu.

Philosophers often forget that scientists are generally taught to "theorize" only in the Platonic sense of constructing abstract mathematical models, but *not* in the more Hegelian sense of attempting a dialectical synthesis. Moreover, the typical context in which a scientist encounters a theory is the textbook. Textbooks present theory not as a challenge to the current disciplinary order, but as a safeguard against posing such a challenge. Theory appears as a glorified mnemonic device for keeping seemingly disparate notions related in the student's mind. In short, the incentive to theorize—in the synoptic sense that philosophers have traditionally thought to be essential for the growth of knowledge—has never been explicitly built into the normative structure of science. Theorizing is, of course, not prohibited, but it is definitely a risky venture professionally: The payoffs of success are big (for both the science and the scientist), but few succeed, and hence few try. But what would be the benefit of eliminating this risk by elevating the search for explanatory theories to a categorical imperative of science?

MACHIAVELLI REDUX?

Can all this talk of legislating and experimenting with the normative structure of science ultimately avoid the charge of manipulation? Manipulation typically presupposes a world in which the manipulable have well-defined interests against which the manipulator then imperceptibly acts. However, as indicated at the start of this chapter, I do not believe that science has any such "internally" or "autonomously" defined interests. Therefore, I deny the presupposition that underlies the morally repugnant sense of manipulation. Moreover, manipulation generally requires that knowledge be asymmetrically distributed across society. Hence, a specific group can always alter the structure of knowledge production, whereas everyone else is a passive recipient of its products. Social epistemology aims to break down the distinction between production and distribution that enables the morally repugnant sense of manipulation to take root in society.

Lest the reader find my denial of Machiavellianism too glib, let me now take up the charge in more detail.

Suppose that each hypothetical imperative associated with the history of science were shown to capture an effect that is emergent on the scientific labor being divided and organized in a certain way. Thus, no particular scientist would be explicitly guided by the imperative. However, the imperative offers the best explanation of what implicitly governs their collective behavior. If the policymaker is interested in maintaining the production of such effects, then she will be forced to gauge the advice she gives to individual scientists in terms of the likelihood that their subsequent actions will contribute to producing the desired effects. In Machiavellian short form, the ends will justify the means that the policymaker selects. Is this line of reasoning objectionably manipulative?

In rough-and-ready terms, manipulation occurs when one person knowingly gets another person to do something unknowingly that goes against her own interest, but benefits the first person's interests (Goodin 1980). Clearly, the knowledge policy strategy advanced in this chapter satisfies some of these criteria: The policymaker's bird's-eye view of the scientific process gives her an advantage over the average scientist in determining the overall significance of the scientist’s work. Given the broader scope of societal aims within which science policy must be made, the policymaker's interest is arguably somewhat different from that of the average scientist. Indeed, the scientist is being made to serve the policymaker's interests. Conspicuously absent, however, is the idea that the policymaker wants the scientist to do something not merely different from, but demonstrably *against*, the scientist's own interests.

Now admittedly a virulent antipolicy tradition exists within the scientific community (Polanyi 1957). This tradition would blame all the deleterious consequences of scientific research on meddlesome policymakers who force scientists to act against their better judgment by making funding hostage to the production of ideologically sanctioned knowledge. Although such cases of deleterious consequences are all too familiar (e.g. Lysenkoism, Nazi genetics, the atomic bomb project), what remains unclear is whether an alternative research trajectory had been inhibited that the scientific community would have pursued left to their own devices. More likely is that some other policy imperative would have given direction to scientific research. If the policy were socially beneficial or neutral, the result would be credited to the “autonomy” of science. But if equally deleterious, the result would be laid at the doorstep of meddling policymakers. These frankly counterfactual speculations make one wonder whether the scientist has any specifically *scientific* interests that are different from those of the policymaker or whether differences in interests are entirely nonscientific in nature (e.g. a scientist's interest in receiving a bigger cut of the available funds). As a natural trajectory to scientific research, independent of policy considerations, becomes harder to identify, the potential diminishes for policymakers to be objectionably manipulative.

But let's reverse the burden of proof: Why would anyone have thought that scientists had a distinct set of interests that could be disentangled from broader social interests and, especially, from the policymaker's interests? After all the idea of "interest" is an anthropomorphism that implies that events in the world neatly correspond to outcomes having different values for rival groups. The world, of course, is not so willing to oblige our efforts at totemism. According to the Doctrine of Double Effect, a self-interested course of action will be received by others who do not share our interests. Yet science policy enables scientists to ignore this elementary point by indulging a deep-seated psychological bias that would not normally be tolerated in other less esteemed groups. The most striking case of this bias is what social psychologists call *the fundamental attribution error*, which explains the asymmetry in the stories we tell about ourselves vis-à-vis those we tell about others (Hewstone 1989: Chap. 3).

We tend to explain the good things that happen to us in terms of enduring ("internal") personality traits and the bad things in terms of ("external") situational accidents. In contrast, we tend to explain what happens to others in reverse (i.e. people fail because of fatal flaws in their character and succeed out of sheer luck). Internalism, then, seems to be integral to the construction of self-identity. In that case, a good way to identify the dominant perspective—the "hegemonic authority"—in a society may be by whose self-identity story is presumed by all. Clearly, if all classes of people are susceptible to the fundamental attribution error, then social coherence can be attained only by privileging some of the asymmetrical accounts of self versus others at the expense of other such accounts. Thus, much of contemporary science policy can be readily seen as privileging the scientific community's commission of the error.

The fundamental attribution error also fosters the illusion that one's self-interest can be discovered by finding a stable personality trace. Yet if referring to anything at all, "interests" refers to utilities that exist outside oneself, cognitive access to which is likely to be no better than to any other external object (Goodin 1990). Indeed, even the interest groups that one identifies with at the beginning of a course of action may not be the interest groups with which one identifies later on, once some consequences of that action have been revealed. This point can then be used to get scientists to realize that as policy is projected into the indefinite future, one's own interests become less distinct from those of others. For example, as a nuclear physicist I may want unlimited funding for my field. But my endorsement of this policy assumes that, at the end of the funding, I still plan to be in nuclear physics and continue to identify with the community that will be receiving the funds at that time. As the quickening pace of scientific change forces a perpetual turnover of specialties, the odds that this will be the case—even without any government directive—diminishes.

Hence, from a purely "self-interested" standpoint, a more rational way to act is not to harm others in the course of benefiting those who are hypothesized as one's own successors (Parfit 1984).

With this in mind, policymakers can be interpreted as manipulative in a way that benefits the scientific enterprise—namely, by counteracting two sorts of nonscientific interests that scientists *themselves* possess:

1. The tendency to see one's own research as the very center of all that is worthwhile in science;

2. The tendency to satisfy the norms of science with as little effort as possible.

Hobbes would have recognized these two interests in the inhabitants of the state of nature and rightly diagnosed them as being born equally of ignorance as desire. In the case of (1), the policymaker arranges funding patterns so as to force scientists to think of their research as parts of larger projects, the realization of which may exceed the cognitive grasp of any of the participating scientists. This policy scenario is most prevalent in attempts to bring multidisciplinary perspectives to bear on pressing but ill-defined social problems. More modestly, the strategy may also be applied to consolidate the knowledge base of a discipline whose research has become highly fragmented through specialization (Fuller 1988a: Chap. 12). In the case of (2), the policymaker designs accountability procedures that force scientists to endure various probative burdens before being licensed to claim a cognitive achievement as their own.

Depending on how much others are expected to rely on the putative achievement, the probative burdens may be as light as simply reporting that one has carried out the appropriate procedures. The burdens may be as heavy as sustaining the scrutiny of other researchers with a vested interest in debunking the achievement or claiming it for themselves (Fuller 1988a: Chap. 4). If policy intervention ensures, so to speak, the productivity levels of science in (1), it ensures quality control in (2).

The trickiest area where STS may have policy relevance concerns the image that scientists have of themselves and their pursuits. STS research has earned its scandalous reputation by revealing discrepancies between scientists' words and deeds. Thus, scientists may appear to be laboring under some sort of false consciousness. Although this belief could well motivate the STS researcher to intervene in scientific practice, imputations of false consciousness to scientists are unlikely to motivate a change in behavior. Indeed, if the large-scale success of science is granted, then at least some forms of false consciousness would seem to have much to recommend them. Consider two hypothetical imperatives in this vein:

A. If science is to enable prediction and control over larger portions of the environment, then scientists had best think of what they are doing as probing ever deeper levels of reality (and not simply as applying craftier techniques).

B. If science is to produce ever more policy-relevant consequences, then scientists had best think of themselves as autonomous inquirers (and not as high-paid civil servants).

Suppose that these two imperatives were shown to work. Would that license a redoubled effort to educate fledgling scientists—and maybe even the public—in the myths of the profession? Here the issue of science policy as *ideological* manipulation is raised with a vengeance. The social psychology of creativity offers some clues as to how to treat this matter (Amabile 1983). Creativity is tied to a strong sense of one's work as "intrinsically motivated." Interestingly, this finding is typically evidenced in the subjects' accounts for their activities. What exactly the expression "intrinsically motivated" picks out in a person's behavior remains open as each person demarcates "internal" from "external" factors differently. Still the suggestion is that the criterion for demarcating these factors can be renegotiated as scientists come to "internalize" political economy and social accountability as part of their motivational structure. However, this process will not be easy, especially as long as scientific language remains autonomous from the greater society.

A RECAP ON VALUES AS A PRELUDE TO POLITICS

How does the social epistemologist propose to mobilize STS research to alert policymakers to alternative strategies for funding and evaluating research? In a nutshell, by shaking policymakers from their unreflective stance of presuming that the "is" and the "ought," facts and values, are fused together in some "implicit norms" or "natural trajectory" of knowledge production. The social epistemologist would aim for policymakers to reject equation of *statistically normal behavior* and *normatively desirable action*. The policymaker would have come to realize that the social construction of facts and that of values pull in opposing directions. Thus, any perceived sense of "normalcy" is only a temporary resolution of this tension.

No doubt the image that my answer initially evokes is that of each policymaker negotiating in her own mind (or for her own jurisdiction) which claims will be treated empirically and which normatively. Such a process would lead to a multiplicity of independent decisions resulting in incommensurable sensibilities about where the fact-value distinction should be drawn. Admittedly, the localistic bias of much constructivist STS literature suggests such a conclusion. Any piece of research can probably figure in any sort of value scheme as disparately interested parties find use for the research. In that regard, research, while not value-neutral, may be inherently *value-indiscriminate.*

Perhaps the best way to dispel this lingering image of value-neutrality is for the social epistemologist to observe that if *values* are generally defined as "the sphere of freedom" and *facts* as "the sphere of resistance," then one person's treatment of a claim as normative may turn out to cause another person to treat that same claim as empirical. The crucial difference is that a decision taken by the first person appears as a brute fact to the second person. An asymmetry of this sort would thus define a *power relation* between the two persons.

THOUGHT QUESTIONS

☙ What does Fuller mean in stating: "Indeed a refusal to steer the course of science policy is itself a very potent form of science policy"? Is science largely an autonomous, self-correcting enterprise that does not require outside management? How are scientists able to maintain the apparent internal and external boundaries of science?

☙ What is "knowledge policy"? What are the similarities and differences between knowledge policy and science policy? How might social epistemologists convince policymakers that they do not know "enough about the production and distribution of knowledge to make intelligent decisions"?

☙ What is the knowledge society? What is the role of policymakers in the knowledge society? What is the folk wisdom that guides science policymakers? What are the problems that arise from this folk wisdom?

☙ What is "Fast Science"? How does Fast Science reward research and devalue teaching? What is responsible for the "inertial character" of science policy? How does science policy support or counter the aims of Fast Science?

☙ How are the aims of journalism akin to or different from constructivism? What role might science journalism play in a social epistemology? How does journalism feed the public's appetite for Fast Science? How might this public attitude affect the processes comprising a social epistemology?

☙ Can science be managed as a business is managed? What is the difference between "basic" and "applied" research? What is the rhetorical dilemma for managing science if we hold this distinction? How do scientists take rhetorical advantage of the basic-applied distinction? What rhetoric is used to distinguish Big Science from Big Business?

☙ Is there such a thing as "scientific creativity"? Is there such a thing as "scientific genius"? If so, can creativity and genius in this sense be managed? If not, then is creativity or genius a socially conferred appellation?

☙ How are the consequences of basic and applied research posed rhetorically? What are the aims of this rhetoric? How might one judge and change this rhetoric? Can scientists be held accountable for the intended and unintended consequences of their work? How might one trace ultimate accountability?

☙ How might a "social construction of facts and values" lend direction to science policymaking? To what scripts do scientists, philosophers, and sociologists refer in describing scientific processes? How do social constructivists tend to interpret these scripts? What is indexicality? What difficulties confront social constructivists in moving from an observed local stage of science to a global stage on which policy can be made? How do Fuller's explanations of knowledge production that attempts to "respect both local and global sensibilities" offer insights into scientific decision-making processes that can be used to set policy?

☙ In determining science policy, why is it important to distinguish between a scientist's beliefs and desires? How might this distinction serve to direct policy based on norms of science such as experimental replication? What is the fact-value distinction? How does this distinction compare rhetorically to the distinction between beliefs and desires? How can the fact-value distinction be used in a grid-group analysis of policymaking, generally and science policymaking specifically?

☙ What does Fuller mean by the "fact-laden character of value commitments" as opposed to the "value-laden character of facts"? How is the normative view that Fuller proposes "logically prior" to pragmatism? Which aspects of the scientific enterprise does the pragmatist miss and normative theorist recover? What is the difference between a "hypothetical imperative" and "categorical imperative"?

☙ What role does theorizing play in science? How is theorizing a different activity in the social science? What limits might a social epistemologist, in crafting a knowledge policy, place on scientific theorizing?

☙ How is a normative approach to legislating the production of knowledge immune from charges of gross manipulation? What rhetorical difficulties does a knowledge policymaker face in invoking a normative stance? What are scientists' attitudes themselves and their pursuits? What are scientists' attitudes toward policymaking? What are policymakers' attitudes toward themselves and their pursuits? What are policymakers' attitudes toward science? How can these attitudes be resolved in to construct a policy leading to normatively acceptable action?

8

Knowledge Politics: What Position Shall I Play?

PHILOSOPHY AS PROTOPOLITICS

Philosophy qua philosophy is protopolitics. At its best, philosophy is at once partisan and nonpartisan. But philosophy stands opposed to the unreflective modes of understanding at a given time and place—no matter how normal, acceptable, or even exemplary—if people are prevented from seeing issues of mutual concern. Indeed half of a philosopher's problem is that her interlocutor does not already see the problem. This core philosophical attitude has been carried into our own day—albeit in ideologically opposed ways—by both the Popperian and Marxist traditions (Adorno 1976). Neither tradition has been especially moved by the *argument from repair*: "If it ain't broke, don't fix it." Whereas the Sophists intervened on behalf of someone who had already perceived a problem (much as a lawyer would today), Socrates endeavored to make people see problems in aspects of their thinking that they would normally treat as unproblematic. He typically launched his inquiry by persuading his interlocutor that seemingly isolated problems of judgment and action, the existence of which the interlocutor would easily admit, were really symptomatic of the same deep conceptual disorder.

The expression "seemingly isolated," used earlier, signals that what the normative inquirer wants to specifically challenge is a *frame of reference*, a *perspective*. The problem, then, is how to get people to see things from a "better" perspective. The scare quotes around "better" immediately signal that we have a rhetorical problem on our hands. The sort of perspective that a philosopher is likely to consider "better" is one that her interlocutor would probably see as "better" only once she has adopted it. Before then, this viewpoint seems like an arbitrary imposition of philosophical will—a challenge to the special scientist's disciplinary turf. This challenge is simultaneously a normative and a rhetorical problem because we need to be clear as to *whose* frame of reference we are trying to change for the better. This point is addressed further in Chapter 10, but for now let us consider a feature of this problem that has perennially led philosophers to monger a particular class of norms known as *methods*.

In the cases where our behavior is hardest to change, yet the incentive for doing so is great, we often already know the right thing to do. That is, we have *merely intellectual* knowledge of the right way of seeing things. For example, we can nod sagely and discourse volubly about the importance of class, status, and power in determining what scientists do, but we still intuitively evaluate science as if there were nothing more to it than a bunch of individuals running around in laboratories. (The acuteness of this problem can be seen whenever someone says the social character of science is "true but trivial.") Common solutions to this problem include raising the underlying reality to the level of appearances (or, what amounts to the same thing, giving some perceptual embodiment to our intellectual understanding) or, better still, enabling us to intuit what we can now only infer with great difficulty. A method lends a way to discount and reinterprete our natural forms of experience so as to arrive at the prescribed way of seeing things. For Descartes, a method was essentially a verbal recipe for changing your mind. But for those less sanguine about the mind's native capacity to correct itself, experimental intervention has been the preferred methodological route. Accordingly, one restructures the environment, usually by introducing controls and eliminating distractions, so that the effects of the hypothesized causes can be seen.

However, philosophers often forget that specifically Socratic intervention requires conversation and, thus, the verbal collaboration of those whose minds we would change. Philosophers are typically quite adept at persuading their colleagues that some benighted group of social or natural scientists need to change their ways as stipulated by the "canons of rationality" or whatever authoritative name the norms are given. Yet the unsuspecting target population of scientists remain out of earshot as objects of philosophical gossip. These rhetorical misfirings are no mere tactical blunders, but grounds for concluding that the proposed norms are "invalid." The standard of validity I am invoking is taken from psychoanalysis in which the patient's acceptance of the analyst's account is a necessary (although not sufficient) condition for the adequacy of that account. Once adopted as the patient's own, the account can then serve as the touchstone for recovery. By analogy, the philosopher of science should not propose norms that would improve the conduct of science *if* scientists were to follow them. Rather, she should propose norms that would likely gain the consent of scientists and *thereby* improve the conduct of science—even if in ways that the scientists had not anticipated.

So far I have characterized philosophy *at its best*—not necessarily as it is normally practiced. The second half of the 20th century has been marked by philosophy's steady withdrawal from the sensitive business of advising people on what they ought to do. This retreat from prescription has taken two forms.

One line of normative retreat can be detected in the work of some latter-day pragmatists, followers of the later Wittgenstein, Heidegger, and even Habermas' theory of communicative competence. The reasoning is as follows: As the relevant norms are already implicit in what we normally do, we should simply alter the way we understand our practices so that their normative structure becomes more apparent, which will then enable us to correct the few isolated infractions that remain. In this context, norms are often said to be "immanent," which means that we could not get rid of them even if we wished. This born-again stoicism reveals an "adaptive preference formation," the social psychologist's way of diagnosing the attitude of "sour grapes" (Elster 1983).

The second line of normative retreat is associated with analytic philosophy's turn to "meta" issues in ethics and epistemology in the wake of legal and logical positivism. In this case, the concern is with identifying the distinctly "normlike" feature of norms. Therefore, norms end up being severed from every other aspect of human practice (MacIntyre 1984). The resulting conception is of norms that can evaluate from afar but offer little by way of guidance for local improvement. Politically speaking, philosophers following this line of retreat are like the utopian socialists whom Marx condemned for espousing an idly "transcendent" radicalism.

In short, normative retreat of the first sort undercuts the possibility for a normative inquiry with radical import. Normative retreat of the second sort subverts the practical thrust of such an inquiry—the Scylla of Mannheim's (1936) *ideology* and the Charybdis of his *utopia*.

Hence, the prognosis for a politics of knowledge looks dim. But maybe in our own day, to recall Hegel's image of philosophy, the Owl of Minerva no longer takes flight at dusk because its soul has transmigrated to a more evolved species. For currently, questions of the scope and urgency associated with Socratic inquiry are identified in what prima facie appear to be very *un*philosophical terms. Specifically, these questions are seen as ones of resource allocation and management often attached to a strong "ecological" or "global" orientation. Unfortunately, politically inspired, empirically informed calls to global consciousness have inherited both philosophy's soul and its rhetorical incapacities. A vivid case in point is the fate of Ehrlich's (1978) best-selling book on overpopulation which suggests an affirmative action to the question addressed in the next section: *Have science and democracy outgrown each other?*

Waddell (1994) dubs Ehrlich "a modern Cassandra" who engages in the *preemptive contempt* of his audience by confessing at the outset just how difficult it will be to convert them to his position. Thus, Ehrlich reminds the reader of the biases toward shortsightedness that evolution has built into the human hardware. The effect is to convey, "You are probably prejudiced against me, but if it so happens that you can rise above your prejudices, here is what I have to say" In prescribing for our future survival (the utopian vision), Ehrlich casts his gaze above the heads of his audience (the metalevel condescension). One suspects the arrogance of privileged insight is combined with an expectation of failure and maybe even humiliation. Perhaps this self-defeating strategy arises when one knows too much for one's own good, and so feels confident in second-guessing the audience's negative response. The audience is indeed provoked, but polarized as well. Those readers already sympathetic to Ehrlich's case will leave more sanctimonious than ever as they have clearly overcome their biased biological hardware. But those readers who came opposed or indifferent will leave feeling dismissed and downtrodden—hardly the most auspicious preludes to constructive action. The phenomenon of preemptive contempt thus offers a glimpse at the ambivalent relations between science and democracy, experts and the public. Let us now take a longer look.

HAVE SCIENCE AND DEMOCRACY OUTGROWN EACH OTHER?

Is science compatible with democracy? The classical, modernist, Enlightenment answer is "yes" because "science" and "democracy" are simply alternative ways to identify what Karl Popper suggestively called "the open society." The more abstractly we conceive of science and democracy, the more plausible Popper's line seems. In effect, the idea of the open society invites us to think of epistemically and politically desirable states as involving no tradeoffs—as one increases, so too does the other. However, once we try to make this point explicit, doubt begins to set in: Is *advanced* science really compatible with *maximum* democracy? The distinction drawn between *plebiscience* and *prolescience* (in the Introduction) implies the answer is "No": Once we try to operationalize science and democracy as decision-making processes, we find that the two states vary inversely with one another. What accounts for this reversal, and can it be remedied?

In what follows, I address the pervious question dialectically. In briefly presenting Popper's "straight" conception of the open society, I reveal and diagnose its conceptual instability. Next, I reformulate the relation between science and democracy not only by taking this diagnosis into account, but also by demonstrating an ironic sense in which science is compatible with democracy. This sense is suited for a "postmodernist" understanding of science and democracy. However, on closer examination, this postmodernist rapprochement will also be shown wanting, which will in turn reveal that the idea of "liberalism" is no less contestable within democratic theory than "knowledge" is within theory of science.

Popper (1950) notoriously excavated the roots of totalitarian thinking in idealist philosophy, singling out Plato and Hegel for critical scrutiny. These roots confer legitimacy on "closed societies." Closed societies operate by the principle of institutional inertia. This principle assumes that social order is not natural, but must be imposed, and that whatever order has been imposed ought to be maintained. In contrast, "open societies" accept human fallibility as the perennially legitimate grounds for challenging any standing set of beliefs. Such fallibility is the basis of human equality, which, therefore, empowers democratic forms of government. Ironically befitting a Hegelian scenario, Popper follows John Stuart Mill and other liberals in suggesting that the open society begins from within the closed society as science emerges as an island of free inquiry, which is destined to transform the entire closed society into an open one. This image led the American sociologist Robert Merton (1973: Chap. 13) to identify the normative structure of science with the regulative principles toward which modern democratic societies more generally strive: "communalism," "universalism," "organized skepticism," and "disinterestedness." The vehicle by which society is rendered more scientific is something called education. Yet the question remains of how much education the average citizen needs to have before science can safely open its doors to full social scrutiny.

Answering this question points to the instabil ity of th e open society ideal by revealing the two polar directions in which philosophical thinking has gone: *Left Popperian* and *Right Popperian*. Paul Feyerabend (1975, 1979) represents the Left Popperian response. Believing that the time is long overdue for science to be made the subject of complete public accountability, he clearly has in mind the forum of classical Athens, in which any citizen could raise any objection to any proposition on the floor for debate. Feyerabend sees the democratization of science as simply the reflexive application of the scientific ethos of free inquiry to science. STS researchers, who have observed the ability of scientists to account for their activities when pressed by nonscientists, reach the same conclusions by empirical means. Although scientists may be inconvenienced in making sense of their activities to a larger audience, they are not precluded from doing so merely because of their work. In contrast, Michael Polanyi (1957, 1969) represents the Right Popperian response. He argues that we are still in the early stages of scientific enlightenment, and that science budgets still need to be protected from public scrutiny because, in its rage for quick fixes, the public is likely to pervert the spontaneous course of scientific development.

The feasibility of the various visions of the open society—Popper's, Feyerabend's, Polanyi's—is relative to the *scale* in which science is done. Specifically, the larger the society and the more extensive the scientific networks in that society, the less plausible any of the arguments for the open society will seem.

Popper's vaunted method of conjectures and refutations works only in small intimate groups, such as research teams, whose members have earned the mutual respect that enables the free flow of making and taking criticism. To ensure the prompt feedback that is necessary for one to benefit from criticism, the mode of interaction should be face to face. Brainstorming, which comes to mind as an activity that exemplifies the qualities of a Popperian open society, entails a social structure quite uncharacteristic of the signature features of modern Big Science. For example, advances in electronic and print media have increased the opportunities for exchanging ideas. One can now, more than ever, incorporate the work of others for one's own purposes. Yet the intellectual contagion spawned by these networks makes the identification, diagnosis, and correction of error more difficult than ever (Shrum and Morris 1990). In short, a *tradeoff* may exist between the free flow of information and the feasibility of rational criticism. (On a smaller scale, the introduction of the printing press to Europe in the 15th century not only enabled the cumulative growth of knowledge, but *also* permitted an unprecedented diffusion of rumor and superstition (Febvre 1982).

Feyerabend's vehement opposition to Big Science presupposes an awareness of scale in a way that Popper's conception does not. Feyerabend follows Rousseau, and the anarchistic-libertarian tradition, in arguing that participatory democracy can flourish only in societies whose homogeneity enables agreement on fundamental value issues so that any other differences will be freely tolerated. Such societies must inevitably be small as those who fail to agree on the fundamentals will be encouraged to form their own society. Sponsoring its own science, each society, because of its size, will be unlikely to interfere with the well-being of people who remain uninvolved. However, Big Science's threat to modern society results from being both beneficiary and protector of an artificially inflated state apparatus that extracts revenue from vast numbers of people for projects about which they are never consulted. An invitingly radical way to read Feyerabend is that science, as the quest for knowledge, *disappeared* on outgrowing the dimensions capable of sustaining participatory democracy. Unfortunately, our models for thinking about what has taken its place have yet to catch up with this reality. This point, regardless of what one makes of the overall Feyerabendian position, offers much food for thought.

No one could be more opposed to this sentiment than Polanyi. However, once we factor the dimensions of Big Science, the prognosis for Polanyi's picture is not particularly pretty. If science is an open society only insofar as both the means and the ends of inquiry are "free," then the long-range forecast is that increasingly technical equipment will be focused on increasingly specialized issues whose overall relevance to society will remain obscure for longer periods. Over the long term, assuming that we do not live in a world of infinitely taxable wealth, ever smaller numbers of people will be allowed to participate in the scientific enterprise. The start-up costs for a lab alone will price virtually everyone out of the market. To be sure, science will engulf all of society in its maintenance operations as everyone slaves away just to enable a diminishing few to enjoy the sustained luxury of conducting inquiry. This sustained luxury is essentially *the freedom to waste resources.*

The tenor of this discussion of the open society's sensitivity to scale is meant to suggest that science may have expanded to the point of being unrecognizable. But what becomes of *democracy* from these changes in scale? Must we be ruled by experts in a scaled-up democracy, or rather must the process of "justifying" knowledge claims change? In support of the latter, the rhetorician Charles Arthur Willard (1996) explicitly challenged the relevance of epistemology to the evaluation of knowledge claims in this era of Big Science. I have reconstructed his argument in the following four steps:

> 1. Given the number of knowledge claims currently made and the differences in background knowledge that the claims presuppose, it is simply no longer rational to try to follow the epistemologist's advice of evaluating each claim on its own merits.
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> 2. Under these circumstances, rule by expert authority becomes *prima facie* attractive. However, whether "experts" are competent in the area where we might want their advice is unclear. Thus, the "public sphere" is populated by ill-defined social problems. *The impossibility of expertise in the public sphere is the best epistemic argument for democracy.*
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> 3. Yet how does the public decide on which expert to believe—and to what extent? The answer lies in a kind of social epistemology that Willard calls *epistemics*. It uncovers the sociopolitical networks that enable someone to command "expert" status on a given issue at a given moment. Because there are no true experts in the public sphere, the public must judge avowed experts by the networks that sustain them: Who benefits and who loses by following this expert's advice?
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> 4. Consequently, the impossibility of making rational judgments about individual knowledge claims implies *not* paralyzing skepticism, but rather that the objects of our judgments must be something other than we originally thought. After all, a public crisis does not disappear just because we are unable to find secure epistemological foundations for our judgments. Epistemics, then, is the field that identifies the pragmatic successor to an epistemology that has outlived its usefulness in the modern world.

Willard's reasoning presupposes clearly distinguishable networks of interests exist that correspond to the epistemic options from among which society must decide. If, however, networks intertwine so that following the advice of one expert rather than another does not guarantee significantly different outcomes, then the very point of having a choice—the essence of participatory democracy—gets called into question. This is a real possibility. Even if reality is a social construction, that a society's members can determine which of many possible worlds they end up co-producing does not follow. Analogously, saying that because officeholders in a democracy are elected by the people does not warrant the inference that the people always elect the candidate they want. This inference confuses the fact that each individual's vote contributes to the election's outcome with the fact that the outcome need not conform to any given individual's expectations—or preferences for that matter (especially if individuals try to vote "strategically" based on a false understanding of what their fellows think). In Sartrean terms, Willard speaks the language of *praxis*, the "socially real" (as it were) in which each possible course of action carries a distinct social meaning. But we must not lose sight of the *practico-inert*, the "really social." In this case, the material residue of social practices often unwittingly undermines the differences in meaning that those practices try to establish (cf. Sartre 1976). An excellent example of the practico-inert getting the better of praxis is afforded by the history of intelligence testing.

At the end of the 19th century, Alfred Binet developed the IQ test as a method uniquely suited for French educational reform. Once able to determine whether a child's cognitive achievement fell below the norm for the child's age, the teacher could then design an appropriate course of study that would enable the child to catch up. No longer (so thought Binet) would slow learners be written off as intractably stupid. Yet throughout the 20th century, the IQ test has been used by many educational interest groups, most of which have been diametrically opposed to Binet's. As an unintended consequence of being used for multiple purposes, the IQ test has metamorphosed from a tool to an object of study in its own right (a branch of psychometrics), with standards of interpretation that can be used to decide between the knowledge claims of competing educational interests (Gould 1981). For example, arguments concerning the racial component in intelligence are typically adjudicated by seeing whether the difference in IQ test means for, say, Blacks and Whites is statistically significant. These developments in the "objectification" of intelligence testing stray far from Binet's original intentions, perhaps even to a point that would make him regret ever having introduced the concept of IQ. Thus, "objectivity" in the scientist's sense triumphs in spite of (indeed *because of*) the efforts of locally oriented agents. The tale is one familiar to sociologists: A means designed for a particular pursuit unwittingly becomes the standard by which the success of all pursuits is judged. Such means-ends reversals extend beyond the IQ test to the general alienation of knowledge under intellectual property law (Fuller 2002a: Chap. 2).

How should our normative sensibilities respond to this story—acceptance or outrage? Acceptance evinces the doubly ironic sense in which postmodernist philosophers of science like Richard Rorty (1989) and Jean-François Lyotard (1983) see us as already living in the open society. Yes, our conception of science is fragmented, but that enables the products of science to circulate freely in the marketplace. And yes, our conception of democracy is equally fragmented, but that prevents any one faction from monopolizing the marketplace. The politics of knowledge so ironized captures a certain image of the open society, the "laissez-faire" side of the postmodernist's liberalism. At the same time, this noninterventionist tendency is much more in line with the spirit of Rorty and Lyotard's wanting to, as Wittgenstein would say, "leave the world alone."

> *Equal-in-Principle Liberalism*: Since all viewpoints are "created equal," in the sense that none has any *a priori* advantage over the rest, whatever historical success particular viewpoints have will come as a result of having adapted to contingencies in the marketplace of ideas.

Like the more strictly political versions of laissez-faire liberalism, this one extends a false hope as suggested in the ambiguity of the term *adapted*. The Lamarckian sense of "adapt" implies that individuals can intentionally adapt to their circumstances. This Lamarckian sensibility is implicit in postmodernist descriptions of agents as trying to capture the spirit of the time and, thereby, maximize their own advantage. If the agents are properly attuned to the sorts of arguments that will persuade their intended audiences, then they are likely to succeed. However, the second, more Darwinian, sense of "adapt" is not nearly so sanguine. Darwinian sensibility insists that what turns out to have made various individuals either adaptive or maladaptive to their intellectual environments is an unpredictable and emergent feature of their collective activities. Thus, the IQ test began by enhancing Binet's instrumentalist views on education, but became an even more potent weapon in the hands of his foes, the racialists. Moreover, in the process, the statistical trappings of the IQ test became part of the standard by which any theory of intelligence is judged.

Yet a "welfare state" side to the postmodernist's liberalism exists that defines a rich culture as one rich in viewpoints. Consider these two variants:

> *Equal-Time Liberalism*: All parties to a conversation should *always* be on an equal footing, no matter what actually transpires during the conversation, including a radical change in the attitudes that the parties have to one another's views.

> *Separate-but-Equal Liberalism*: Because a viewpoint is valid for the culture from which it arose but invalid (or at least inappropriate) for any other culture, it follows that all cultural viewpoints should be protected from outside interference.

These two versions of liberalism effectively translate epistemic relativism into political terms. They are expressions of Left Popperianism. They share a commitment to egalitarianism that is sufficiently strong to justify the application of force to prevent equality from disintegrating under either (in the first case) the emergence of a dominant voice or (in the second case) cultural imperialism. The implication, of course, is that such disintegration would *naturally* occur without the liberal's intervention. Thus, if the postmodernist is a liberal in either of the prior two senses, her social policy is given to a certain amount of artifice. An example of the equal-time case, supported by someone like Feyerabend, would be to compensate for the meager attention that astrology has received in the recent past. To do so requires allocating astrology a disproportionally larger share of attention in the future. An example of the separate-but-equal case would be to ghettoize knowledge production into mutually exclusive domains of inquiry. Thus, instituting technical terminology and departmental bureaucracies is how academic disciplines normally coexist in the university.

These two species of liberalism propose to enforce some sort of cognitive egalitarianism. As such they suffer from the ancient conundrum of trying to maintain the equality of things (in this case, forms of knowledge) that embody seemingly incommensurable values. Seen through Aristotelian spectacles, the *equal-time* and the *separate-but-equal* cases reproduce, respectively, the quandaries of *commutative* and *distributive* justice.

In the equal-time case, we are interested in redressing an earlier injustice between, say, astrology and astronomy. Seemingly, the desired end state is the *literal* availability of air time for the two disciplines as measured by courses given, journals published, and the like. However, if the audience for this air time includes the general public, then arguably one or the other discipline may be advantaged in terms of receptiveness and background knowledge. In that case, granting more exposure to the less advantaged discipline would make sense for the two to have, so to speak, "comparable epistemic effects." But what would such effects look like? *How plausible* would *how much* of each discipline need to appear?

A similar problem arises in the separate-but-equal case once we consider that astronomy and astrology do not utilize air time in the same way. Contrast the two disciplines with regard to the nature and need for communication among practitioners and special research environments. In Aristotelian terms, distributive justice here would demand that unequals be treated unequally. Assuming, then, that astronomy is a much more technology-intensive field than astrology (e.g. advances in telescopes make a difference to the former's knowledge base that they do not to the latter's), astronomy could receive five times the funding of astrology and still argue that its activities are "externally constrained" since astronomers are not being funded in proportion to their needs as astrologers are to theirs. This example illustrates a failure to consider the material conditions for meeting intellectual needs.

BACK FROM POSTMODERNISM AND INTO THE PUBLIC SPHERE

A postmodernist reflecting on the varieties of liberalism under discussion might well make the following response to the so-called deep problems of equality and justice I have managed to elicit:

> Aren't these problems simply artifacts of the rather modernist way in which you have framed the issue? After all you presume that there is a concentrated and finite amount of "concern," "attention," "resources"— call it what you will—that focuses the efforts of the various disciplinary perspectives. In large

measure, that seems to be what you (and, say, Habermas) mean by "the public sphere." But no such carefully circumscribed epistemic field of play exists. The only feature of today's liberal societies that resembles this image of the public sphere is voting—a rarely, and then mindlessly, performed ritual. If you treat voting as the focus of democratic liberalism, then most of the real action happens offstage.

This critique (due to Joseph Rouse) has some merit, but unfortunately suggests that the problems traditionally associated with the public sphere, such as the existence of asymmetrical power relations, have disappeared. That suggestion, however, is unwarranted. One way to look at the fate of the public sphere is that the "forums" that used to focus the life of a *polis* are now commercially licensed to the media for purposes of mass consumption. Consequently, reformers—be they politicians or intellectuals—may wind up as disposable media icons whose widespread exposure serves to outwear their welcome before they have had a chance to make a lasting impression. Mistaking *entertainment* and *influence* occurs commonly. As a result, one can confuse the ability to command people's time and money on an occurrent basis with one's ability to transform people's underlying dispositions. Still power remains at work. In this instance, power can be effectively exercised only by people whose projects are insulated from, and perhaps even camouflaged by, the endless circulation of limelight. Thus, the diffuse marketplace atmosphere of postmodernism's "anti-public" is ultimately a playground for Machiavellis (cf. Elster 1989 on market vs. *polis*).

Political naiveté aside, the postmodernist trades on a certain epistemic asymmetry, allowing a privileged perspective on her own position that she explicitly denies to her opponent's. Thus, if the postmodernist were correct that contemporary democracy lacks a public sphere to center its activities, and hence is no longer the sort of thing that one can centrally (or philosophically) plan, then how could she have come to know such a thing? What sort of global understanding would she have had to attain? What type of surveillance operations would she have had to perform to reach this conclusion? What would be the implicit "center" of her own conception of contemporary democracy? The postmodernist requires just as sure and comprehensive grasp of the knowledge system as the modernist to conclude that any major change in our epistemic institutions would be misguided.

In a brilliant critique of the "reasonableness" of laissez-faire skepticism of public policy, the distinguished political economist Albert Hirschman (1991) noted that liberal and radical politics—the politics of change—have typically been bolstered by claims to knowledge that entitle their possessors to construct a new order: "Enlightenment is Empowerment!" or "The truth will set you free!" (Fay 1987; Sowell 1987). Not surprisingly, conservatives and reactionaries have been able to occlude their own politics by simply disputing the epistemic grounds of such calls for change: Do we really know more now than the accumulated wisdom of the past has taught us? Are utopian promises worth risking a world to which most of us have grown accustomed? Hirschman points out that if our understanding of human affairs is as partial and indirect as both postmodernists and conservatives think, then this partiality and indirectness should equally apply to our own knowledge: If history allegedly teaches us that central planning always fails, then maybe we should equally distrust the central planning of the historical record that was required to draw that conclusion. In that case, we are entitled to at least a modest optimism about the prospects for social experimentation.

But what kind of social experiment should we make to circumnavigate the three liberalisms that today dominate the thinking in contemporary democracies? The answer provides social epistemology's attempt to reconstruct the public sphere, or "forum," as a site of interpenetration. Postmodernists rightly reject nostalgic views of the public sphere that find no counterpart in recorded history. Habermas, Dewey, and even Popper sometimes write as if there were a time when discourses were commensurable and various ends of concern to significant sectors of the population were openly disputed and ultimately resolved. These pleasant mystifications have led postmodernists to jettison the forum as just so much excess normative baggage. On the contrary, I argue, the rise of incommensurability is precisely what has motivated the mass translation and communication projects associated with the forum in politics (and "reductionism" in epistemology). For an ironic consequence of the increasing division of cognitive labor in society is that more of us, for more of the time, share the role of *nonexpert*. This universal sense of nonexpertise is the epistemic basis for reconstructing the public sphere today.

Nonexpertise is occluded in both the philosophical and sociological literatures by discussions of the rational grounds for "deferring to authority" (Stich and Nisbett 1984) or "trusting the relevant experts" (Giddens 1989). These panglossian discussions rest on an unanalyzed conception of *trust* that valorizes the rarity with which we are in a position to scrutinize the activities of our fellows. After all we might draw two quite different conclusions from the fact that, living in a highly complex society, we are forced to trust others for things that we cannot do ourselves:

1. Everyone is as *competent* in their field as I am in mine.

2. Everyone is as *incompetent* in their field as they are in mine (or I am in theirs).

Both (1) and (2) are, prima facie, inductions that can be equally made from my own experience. The difference between the two inductions is the amount of interpretive charity that I bestow on the actions of others. But, epistemologically speaking, what is my evidence to support each of the following claims?

1. I am competent in my field.

2a. Other people are incompetent in my field.

2b. I am incompetent in other people's fields.

3. Fields are sufficiently similar to each other that I can infer from what "competence" means in my field to what it would mean in other fields.

Most of the evidence is of a "default" nature: It is driven by the relative absence of evidence that contradicts the prior claims. This characteristic is hardly the stuff of which robust epistemic commitments should be made. Now consider the following two interpretations of what "trust in action" amounts to:

A. I have a live option to check up on someone, but do not do so in deference to the presumed character and ability of the person.

B. I have no such option because I lack the time and skill to do so, and so I am forced to rely on that person's judgment.

(B) is more commensurate with our real epistemic situation than (A). Still this point is easily masked. To remedy the cognitive dissonance created by our near-universal inability to scrutinize the actions of others, we lower the standards for what we expect from those who purport to act on our behalf. Competence, therefore, dissolves into a measure of the number of irreversible errors (the fewer the better). Competence need not imply performance significantly better than would be expected of a nonexpert. Given the diminished expectations that have accompanied our "society of trust," that experts and nonexperts may perform equally well at so-called *expert tasks*, as measured by "real-world" standards (Arkes and Hammond 1986), should come as no surprise. Thus, the egalitarianism required of the public sphere has reentered through the back door!

After (rightly) stressing the pervasiveness of incommensurable discourses in contemporary democracies, postmodernists (wrongly) shift the motivation for making knowledge claims from *communication* to *self-expression* (O'Neill 1990). However, if one retains a rhetorical interest in communication—in spite of this admitted incommensurabilty—then the ideal of a forum is needed to prevent the expressive environment from inhibiting responses to whatever claims are expressed. Aside from the unfeasibility of dealing with an indefinite number of voices, that those voices would wish to be among indefinitely many others clamoring for attention seems unlikely. The scarcity of air time presupposed by the forum is, therefore, the mark that communication is of ultimate concern. Moreover, one cannot simply argue—as Habermas (1985, 1987) tends to do—that a normative conception of a public sphere is *already presupposed* as an ideal limit to our everyday talk. On the contrary, this claim puts matters exactly backward. The need for norms emerges from the material exigencies of the speech situation—the need in real space and time to discipline expression for communication to elicit action in a timely manner. In a world without exigence or scarcity, the sort of norms that Habermas so rightly seeks would not be needed. With all that in mind, we may now sort the wheat from the chaff in the three liberal models of the forum.

The *equal-in-principle* model's strong suit comes from assigning a central role to contingency in the relative standing of knowledge claims. The model, thereby, instills an "adaptationist" mentality in claimants wanting to survive the vicissitudes of the marketplace of ideas. However, the major disadvantage is that the selection and survival of claimants is nothing more than the outcomes of their contingent interactions. There must be a normative dimension that interestingly complements this natural state of contingency. In the case of *equal-time* liberalism, the wheat is the sustained concern that all major positions have equal access to the means of knowledge production. The chaff appears as the tendency to inhibit people from changing their minds in light of the sort of vicissitudes that the first model stresses. That something akin to a monopoly might spontaneously emerge from the marketplace leads the equal-time liberal toward wanting to restrict free trade. Needed here are notions of equality and contingency that do not pull in opposing directions. Finally, the *separate-but-equal* model is to be applauded for its attempt to preserve differences in positions. But here the liberal seems willing to pay the cost of reifying those differences as "cultures," which limits the possibility of redefining differences especially as new knowledge claimants enter the marketplace.

From this selection procedure, I conclude that the desired liberal forum is a communicative environment that simultaneously sustains epistemic discourses that are mutually adaptive, indefinitely alterable, and equally available to the public. The norm I propose to stabilize this environment is taken from the economist's notion of *fungibility*—the extent to which a good is interchangeable with some other good in a consumer's preference structure. A highly fungible good is one that the consumer is willing to trade for another under appropriate circumstances. For example, I may possess a lot of food, more than I can eat right now. For the right price, I would be willing to exchange a large amount of that food for a good that would be of more use to me now. Consider, by contrast, the case of the car that happens to be my only possession not necessary for my survival. I may wish to buy something that costs considerably less than the car is worth. However, taking the car apart and then using a given part of comparable worth—say, the carburetor—in trade for the desired good makes little sense. The reason, of course, is that my car would no longer work, and my potential buyers would probably have no use for a stray carburetor. The car, in this example, is less fungible than the food. My earlier example of astronomy and astrology suggested how the analogy applies to knowledge production, which I now propose as a principle:

> *The Principle of Epistemic Fungibility*: In a democratic forum, an epistemic discourse must be aligned with practices whose fungibility increases as the demand that the discourse places on the cognitive and material resources of society increases.

Epistemic discourses may be more or less fungible. Their fungibility depends on the ease with which their knowledge claims can be translated in non-native idioms without causing the natives to claim a loss of epistemic value. For example, the knowledge claims of contemporary high-energy physics cannot be so translated. No cheap substitutes exist for particle accelerator experiments, advanced mathematical calculations, and the like which would enable more people either to participate in physics or to partake of its current budget. Any attempt to find more economical and less discursively formidable means to test physics claims will be met by cries of "vulgarization" on the part of the physics community. Thus, one must sequester funds and expertise specifically *and exclusively* for the conduct of high-energy physical inquiry—or not at all. There is no middle position, no room for negotiation.

Philosophers of science, Polanyi most notably, have traditionally portrayed this non-negotiability (or "autonomy," to use the euphemism) as an appropriate aspiration for the special sciences. Even philosophers such as Nicholas Rescher (1984), who fully realize that this goal is bound to be economically unfeasible, nevertheless endorse the same principle. But what principle? To underscore the intuition behind the Principle of Epistemic Fungibility, one should stop thinking of Big Science as a self-sustaining and progressive enterprise whose trajectory we may someday be forced to curtail for "merely practical reasons." Rather, one should imagine contemporary physics, in this instance, as a large fossil-fueled industry that became overadapted to an environment that no longer exists and now resists converting to ecologically sounder energy sources. The age and size of physics would thus mark the discipline as a dinosaur whose continued existence—in its current form—threatens the livelihood of other discourses also in need of resources.

The crucial phrase here is "in its current form" because making physics safe for democracy *is* possible. This fungible physics would recognize the deeply conventional character of expressing its theoretical claims in certain sorts of terms that are then tested on certain kinds of machines. Showing that academic, military, and industrial interests configured physics in this way in the aftermath of World War II (Galison 1987; Galison and Hevly

1992) would be easy. Although those interests have moved elsewhere since that time, the configuration survives as an atavism in today's world. *The Governance of Science* (Fuller 2000a) is largely a meditation on the implications of this point.

Fungibility requires a new configuration of verbal and other material practices. The first option would enable others currently in the forum to pursue their interests (should physics wish to retain its current large scale). The second option would downsize physics to a point at which its own esoteric pursuits no longer threaten the viability of other epistemic discourses (should physics wish to retain its current autonomy). An example of the first option would be for physicists to encourage social scientists to do the sorts of inquiries that would make it easy to understand and evaluate high-energy physics research as large-scale political, economic, and cultural phenomena. At the moment, social scientists often professionally suffer when they attempt such intensive scrutiny (Traweek 1988). An example of the second option would be for physicists to agree to decide their high-level theoretical disputes by using only advanced mathematics or relatively inexpensive computer simulations. This idea is sometimes what is meant by the "end of science" (Horgan 1996).

As a normative model of communication in the public sphere, fungibility's economic origins may leave something to be desired. After all "the market experience" (Lane 1990) tends to collapse heterogeneous value dimensions into a money-based standard of utility. However, the market need not be rendered in the image and likeness of neoclassical economics, that is, as a field of utility maximizers in an overall state of equilibrium. A more attractive image of the market has emerged in recent years, an "economic sociology" designed to articulate what Max Weber and Joseph Schumpeter had seen earlier in this century as the source of capitalism's cultural dynamism (Swedberg 1989; Block 1990). According to this picture, the producers and consumers of a good are oriented differently, which makes their interaction in the marketplace always somewhat adventitious. Under normal circumstances, producers are primarily oriented toward each other as they internally differentiate a niche for goods that consumers, supposedly, will regard as competing for their attention (White 1981). Consumers, however, are primarily oriented toward types of functionally equivalent goods that place conflicting demands on their appetites. A market is present insofar as producers and consumers orient their activities in terms of each other's projected array of options.

Allow me to put the last point crudely. Knowledge producers, currently, would like consumers to think in terms of disciplinary alternatives—say, "microphysics versus microbiology" (and hence support the research trajectories projected by one or more of these fields). However, the knowledge-consuming public (which includes not only government, taxpayers, and industry, but also other professional knowledge producers in search of ways to deploy their labor) really thinks in terms of such problem areas as "nuclear power versus cancer research." This difference is important for understanding the role of innovation in reconfiguring consumer needs in a market economy. A "consumer need" cannot be identified independently of the relevant set of goods that consumers take to be functionally equivalent and, hence, interchangeable in a given transaction—the original meaning of "fungible." Thus, as the array of rival products changes, so too does the nature of the need. The most successful innovations do not presume the objectivity of consumer needs, and hence some metric of efficiency by which such a need can be better satisfied. Rather, successful innovations reconfigure a market niche by causing consumers to make choices between products that they previously did not take to be functionally equivalent, which is to say in competition with each other (Brenner 1987).

Thus, innovation can go in one of three general directions:

1. Producers can compete to satisfy an already existing consumer need more efficiently. This strategy, the principal source of competition in economic markets, includes the "demand pull" explanation of technological progress (Layton 1977). However, given the control that professional associations exert over the manufacture, sale, and assessment of knowledge products, major external—often state-induced—incentives must be provided before this "seller's market" is broken. This action enables the formation of interdisciplinary coalitions that can address consumer concerns. An obvious example is the establishment of well-endowed National Institutes of X (where X is some pressing social problem) that entice researchers away from their pet projects. Without such institutions, knowledge producers can make comfortable livings simply by addressing discipline-specific problems.

2. Producers may try to reconfigure consumer needs to bring them into optimal accord with producer capabilities ("demand management" in Galbraith 1974). Advertising often functions as a precipitant of wants for things that people had not previously desired. Universities typically operate as de facto advertising agencies for knowledge producers. For example, students interested in solving the mysteries of cancer are told that, instead of dealing with that problem directly and comprehensively, they should accredit themselves in some subfield of biology and contribute to one of its standing research programs. With some luck, their research may eventually solve part of the mystery. In this epistemic bait-and-switch, a yearning for civic relevance is all too often satisfied by academic filler.

3. Radical innovation, which impressed Schumpeter (1942) as the lifeblood of capitalism, succeeds by reconfiguring consumer need in restructuring the relationships in which producers stand to each other.

> The innovator, like the STSer, regards the current organization of personnel and equipment in the scientific community as a conventionally divided pool that may be redivided to strategic effect. For example, a new research program that promises much without demanding major retooling from interested personnel might attract and combine disparately trained scientists who form a disciplinary arrangement that generates needs and products quite unlike anything previously seen. This strategy worked, for example, to the advantage of Wilhelm Wundt, who showed that philosophers, physicists, and medical researchers could contribute their expertise to a new science of the mind—"psychology" (R. Collins and Ben-David 1966).

Finally, my formulation of the Principle of Epistemic Fungibility is influenced by the history of mass media law in the United States (Lichtenberg 1990). Instructive are the debates surrounding the so-called *fairness doctrine*, whereby a public medium is required to offer free response time to someone criticized in the medium. The doctrine was expanded to include the presumption that a medium will include the major sides of a controversial issue that it plans to air. Interestingly, the expanded fairness doctrine has been criticized from two quite opposite political quarters, but for what amounts to largely the same reason.

The fairness doctrine, which was developed with television and radio broadcasts in mind, has been questioned by those who see broadcasts as sufficiently continuous with print media to be worthy of the same legal coverage. Newspapers are typically not obligated to print the responses of people who are criticized on its pages because readers already understand that a newspaper is a partisan medium. An appropriate course of action for a person being criticized is to find a paper sympathetic with her views and to write for it. To obligate newspapers to publish responses would thus serve only to dilute expression and confound public debate.

In contrast, the fairness doctrine has been attacked by those who believe that certain positions—especially if they are morally repugnant—do not deserve any air time. Like the protectors of a free press, the defenders of censorship also worry about the resulting dilution and confusion. In this case, however, the concern is that "right-minded" opinions may not appear as such unless they receive a clear and exclusive public hearing. Still the main fear of sophisticated censors is not conversion to a "wrong-minded" opinion. Rather the fear is that equal access breeds tolerance that, in turn, leads the public to question the significance of having to make a choice between opinions.

Both the independent newspaper and the moral censor take for granted a certain lack of natural fungibility in opinions. To wit, what can be said clearly in 30 minutes can be said only confusedly or dilutedly in 15 minutes, especially alongside a competing opinion. Knowledge production in contemporary democracies cannot afford to make this assumption. On the one hand, I do not endorse the "sound-bite politics" that the fairness doctrine seems to promote in our day (i.e. where everyone is limited to 30 seconds so as to enable all six candidates to speak). On the other hand, I believe we leisure-ridden academics should be reminded often that the need for norms governing our epistemic pursuits arises from the sorts of "real-world" constraints that the mass media have directly addressed—even if not to everyone's satisfaction.

BEYOND ACADEMIC INDIFFERENCE

Academic students of knowledge production only reluctantly recommend courses of action based on their research. One classic humanist argument in this vein asserts simply that if the past is to be understood "as it actually happened," then historical inquiry cannot be subserved to contemporary interests. After all denizens of the past were addressing each other, not us. To presume otherwise would constitute epistemological malfeasance. Humanists impressed with this argument often recommend an "antiquarian" research strategy that makes the identification of dissimilarities between past and present a scholarly desideratum. To counter, one can argue that antiquarianism's rigorous pursuit of historical incommensurability has been rhetorically *very effective* in delegitimating contemporary practices. These pursuits show that societies have functioned quite well without what is now taken to be necessary for the continuation of our own society. Indeed the more self-contained the past is made to appear, the more today's trenchant "necessities" look like dispensable "contingencies." Much of Michel Foucault's account of the role of "madness" in European society prior to the emergence of psychiatric and penal institutions is presented in this spirit. As with ethnographies that stress the salutary divergences of native lifestyles from our own, an exaggerated antiquarianism may jar any ethnocentric, presentist complacency. Here the real issue is not the potential corruption of scholarly methods. The issue, rather, is how explicit the humanist needs to be in drawing out the intercultural differences implicated in her own line of inquiry. Taking note of those differences in a completely neutral manner is difficult. Much will depend on the standards by which—and the audiences to whom—humanistic scholarship accounts for itself. What sorts of things should one expect to learn from such inquiries?

From the more immediate ranks of STS researchers, social constructivists have made abstinence from policy look fashionably radical. Notwithstanding, constructivists play a familiar "value-free" policy role when laying out the various implications that follow from reading the evidence in different ways. Suppose we grant that, in constructivist hands, the "facts" are multiply interpretable texts that no longer speak in one voice. Nevertheless, the constructivists' studied neutrality on policy issues makes one wonder whether they are high-minded ("beyond politics"), opportunistic (available to the highest bidder), paralyzed (anxiety-ridden, in the manner of Pontius Pilate), serenely cynical, or simply oblivious to the fact that neutral acts are still acts (hence no less interventionist than committed ones). One rigorously self-consistent stance characteristically adopted by the constructivists is a logically relentless pursuit of *reflexivity*, which is given detailed consideration in the next chapter. Yet let us call the enigma posed by constructivism, and by all officially uncommitted scholarship, *the inscrutability of indifference.*

Such inscrutability does not deter policymakers from making use of STS scholarship to suit their own purposes—especially once they fund it. Like most scholars, STSers are willing to play enough politics to get funded, but rarely enough to take responsibility for the extramural consequences of that funding. As the case studies produced by social constructivists are designed to show that seemingly ironclad instances of scientific reasoning or technological application can be called into question, it should come as no surprise that this work is used to slash the budgets of both military and medical research—projects in both artificial intelligence and social work. Thus, the intellectually radical metamorphoses into the politically capricious. Many, if not most, of these cuts would probably be condemned by the constructivists when speaking as "private citizens."

Even if we grant that a scholar can live the Weberian dream of neutrally presenting the courses of action available in a given situation, nothing follows about the number of possibilities that *should* be presented. Here I mention the introduction of values not as something that distorts choice by clouding judgment, but rather as something that enables choice by focusing judgment. In the terms of cognitive science, a scholar's value judgment functions as a "heuristic" for her audience. If the scholar (or teacher, more generally) lays out more possibilities than a policymaker (or student) can reasonably be expected to weigh in her mind, she effectively subverts her charge to motivate action. The move is tantamount to saying that incapacitation, confusion, and arbitrariness are preferable to following specific advice if the advice is anything less than foolproof or unbiased. Faced with this scholarly obstruction of action, the audience can act intelligently only if they decide to ignore outright—rather than discount on reflection—what the scholar has told them.

Let us say that the scholar does succeed in presenting a cognitively manageable range of options to the policymaker. The scholar's satisfaction with *that* state of affairs—that she has no further obligation to resolve the issue—reveals not so much the suspension of moral commitment as a positive commitment to *moral vagueness*. Moral vagueness is the academic's latent preference for keeping ideas in a state of perpetual play over advocating one such idea for the purpose of changing the audience's mind. It is as if intellectual assent—the judicious nod—were assent enough. Certainly, such assent is enough if one is interested in reinforcing the idea of an internal history of science that is subject to its own inertial motion until arrested by an external force. This point would be more apparent if the usual academic presumption about ideas were reversed. Consider, for example, if ideas were regarded as *normally* motivating action, unless willfully prevented from doing so by, say, the sort of moral suspension of practice required to "entertain" ideas. Currently, however, academics are professionally disabled from distinguishing serious from playful utterance. John Dewey, where are you when we need you!

Changing minds, the alternative to gaining mere intellectual assent, is admittedly no easy matter. Academics these days are not particularly up to the task. One reason is the fear that open advocacy will be perceived as "dogmatic," the ultimate academic breach of tact. In the public sphere, we normally regard the taking of stands as a sign of intelligent engagement with the world. Not so in the academy, which overestimates "the power of ideas," and so advises a policy of self-restraint as a courtesy to a world unprepared to properly assimilate those ideas. As an antidote to this line of reasoning, dogmatism must be seen as a rhetorical accomplishment. Dogmatism, then, should not be seen as the inherent property of an opinion or even of the person holding it. Rather, dogmatism is a social fact that is constructed whenever a speaker takes a position *and the audience refrains from resisting it.* Lack of resistance inhibits the emergence of a standard by which the position can be held accountable. Such standards typically arise from the rhetorical obstacles that interlocutors pose in the way of their acceptance of the speaker's position. By somehow trying to remove the obstacle, the speaker implicitly acknowledges the presence of a standard of accountability. Advocacy compromises objectivity *only* if the advocate is addressing a captive audience. To the extent that people worry that education is slipping imperceptibly into indoctrination, classroom conventions, to the same extent, have yet to emerge which redress the asymmetrical power relations enjoyed by the lecturer.

Where charges of dogmatism are lodged one can expect to find a rhetorical vacuum. In this instance, hardly anyone is uttering opinions, and probably little communication is taking place between those who dare utter. Unless public encounter is actively encouraged in the academy, the official policy of bland tolerance is likely only to exacerbate this tendency. Indeed placing the tolerance of alternative viewpoints above all other intellectual virtues leads to what may be called *reverse dogmatism.* Reverse dogmatism follows from the tendency of all opinion to gravitate toward the epistemology of existentialist theology—namely, the essential irrationality, and hence uncriticizability, of any beliefs that matter to their holder (Bartley 1984). Under such circumstances, any attempt to assert that one's viewpoint is superior in some way to another's is perceived as a charge of "bad faith" against the people holding the allegedly inferior opinion. Hurt feelings, not counterarguments, are the likely result. In many ways, the existentialist scenario is not the worst possible result. One can further imagine an academic culture that becomes so accustomed to intellectual self-restraint that members of the culture unwittingly turn themselves into classical skeptics filled with the spirit of *ataraxia*—the peaceful indifference that comes from not feeling compelled to take a stand on anything, either publicly or privately.

Even more than lacking a taste for public encounter, academics are simply ill practiced in the art of changing minds. They are cursed with captive, docile audiences—students and colleagues. These audiences are largely forced (or paid) to listen to them usually by an institutional mechanism that is only tangentially related to the promotion of the interests of either speaker or themselves. As a result, in this rather ironic way, the academic audience listens for its own sake *because the exercise serves no other useful function!* Simple facts sum up the case here. Since the academic's livelihood depends not on the size of their audiences, but on the bare inclusion of their courses in the curriculum or their papers in conferences, only in a very weak sense do they need to compete for "air time." (This may be more true in the United States than in, say, France, where the boundary between the academy and the general culture is more permeable.) Moreover, air time is implicitly devalued because oral presentations are often heralded and judged as surrogate writing events; hence, lectures are "read." Given such insensitivity to media, one's rhetorical skills can easily grow fallow. An academic is typically under no professional obligation to render her viewpoint as an extension of one that the audience already holds. Hence, she is typically unused to treating the audience's intellectual and material resources as necessary means for achieving her own ends. Indeed the academic audience is typically made to feel guilty for *its* failure to grasp what the speaker has said.

Policymakers frequently remark on academic's inability to express themselves in the manner demanded by our harried, postmodern times. The masters of this desired form of communication run management training seminars, which have quickly become standard weekend events at hotels and campuses across the developed world. These seminars are intensive, but modularized into clearly delineated "mind-bites," so that the audience is capable of chunking the information presented at a manageable rate. The oral presentation is animated, memorable, flexible, and interactive—the management trainers being unafraid to tailor the relevant principles to the needs of the audience. Indeed trainers even draw attention to the value of such tailoring, which academics would be inclined to see as instances of equivocation best kept hidden if unavoidable. What the academics miss, however, is that "The Top Ten Tips to Talk Turkey" are not meant to be vulgarized empirical generalizations about successful negotiation strategies. These principles, rather, act as mnemonics that the negotiator can call to mind to stimulate lateral thinking about her current situation, where the hidden puns and equivocations serve as the source of opportune associations. In *Social Epistemology*, I spoke about this rather unacademic use of language as characteristic of maxims and aphorisms found in the law and literature. These principles are worded so as to offer the most economical expression of an idea that is intended to have the widest possible application (Fuller 1988a: 204-5). Designing principles that are both parsimonious and inclusive ensures an interpretive flexibility. These characteristics prevent the principles from stereotyping the cases to which they are applied and decreases the likelihood that they will be renounced in light of a conclusive test case.

In short, then, academics often fail to impress policymakers—even when they try—because they misunderstand the social function that their words are being asked to perform. Policymakers want language that can be used as a tool, as part of a course of action, with a clear aim in sight. What that aim is, however, turns out to be more negotiable than academics are usually willing to recognize. Ideally, policymakers would like the academic to offer advice *as if it mattered to the academic herself*, and thus assume a stake in the outcome of the policy issue under consideration. The academic's failure to take up the challenge is probably a greater source of disappointment and resistance than any hidden agendas or ideological preconceptions on the part of the policymaker. Craig Waddell (1990) is right that the devaluation of *pathos* in academic rhetoric is principally to blame here. Consequently, not even self-avowed "socially responsible" academics, such as ecologists, seem to know how to convey commitment in their speech and writings so as to motivate the appropriate action.

An academic is taught to speak and write as if her audience were going to evaluate her utterance "on its own terms." Thus, the academic is expected to mobilize facts and reasoning that are sufficient for her intended audience to license the conclusion that she wants to draw. Taken at face value, this rhetorical charge is quite weak. All the academic speaker must provide are "good reasons" for her claim. At most such reasons will convince her audience that holding the view expressed is not irrational. But the audience will still be left wondering why the speaker would *want* to hold such a view, and, perhaps more importantly, why anyone else *should*. After all a defensible view is not necessarily worth defending. In the next section, we see that a rhetorically adept speaker typically enables an audience to see her viewpoint as an extension of theirs. This strategy certainly smoothes the passage between intellectual assent and motivated action. Unfortunately, academic communication often seems to move in the opposite direction; audiences are forced to accommodate their own agendas to what the academic speaker says. Indeed academics have been known to wear their rhetorical intransigence as a badge of integrity—or at least disciplinary purity. However, the academic desirous that others recognize her integrity must hope that her audience does *not* follow her own example! Kant and Habermas would be very disappointed by the lack of symmetry in the expectations of speakers and audiences in the typical academic speech situation.

A truly democratic rhetoric, one comprehensive enough to cover academic discourse, requires that change of mind *not* be the product of what may be called the *belligerent syllogism*:

> One of us must move.
> I won't.
> Therefore, you will.

Instead change of mind must result from the *facilitative syllogism*:

> We're already trying to move in the same direction.
> There is an obstacle in your way.
> Therefore, let me help you remove it.

As the form of facilitative syllogism reminds us, changing minds begins only once the speaker already detects a common core of intellectual agreement with her audience, but the audience has yet to see that agreement as a basis for action. In this context, I recommend two rather opposing strategies.

The first strategy appeals to an aspect of everyday cognition for which academics have a trained incapacity. This strategy involves seeing that conceptually unrelated items may be materially inseparable. An example, from the previous chapter, would be using "referential opacity" when critiquing "Managing the Unmanageable." Following the human geography literature, Anthony Giddens (1984) has called the process *space-time binding*. In this instance the academic shows that by acting on her seemingly rarefied point, the policymaker will *also* be in a position to do what she has wanted all along.

The second strategy, however, requires less special training. This strategy caters to the academic's taste for discriminating essential from non-essential features of an object. To wax Aristotelian, the idea is to argue that by concentrating too much on the immediate "matter" of the object, policymakers have failed to do what is best to realize the object's underlying "form." Less metaphysically speaking, policymakers often fetishize a particular means while forgetting the end it supposedly serves. Distancing ultimate interests from current courses of action is a powerful and versatile strategy. To make the point, consider two arguments one might make to persuade policymakers that it would be in science's *own best interest* to be downsized.

What is essential to maintain the scientific enterprise and what are mere accretions on that essence? One argument says that if science does, indeed, aim to increase the storehouse of knowledge, then the quality of communication between inquirers will matter more than the sheer quantity of inquirers communicating. If true, one may show that, beyond a certain number, each additional scientist *reduces* the likelihood that *any* of them will make substantial contributions to knowledge. Therefore, both established and novice scientists have an interest in restricting their own numbers. What is euphemistically called "personnel redeployment," however, is rarely a popular cause. The academic may have to reverse her tactics.

A second argument identifies science essentially with its practitioners and only inessentially with what they do. For example, if science is portrayed as a democratic process that works better as more people's opinions are incorporated, then what is needed is a strategy to maximize everyone's involvement, within resource constraints. On this line of reasoning, let us assume that high-tech equipment has become so expensive that only a privileged elite can participate in cutting-edge science. Accordingly, policymakers would need to be persuaded that only unreflective, conventional practice leads them to think that advanced theories in physics, say, must be tested by such means. Reducing the cost of apparatus and training needed for testing theories then would improve the quality of scientific judgment by expanding the pool of potential testers.

THE SOCIAL EPISTEMOLOGIST AT THE BARGAINING TABLE

In what frame of mind should the social epistemologist approach the epistemic bargaining table? How may she ply her interpenetrative trade to maximum effect? At the outset, the social epistemologist faces a "coordination problem": *How does she get her foot in the door without putting it in her mouth as well?* Levity aside, the social epistemologist must justify her existence to audiences jealously guarding their autonomy from unwanted normative incursions. In this context, she must bear in mind that the one who raises a problem is not necessarily in a privileged position to solve it. If the social epistemologist can persuade her audience to confront a problem, that she was the one who first articulated it is immaterial. We have, then, a shared problem that all parties have identified as their own and that necessitates collective judgment.

Next, the social epistemologist needs to appreciate just how much the knowledge-production process has changed in the 400 years since the first politically sanctioned scientific societies. She cannot simply assume the mantle of earlier philosophers of science given the change of scale in the scientific enterprise. A change of scale typically implies a change in causal structure, which, in turn, implies new pressure points for intervention. An illuminating analogy can be drawn between the three stages in the history of industrial management proposed by geographer David Harvey (1986) and the stages undergone by knowledge policy during the same period.

1. *Traditional*:

Free labor dictated its own terms. The conception and execution of work remained in the hands of the same individuals, who together constituted a "guild" with exclusive rights over a "craft." The normative structure of work would be initially passed on through apprenticeship under a master of the guild. Eventually, the individual would be allowed discretionary power over the conduct of her work. Although rules of thumb may be proposed for the performance of labor, these rules will offer little guidance to someone not already a part of the guild. In terms of knowledge policy, this arrangement corresponds to the period from the 17th to the 19th century, when philosophy of science was done primarily by scientists reflecting on their experience as "natural philosophers."

2. *Modern*:

Labor's terms are dictated by management. That work is conceptualized and executed by two mutually exclusive groups of people amounts to a class difference. Labor and management receive different training and, indeed, are in contact with each other only in the formal work setting during the evaluation of labor's performance. Thus, management may have little more than a cursory, often stereotypical understanding of the actual practices of the labor they supervise. Given this pattern of interaction, management unsurprisingly thinks that the execution of any particular task can always be streamlined along dimensions that the task shares with other seemingly unrelated tasks. Labor responds, also unsurprisingly, by resisting management's strictures by asserting the heterogeneity of tasks.

The introduction of "external" managerial standards for labor parallels the rise of philosophy of science as a field of inquiry distinct from science in the late 19th century. As the positivist movement clearly illustrated, all sciences were subject to the same principles of evaluation regardless of content, method, or stage of development. The philosophers were sometimes trained in the formal aspects of the special sciences, but more likely in logic and epistemology. A clear example of the alienation of philosophical conception from scientific execution was the introduction of a strong distinction between the contexts of justification and discovery. Additionally, philosophers explicitly raised the issue of "the ends of knowledge"—often under the rubric of principles of scientific progress—under the presumption that scientists produced knowledge for some larger purpose of which they might be only dimly aware. This axiological discussion corresponds to Frederick Winslow Taylor's original conception of the industrial manager as someone who steers the course of labor in the direction of the "public good"—something that was not necessarily served by labor left to its own devices.

3. *Postmodern*:

Transnational corporations have highly diversified financial interests spread throughout the globe. In this context effective management demands a flexible power structure that can exploit new markets as opportunities arise. A flexible power structure implies not only a proliferation of decision makers, each of whom can act in relative independence from the rest, but also a mobile labor force. Managers fully realize that highly skilled labor is difficult to monitor and replace, so the modernist tactic of dividing the conception from the execution of work is ineffective. However, management can ensure that labor remains loyal to corporate ends by preventing the emergence of any local power base. The idea here is not only to keep labor circulating around different work settings, encouraging temporary collaborations designed to develop new products, but also to simultaneously inhibit the formation of group attachments that could jeopardize the corporation's adaptability to future market changes.

The corresponding tendency in knowledge policy is the one promoted by social epistemology. The academic division of labor has rendered absurd the idea of philosophers telling scientists how to run their daily laboratory activities. More absurd would be to give all scientists the same advice as the logical positivists tried to do under the rubric of methodology. Moreover, whether this "competence gap" is addressed by social epistemologists' acquiring a smattering of training in a "hard science" remains unclear. So doing would reinforce the science–society boundary that STS claims is nothing more than convention. *A fortiori*, this point applies to more educationally extended attempts at meeting scientists on their own turf. Can the social epistemologist obtain the degrees and skills that would mark her as a science "insider" without becoming coopted in the process? After all the longer one spends in professional training, the more psychologically primed one is to find something worthwhile in it. It is very difficult to learn only negative lessons from one's experience. A subtle site for cooptation of this sort is the science criticism that scientists such as Stephen Jay Gould practice.

Scientifically trained science critics, although fully aware of the error and deceit that have traveled under the name of "science," are nevertheless prepared to find fault only with particular individuals. Rarely do they extend their critique to the institutional structure of science. Indeed the institution's tendency toward epistemic equilibrium is usually credited with ultimately uncovering the individuals at fault (Chubin and Hackett 1990 demystify this ideology as it affects science policy). Thus, in scientists' hands, science criticism often turns out to be just another opportunity to celebrate science's capacity for self-governance. Readers of such works are able to vent their indignation at the outlaws of science without any spillover effects that might lead to a reconstruction of the scientific enterprise. An apt analogy for capturing the difference in the normative sensibility between this form of science criticism and social epistemology is the relation in which the Protestant Reformation historically stood to fully secularized European culture.

Still social epistemologists should not avoid the company of scientists. Rather, following from STS training, the social epistemologist should engage in what ethnomethodologists call "participant observation" of scientific practices. She should learn, then, to ply her trade in the presence of those whose company she is most likely to loathe. That is really the only way to avoid the trap of all Enlightenment projects—namely, *preaching to the converted.*

A gambit true to social epistemology's character is to try persuading scientists that *their own* interests are served by becoming social epistemologists. In particular, scientists need to realize that competence is context-dependent. A scientist is not competent per se, but competent relative to standards of performance and especially to the control that the scientist has over the circumstances under which she is expected to perform. The patina of expertise enjoyed by physicists and economists stems, in large measure, from their ability to dictate the terms in which they display their knowledge. They always seem to get to play in their own court. But this patina would fade by making interdisciplinary projects unavoidable. For if scientists are required to pool their resources with those in other fields, then the terms of epistemic exchange will need to be continually renegotiated. As a result, no group of scientists will be able to gain the sort of power that accrues to workers who routinely have discretionary control over the use of their labor. In turn, scientific discourse must be intelligible to a larger constituency and, indirectly, open to greater public scrutiny. The relevant normative instruments will no longer be methodologies, but *incentive structures*—strategies that enable disparate scientists to see that their own best interests served by working together on projects that will have generally beneficial social consequences. These strategies should not be taken as anti-science. Rather, they underscore the priorities of social epistemology's brand of Enlightenment politics: *Before society can be scientized, science must first be socialized.*

So let us say that the social epistemologist has arrived at the bargaining table. She plans to mediate between conflicting or noncommunicating groups of researchers, typically representatives of different disciplines, to get them to collaborate on some pressing need, be it broadly "political" or narrowly "cognitive." What might she do under the circumstances? To appreciate the types and levels of intervention, let me start by paraphrasing a question originally posed by Georg Simmel (1964) in defining the sociology of conflict: If you see two groups of researchers in conflict, what do you do:

a. Ignore the conflict (isolationism);
b. Engage both sides in conflict (jingoism);
c. Take a side (ideological alliance);
d. Make yourself essential to *any* resolution?

Alternatives (a), (b), and (c) represent familiar philosophical roles. Professionalism over the last 50 years has made (a) an increasingly common response as philosophers regard their task as the production, correction, and maintenance of philosophical texts–full stop. The difference between (b) and (c) captures, in rough-and-ready form, the legendary antagonism between positivists and metaphysicians, respectively. Metaphysicians would try to build their favorite sciences into the groundwork of reality, whereas positivists opened up the sciences to as much logical and empirical contestation as possible. The social epistemologist aims to be both more opportunistic and more useful by adopting role (d), which Simmel called the strategy of the *tertius gaudens*.

Strategies for following (d) can be ordered from least to most involvement with the conflicting parties. (This ordering is drawn from a standard model for conceptualizing the role of the legal system in resolving interpersonal disputes: Golding 1974):

I. *Facilitator*: You provide a neutral forum for the combatants to work out their differences.
Philosophical precedent: Habermas' ideal speech situation.

II. *Negotiator*: You present each side to the other divested of unnecessarily polemical trappings.
Philosophical precedent: logical positivism's reduction of claims to their "cognitive content."

III. *Arbitrator*: You design the mechanism that resolves the dispute for them.
Philosophical precedent: Popper's crucial experiment.

Now what might result from this mediation? Consider four possible outcomes of the border war waged between philosophy and psychology as presented in Chapter 3. At the risk of sounding cynical, I imagine that movement down this list will accelerate as university budgets tighten.

1. Psychology and philosophy recognize that they are engaged in completely different activities that do not yield to direct comparison.

2. Psychology and philosophy complement each other's activities, thereby enabling an integration of disciplines.

3. Psychology asserts its authority over philosophy by placing empirical constraints on any adequate philosophy.

4. Psychology replaces philosophy as its successor discipline.

Moving from general strategies to particular tactics of interdisciplinary mediation, one of the most important obstacles to success is the belief that certain things—such as the truths espoused by a discipline or the character of the knowledge that it produces—are, fundamentally, unchangeable, and hence *nonnegotiable.* One example would be arguments to the effect that philosophy can be only one way because the "nature" of philosophy is to be that way. The fallacy here – to confuse what is innate or original with what is fixed forever—can be diagnosed as a violation of STS' Conventionality Presumption.

The rhetorical solution to such non-negotiability is what may be broadly called *compensation tactics*. These tactics, in turn, may be *prosthetic* or *corrective.* The choice depends on whether one takes the interlocutor's non-negotiability at face value and thus proposes a course of action to mediate or transform its inevitable effects, or one takes the non-negotiability as negotiable under the right circumstances, say, at the right price. After deploying either compensation tactic, one may then proceed to more explicit forms of persuasion. One can, thus, envisage a *continuum of rhetoric* ranging from the non-negotiable (and hence coercive), through the compensatory (and hence manipulative), to the negotiable (and hence truly persuasive):

a. Mechanically move the interlocutor to do what you want;
b. Threaten the interlocutor to do what you want;
c. Pay the interlocutor to do what you want;
d. Change subtly the situation to cause the interlocutor to do what you want;
e. Persuade the interlocutor to do what you want by appealing to the interlocutor's interests, while keeping yours hidden;
f. Persuade the interlocutor to do what you want by appealing to the prospect that both your interests and the interlocutor's interests would be served;
g. Agree—likely as a result of the interlocutor's changing your mind somewhat—that your mutual interests would be served by pursuing a common course of action.

If the social epistemologist were interested in changing minds as unobtrusively as possible without opening herself to a possible change of mind, then a compensatory tactic such as (c) or (d) would be preferred to either (a), (b) or (e), (f), (g). However, cognitive dissonance research suggests that corrective compensations may eventually backfire. The presence of payment or otherwise artificial conditions for judgment may continue to remind the interlocutor of the distance between her "real" position and the one to which the social epistemologist has managed to get her assent. After all if the interlocutor naturally held the position, why would she need to be compensated for holding it?

For similar reasons, the Machiavellian school of political sociology (Vilfredo Pareto, Gaetano Mosca, Roberto Michels, and their followers) has tended to see the power struggle between "lions," who appeal to brute force, and "foxes," who appeal to negotiation. Whereas the lions simply eliminate potential opponents, the foxes treat them as potential coalition members, and thus appease them by various compensatory tactics. However, such activities eventually absorb all the foxes' energies. Payments must increase in line with the coalition's increasing awareness of its centrality to the foxes' remaining in power. The Machiavellian moral to this story is that the social epistemologist should avoid rhetorical tactics that are *nonreusable*—that is, likely to wear thin over time. This story lends another rhetorical lesson. Even if the boundary between "natural" and "artificial" (or "internal" and "external") is continually renegotiated, an interest that the interlocutor originally held to be "artificial" must subsequently be interpreted as part of the interests that the interlocutor considers "natural." If not, the appeal to artificial will eventually wear itself out. For example, if the original artifice is financial, as in the cognitive dissonance case, then the social epistemologist may have even more of an incentive than usual to get the interlocutor to see political economy as integral to her activity to divest financial interests of their "artificiality."

Aside from reusability, the social epistemologist needs to be reminded of the *humility* of her own position. That is, the person whose mind you are trying to change may have good reasons to resist your efforts, which, if you gave her half a chance, she would tell you about and which would perhaps even change *your* mind. Within science, the issue of humility has become especially relevant to the notorious inability of psychologists to convince their subjects of the "errors" of their ways in post-experimental debriefing sessions.

In a fit of perverse consistency, psychologists have traditionally believed that subjects whose behavior deviates so strikingly from their own folk theories of themselves would probably resist any attempt to acknowledge such deviations. Here, however, the scope of the psychologist's own inquiry comes into question. If, for instance, the psychologist has detected deep inconsistencies in a subject who prizes consistency as one of her great virtues, she would seem obliged to have the subject grasp her finding. Nevertheless, the psychologist might not care to put in the additional persuasive effort because she sees the inconsistency as being of little significance for the subject's everyday life. This opinion is not only a self-fulfilling hypothesis, but also involves a meta-appeal to humility to preempt a more genuine application of the humility principle. Accordingly, both the subject and the psychologist would negotiate the exact relevance of the detected inconsistencies for both science *and* everyday life.

If insensitive to the power relations in which her position is embedded, the social epistemologist may unwittingly be met with the passive resistance of those she seeks to help. They, too, may conceal vital information or perspectives. Imagine representatives of two disciplines politely engaged in dialogue with an interdisciplinary mediator and then going about their business as usual after the meeting. The mediator must uncover latent disagreements, hostilities, and misunderstandings. The literature on postcolonialism is an excellent source for thinking about this entire problem. To wit, colonized peoples tend to use the explicitly cognitive appeals that colonizers make for their authority ("We know what is best for you") as the basis for subtle, usually negative judgments of the colonizers' moral worth. In sum, then, to avoid the colonizer's fate, *pace* John Rawls, the social epistemologist needs to abide by two principles of *epistemic justice*:

> *The Principle of Reusability*: When trying to get someone to change her ways, avoid tactics that are nonreusable or are likely to wear thin over time. (This idea captures the pragmatic punch of more ethereal appeals to the "universalizability" of the means of persuasion. Hence, the tactics must work not only here and now, but at any place and any time; so coercion and less than seamless forms of manipulation will not work in the long term.)

> *The Principle of Humility*: The person whose ways you are trying to change may have good reasons to resist your efforts. Given the opportunity, she would tell you these reasons and would, perhaps, even change *your* mind. (This view helps safeguard against the high-handed tendencies of demystification and debriefing. Often the zeal for remaking others in the image and likeness of one's own theories can prevent the reformer from catching potential refutations of her own theory.)

Finally, contrary to the received wisdom of STS, the ethnographer may not be the purest exemplar of humility. As an alternative, consider the *student*. Generally, the ethnographer is ultimately interested in the natives to have something to bring back to her own tribe. Yet the student training in a particular discipline wants to "go native" largely for its own sake—and not for the sake of some other enterprise, such as the enhancement of anthropology. Moreover, this aspect is incorporated in how the natives (i.e. the professors) treat the newcomer (i.e. the student). The tolerance that natives often show the ethnographer disappears once the interlocutor is recognized as no mere visitor, but, for better or worse, as a collaborator and perhaps ultimately a successor in the continuation of native culture. Because the stakes are at least this high for the student, the standards imposed on behavior and its interpretation are stricter. This point is important for one can easily confuse the polite tolerance of one's colleagues in other fields with genuine interdisciplinary negotiation. Students are treated more harshly than ethnographers because, in an important sense, the students are taken more seriously by the natives.

THOUGHT QUESTIONS

☙ Regarding knowledge policy and knowledge politics, Fuller refers to different kinds of sciences—plebisceince and prolescience among them—to make his arguments. Generally, how do Fuller's distinctions among different kinds of sciences support and clarify a conception of a social epistemology? What rhetorical difficulties does the social epistemologist face in using these distinctions in fashioning a policy and politics of knowledge?

☙ Where does Fuller locate philosophy with respect to the mission of social epistemology? To what philosophical and rhetorical traditions does social epistemology appeal?

☙ How is it possible to determine a "better" perspective on the conduct of science? If achieved, what rhetorical resources are needed to convince scientists to adopt the "better" view? Fuller claims that: "[A] rhetorically savvy normative theorist would multiply the probability that her intended audience can be persuaded … by the product and improvement that would result from being so persuaded and the probability that such an improvement would indeed result." What philosophical practices seem to blunt normative theorists' ability to persuade audiences? How might a theorist counteract the rhetorical impoverishment of normative philosophy and, hence, social epistemology?

☙ What does Fuller mean in suggesting that "advanced science" is incompatible with "maximum democracy"? What rhetorical difficulties does the social epistemologist face in aligning fragmented working and idealistic concepts of democracy and science?

☙ How does Fuller characterize the positions of Popper, Polanyi, and Feyerabend with respect to the governance of science?

☙ How does availability of information affect the possibilities of rational criticism? What special freedoms do scientists enjoy?

☙ How does Fuller characterize Willard's line of reasoning with respect to the evaluation of knowledge claims? How does this position differ from the one that Fuller advocates? How does intelligence testing serve as an example that counters or supports Fuller or Willard's position?

☙ What forms of liberalism support egalitarian forms of knowledge production? What accounts for the postmodernist's difficulties in rendering diagnoses on the functioning of the public sphere?

☙ How is competence or incompetence in a given professional field determined? What is the relationship between competence and expertise? What is the relationship between incompetence and nonexpertise? Does a lack of expertise preclude the possibility of rendering a competent judgment on the activities of an expert? What does Fuller mean by a "society of trust"? How might a universal sense of nonexpertise lend a basis for reconstructing the pubic sphere, hence public decision making about science?

☙ What is the "principle of epistemic fungibility"? How might this principle, in practice, help to select and shape the kinds of epistemic discourses—in, say, physics, sociology or philosophy—we might wish to pursue? What innovations to epistemic practice (e.g. academic disciplines) might follow as a result of adopting this principle? How does epistemic fungibility compare to the fairness doctrine? How might the fairness doctrine be applied by nonexperts' to judge of academic research?

☙ What is the difficulty in using history in assessing contemporary knowledge production? Can constructivist case studies provide a basis on which we can assess contemporary knowledge production?

☙ What roles do indifference, moral vagueness, and dogmatism play in crafting a rhetoric of knowledge politics? How do these ideas affect the process of social epistemology? Why are academics especially ill prepared for providing a public rendering of the value of their research projects? What rhetorical problems are embedded in the structure of academic communication? How would a "democratic rhetoric" overcome the deficiencies of academic rhetoric?

☙ How does a social epistemologist justify her existence to an audience of scientists? How is the social epistemologist's purpose similar to or different from the traditional purpose of philosophers?

☙ How is management considered differently in the modern and postmodern eras? How are historically changing conceptions of labor and management apparent in the performance of contemporary science? How does one persuade scientists to become social epistemologists?

☙ What roles might social epistemologists play in scientific controversies? What does Fuller see as the evolving role of psychology in a social epistemology? What difficulties confront a process of interdisciplinary mediation? Are the possible roles that social epistemologists occupy in a scientific controversy similar or possible in a process of interdisciplinary mediation? What kind of rhetoric is possible, or needed, to problems that are seemingly non-negotiable? How would the possibilities listed in the "continuum of rhetoric" be tempered by the principles of "epistemic justice"?

PART IV

SOME WORTHY OPPONENTS

9

Opposing the Relativist

Ever since Socrates first confronted the Sophists, philosophers have tried to defeat relativism on conceptual grounds as "self-refuting." However, most self-avowed relativists, from the ancient Greek Sophists to present-day sociologists of knowledge, advance their position on *empirical* grounds. Relativists, then, have not been moved by Socratic charges of conceptual incoherence. But this attitude makes their position *more* vulnerable, as well as more interesting, to the various empirical disciplines whose research can bear on the relativist's claims. In what follows, I argue that relativism is, on empirical grounds, an obsolete position for studying science in society. Moreover, relativism is obsolete *especially* if one wishes to derive a point of normative intervention based on such research. In making this argument, I elucidate the sorts of sociology that social epistemology countenances, and also settle the score with the problem of "reflexivity" that has traditionally dogged both relativist and normative projects and that occupies the imagination of STS.

THE SOCRATIC LEGACY TO RELATIVISM

That Socrates was the most artful Sophist of them all is a recurrent theme in the history of Western philosophy. The idea is that Socrates outwitted his sophistic interlocutors by using their own rhetorical skills. One trick in particular deserves mention. With only a hint of hindsight, we may say that Socrates managed to persuade his audience to treat *relativism* and *antirealism* as one and the same position. The audience confused the thesis that (epistemic or moral) standards are relative to a given locale with the thesis that standards are nothing more than what one says they are at a given moment. We suffer from this confusion today. (See Fuller 2003b, where antirealism is called *constructivism*.) Call it the *Socratic Conflation*. One finds evidence for Socratic Conflation in the way philosophy students are taught to interpret the Protagorean maxim, "Man is the measure of all things." Today, one often takes the "man" in the expression to mean the solipsistic individual, who is a standard unto himself ("true for me" truth). However, *anthropos* in its original Sophistic use referred to the "average man" in a community, in terms of whose standards one could tell whether one was in the right or the wrong.

As a dialectical strategy, Socratic Conflation converts relativism from a positive to a negative thesis. Thus, once Protagoras advises that when in Athens do as the Athenians do, Socrates interprets him to mean that when not in Athens one need not do as the Athenians do. Protagoras thought he was respecting local customs, but Socrates managed to portray him as cynically trying to appease the yokels. Socrates apparently obscures for future generations the possibility that relativism might be aligned with *realism*—that spatiotemporally indexed "facts of the matter" may exist. By successfully reframing Protagorean deference as cynicism, Socrates suggests that if a fact is *determinate*, it must also be *universal*. Moreover, Socrates managed to suppress the deep cynicism implicit in his own position. For as soon as Socrates granted the universality of standards, he denied that any particular native understanding of those standards was adequate. Indeed philosophy's task was to relieve the natives' confusion by informing them of the principles that had, all along, implicitly underwritten their sense of right and wrong.

Socrates' ability to make the Sophists look bad suggests a couple of interesting points about people's psychological reaction to relativism. First, relativism is not the attitude that people normally have toward their own beliefs. Relativism requires explicit cultivation, as when one engages in "disinterested" research into people's beliefs. For example, anthropologists typically have a clearer sense of the differences between their own culture and the cultures that they study than the natives of those cultures would. (Indeed, one might plausibly suggest that the discipline of anthropology could have only arisen in the West given, since the time of the Greeks, the fascination with its own cultural identity.) Similarly, David Bloor (1976) and Harry Collins (1981) are quite right in seeing sociologists of knowledge as "professional relativists." Second, people would prefer to think that universally shared beliefs or standards exist—even having only imperfect access to them—than to think that such beliefs or standards had merely local purchase on people's actions. In other words, then, what makes norms "normative" is not knowledge of their specific content, but the fact that everyone abides by them, whatever their content.

THE SOCIOLOGY OF KNOWLEDGE DEBATES: WILL THE REAL RELATIVIST PLEASE STAND UP?

Philosophers continue to reenact Socrates' original ruse when encountering relativists. For example, Laudan (1990: 74) caricatures the relativist as sliding from saying that *nature* does not determine theory choice, to her saying that *evidence* does not determine it, to her concluding that *reason* fails to settle matters. The relativist, then, is made to look like a skeptic and an irrationalist. Laudan makes his job easy by taking advantage of the rhetorical appeals that Harry Collins and other radical sociologists have made to Quine's thesis that data always underdetermine theory choice. By endorsing this thesis, the sociologists unwittingly buy into Laudan's *arationality assumption*, which provides a place for social accounts of science only once accounts based on "rational methodology" have been exhausted. The sociologists think that Quine supports their case because he seems to believe that the methodological accounts are *always* exhausted. However, this sense of exhaustion leads critics like Laudan to infer that relativists believe that the grounds for theory choice are always makeshift.

Now in Laudan's defense, the more radical "reflexivists" among the social constructivists *do* assimilate their relativism to a form of antirealism that opens them to the prior charge. The bluntest form of the charge comes as a *tu quoque*: If happenstance always resolves which theory should be selected, then doesn't this point also apply to the relativist's own account of science? To their credit, reflexivists such as Steve Woolgar (1988b) readily concede the point, but then try—in classic Pyrrhonian fashion—to convert their dialectical ambivalence into an instrument for destabilizing any presumptions the reader might have about how scientific knowledge is constructed. The "New Literary Forms" that Woolgar (1988a) and his colleagues in discourse analysis once pursued are Borges-inspired attempts to ensure that the reader's ruminations never reach a resting point. Thus, the reflexivists forsake the "cognitive" or "representational" function of language in favor of exploiting language's ability to provoke and interrupt thought processes. Whatever Laudan and other logically trained philosophers of science may privately think of this project's efficacy, they can, perhaps, respect its self-consistency: at last, relativists gladly eating their own words!

Unfortunately, however, the relativists that Laudan explicitly attacks—Bloor and Collins—are not antirealists; consequently, they have felt no need to exchange empirical assertion for more exotic forms of verbal expression. Given his Socratic view of relativism as antirealism, Laudan argues, perhaps unsurprisingly, with thinly veiled contempt against Bloor and Collins. Although, there is a sense (to be explained later) in which these relativists *do* deny that nature can determine theory choice, they most certainly do not deny that reasons can. Rather, Bloor and Collins restrict the scope in which any set of reasons applies. They hold that no *unconditionally* good reasons exist for selecting a particular theory. This view is normally called the *instrumental theory of rationality*: The justifiability of beliefs is relative to the epistemic constraints under which one operates—in particular, the methods available and the ends toward which inquiry is directed. But this explication puts us dangerously close to Laudan's (1996) own "normative naturalism." This view's attendant theory of rationality consists of a set of historically verified hypothetical imperatives.

Truth be told, one may argue that Laudan's instrumental rationalist is *more* of a Protagorean relativist than is the image of the scientist who emerges from Barnes and Bloor's Strong Programme in the Sociology of Knowledge. After all, Barnes and Bloor (1982) hold that instrumental rationality is fundamental to the human condition. Science, then, is simply a particular set of situations and utilities that frames instrumentally rational action at certain times and places. The Strong Programme's four methodological tenets—impartiality, causality, symmetry, reflexivity—

ensure that instrumental rationality can figure in the explanation of any human action if it could, in principle, figure in the explanation of *every* action (regardless of, say, our approval of the action's consequences). Laudan hardly aspires to such universality. However, this point is often obscured because Laudan selectively samples from the entire history of science for instances of instrumental rationality. But only a few figures and episodes are eligible to be drawn from each period. Included in Laudan's selections are people who, in retrospect, can be seen as having been driven by epistemically appropriate ends—in short, the progenitors we would have chosen as our own. Although a "culture" that encompasses both Newton and today's best scientists is more spatiotemporally diffuse than the paradigm cases of culture familiar from anthropology, Laudan's relativism here is unmistakable. By setting stricter conditions than his sociological foes for the presence of rationality in science, Laudan contributes to the image of science as a rather idiosyncratic human practice—the very image that one would expect from a relativist!

Yet general agreement suggests that Laudan scored a major rhetorical coup by avoiding all association with relativism. He succeeded by highlighting certain claims by Bloor and Collins that suggested the irrelevance of nature to the selection of scientific theories. Perhaps the most notorious of these claims is this oft-quoted one by Collins (1981: 54): "The natural world in no way constrains what is believed to be." From this quote, Laudan invites us to infer that Collins holds that we are so embedded in our social constructions that nature can never have any purchase on our beliefs. Now, even if Collins were saying just this, such a belief would not necessarily commit him to a social idealism or solipsism. On the contrary, this interpretation relates to a widely held belief among ethologists. On this view human beings, in comparison with other members of the animal kingdom, are sheltered from any direct contact with the forces of natural selection largely because we are encased in a socially constructed environment within which our behaviors are selectively reinforced. In fact, according to Byrne and Whiten (1987), the perceived complexity of the natural world may be little more than a function of the complex social relations in which one must engage to have access to nature. Such complexities are true whether one is talking about getting a bite to eat or getting a publishable scientific finding. Byrne and Whiten thus claim to be able to correlate primate intelligence with sociological complexity.

But we need to appeal to such a thesis only if Laudan has got his intended sociological targets right. However, the following quote from Barnes and Bloor (1982: 34) indicates that Laudan is off the mark:

> The general conclusion is that reality is, after all, a common factor in all the vastly different cognitive responses that men produce to it. Being a common factor, it is not a promising candidate to field as an explanation of that variation.

This formulation puts an entirely different slant on things. Nature cannot determine our theory choices because it is *always already* a component of those choices. Barnes and Bloor make this point in the course of arguing against a view often supposed by rationalists. Thus, scientists whose theories have stood the test of time were somehow in closer contact with nature than the scientists whose theories have not. Here Barnes and Bloor want to *oppose*, not support, the idea that epistemic differences reflect ontological ones, which implies that their relativism presupposes not antirealism, but realism.

INTERLUDE I: AN INVENTORY OF RELATIVISMS

The careful reader will notice that I countenance earlier at least three different positions that are legitimately called "relativism." For the sake of analytic clarity, I present the following inventory designed to show three different contexts in which relativism figures in opposition to some other position in science studies debates. However, over the past decade, I have come to be persuaded that *relativism, constructivism,* and *antirealism* share little more than a common opposition to the universalist version of realism that was common in the philosophy of science. This account was superseded in the 1980s by a "disunified" vision of scientific ontology (Fuller 2000b: Chaps. 6-7; Fuller 2003b; cf. Galison and Stump 1996). Yet in what follows, I continue to address these matters as if constructivism and antirealism were strongly related to relativism because relativism remains the philosophical lightning rod of STS. So, what might "relative" mean?

R1: *Local (vs. Universal)*: This is the relativism of Protagoras, Mannheim, and the Strong Programme. "Local" relativism presupposes realism in two senses: (a) a fact of the matter exists as to what is true and false, right and wrong, but this fact is spatiotemporally indexed, often specifically to cultures; (b) all of our thoughts and actions—not just the ones we deem true or right—are grounded in a reality independent of our conceptions, which serves, in Kantian fashion, to convert all questions of metaphysics to ones of epistemic access.

R2: *Indeterminate (vs. Determinate)*: This is the relativism of the later Wittgenstein and more moderate social constructivists of science. "Indeterminate" relativism is antirealist in the sense that no fact of the matter as to what is true and false, right and wrong exists until closure is brought to an interpretively open situation. These episodes of closure constrain the justification—although not necessarily the commission—of future action. They establish *conventions*. There are two general reasons that interpretively open situations might call for conventions: (a) a *surfeit* of competing interpretations, as in the variety of tradeoffs that can be made when no single theory maximizes all the relevant cognitive criteria or no course of action harmonizes the interests of all the relevant parties; and (b) a *dearth* of competing interpretations, as when certain conceptual (i.e. theoretical) distinctions fail to make any empirical (i.e. practical) difference, until practices are instituted—such as alternative experimental outcomes—that operationalize the distinction.

At this point, notice that one can possibly be both an (R1) and an (R2) relativist. For example, most moderate social constructivists, such as Collins, and Knorr-Cetina (1981), are (R1) relativists with regard to social scientific discourse (and hence are, after a fashion, "local social realists"), but (R2) relativists with regard to natural scientific discourse (and hence are "antirealists," in the sense that philosophers of science normally use the term). In practice, this belief means that these constructivists respect the integrity of science as a culture, but they refuse to privilege the scientists' own understanding of their culture. As Woolgar and other more radical constructivists have observed, this view suffers from a lack of reflexive consistency since (R1) clearly privileges the sociologists' scientific understanding of *any* culture.

R3: *Irrational (vs. Rational)*: This is the original relativism of Edward Westermarck (1912), Max Weber, and the logical positivists. "Irrational" relativism involves a *de gustibus non est disputandum* attitude toward values and captures the Pyrrhonian side of the reflexive social constructivists of science. In a backhanded way, this form of relativism presupposes a *deep* ontological distinction between what is real—and hence representable and cognitively accessible—and what is not. Values, for example, fall in the latter category because they allegedly rest on subjective choices and emotional commitments for which no independent rational grounding can be given. Verbal reinforcement (i.e. "ethics") and ritual then serve to routinize these commitments, which—from a more objective standpoint—may no better contribute to a society's survival than would some other combination of behavioral and verbal conditioning. However, the ultimate test of a morality is not what some outside observer thinks, but whether the insiders can "live" with its strictures.

As a point of reference, the history of anthropology has exhibited all three forms of relativism. (R1) reflects the "idiographic" commitments of orthodox ethnographic method pioneered by Franz Boas and still dominant among symbolic and cultural anthropologists. (R2) captures the reflexive ethnography that "inscribes the ethnographer in her own text" (cf. Clifford and Marcus 1986), and in that way removes the last epistemic vestiges of imperialism. However, in the process, this move may also eliminate anthropology's traditional object of inquiry—the self-contained alien culture. Finally, (R3) may be observed as structural-functionalist social anthropology (Malinowski, Radcliffe-Brown), especially in versions that stress discrepancies between the anthropologist's and the native's perspectives, as in the "latent functions" performed by seemingly irrational social practices. The skeptical side of constructivism results from a reflexive application of (R3), as becomes clear in my critique of Malcolm Ashmore's work.

INTERLUDE II: MANNHEIM'S REALISTIC RELATIVISM

Two German-Canadian sociologists, Volker Meja and Nico Stehr, have translated the debates surrounding the initial reception of Karl Mannheim's (1936) sociology of knowledge in Germany (Meja and Stehr 1990). Those participating in the sociology of knowledge disputes—Laudan, Bloor, Collins, and others—would be struck by several turns that the dialectic has taken since Mannheim first met his critics. Whereas today's sociologists of knowledge tend to define themselves as *opposing* philosophy, Mannheim usually tried to blur the difference between the two disciplines. In fact he displayed his sympathy with the classical philosophical aspiration to universal truth by explicitly opposing antirealist forms of relativism. Mannheim instead proposed the doctrine of *relationism*, which states that social conditions determine which truths are epistemically accessible. This doctrine was elaborated in a discussion of the social significance of the sort of synthetic thinking championed by Hegel.

According to Mannheim, Hegel was part of a generation that was in a position to pull together strands of thought that were left unraveled by earlier generations. Mannheim certainly did not consider the Hegelian synthesis as final. But he seemed to think that these ideas marked genuine progress that would not have been possible had Hegel not had specific precursors and had he not lived in the time and place that he did. The idea, then, seems to be Hegel's very own—universal truths may be glimpsed only at certain moments in history. To put it as a question: *If there are, indeed, universal truths, then why have we not always known them?* Interestingly one can read Mannheim, as did some critics, as claiming that if one takes *very seriously* the idea that certain things are true for all times and places, then sociology of knowledge simply takes up the traditional tasks of epistemology by explaining the differential access that people living in different times and places have had to those truths.

Mannheim's critics raised doubts about whether the sociology of knowledge was equipped to subsume the philosophical enterprise of epistemology. In retrospect, Mannheim's strategy seemed much like Quine's (1985) "naturalization" of epistemology. Both held that the relevant special science—be it sociology of knowledge or behavioral psychology—can subsume epistemology by showing that the sorts of positions that traditionally distanced epistemology from the sciences (i.e. absolutism, foundationalism) are empirically untenable. Perhaps more than Quine, Mannheim took this idea not as a capitulation of philosophy to the special sciences, but rather a consistent application of philosophical reasoning to the point of transcending the disciplinary boundary separating philosophy from the special sciences. (After all, does not the institution of philosophy in the 20th century—not only philosophical thought—clearly demarcate philosophy from the sciences?) Indeed Mannheim periodically cast his own interest in the "existential connectedness of thought" as continuous with Heidegger's search for existential structures in *Being and Time*. In this way, Mannheim managed to answer most of his critics' charges of relativism.

However, Mannheim failed to stave off the concerns raised by his Frankfurt School critics, Herbert Marcuse and Max Horkheimer (Meja and Stehr 1990: 129-57). They located Mannheim's latent relativism in the sociology of knowledge's failure to specify the sense in which a form of thought "reflects" its social conditions. After all a body of thought, such as Marxism, may be very much a product of its time. Such ideas, however, may serve to radically transform the social order, not merely reproduce it, and so enable a completely different sort of thought to be generated in the future. Still Mannheim's implicit sociological functionalism dampened the prospect that substantially different consequences might follow from the political options available in a given time and place. Not surprisingly, then, Marxists have tended to distrust the surface radicalism of the sociology of knowledge as masking a politically quiescent worldview.

IS RELATIVISM OBSOLETE?

The Frankfurt School's political dissatisfaction with Mannheim's sociology of knowledge can be analyzed in more strictly epistemological terms and generalized to other forms of relativism. Claiming that a knowledge system is adapted, or "existentially connected," to its social context suggests that people exert considerable control over their thought processes—probably more than is warranted by the psychological evidence concerning human cognitive biases and limitations. For example, setting aside cultural differences that are marked primarily on racial grounds, what is striking about the phenomenon of cultural diversity is just how *invisible* it is to most people most of the time. Consequently, when anthropologists try to get the natives to reveal their local customs, the natives often find themselves attending to their behavior in unique ways. Indeed when "going reflexive," anthropologists begin to wonder whether they might be subtly coercing the natives to draw distinctions where none exist. This observation does not deny that laying claim to cultural identity and difference is a pervasive social practice. Rather, I question whether the practice amounts to anything more than a mobile rhetoric deployed on various occasions to achieve various ends. Thus, although the average anthropologist knows enough to put the native's distinction between "good magic" and "bad magic" in scare quotes, she has yet to learn that the same policy should apply to the more seemingly fundamental line dividing "them" from "us."

If the idea of the rhetorical character of cultural differences is correct, then Mannheim's question should be turned on its head. Instead of explaining what appears, from the inquirer's standpoint, as the *real diversity* of beliefs, the deeper concern should be with explaining the *apparent uniformity* that different believers experience (or, rather, presume). Recall the realist epistemology that motivates Mannheim's enterprise: If one reality, or nature, exists with which we are always in contact, what explains, then, the difference in access to that reality as implied by the existence of alternative knowledge systems? Now let us turn the tables on Mannheim's realist presumption by subjecting it to the same test of epistemic access: If there are indeed deeply diverse knowledge systems, which nevertheless affirm a belief in a common reality, why then should we think that instances of such a belief imply the existence of such a reality? For if the mere existence of one world were sufficient to cause different people to experience a world that they presume others also to experience, then there should be no diversity at all. However, the fact that diversity exists suggests that people unwittingly presume different worlds of one another. These differences can be best seen at the group level in the form of spatiotemporally grounded "cultures." The mechanism at work here may be a generalization of the argument made in *Social Epistemology*: *The illusion of epistemic agreement is maintained by a failure to detect real differences that emerge in the process of knowledge transmission.*

From an epistemological standpoint, Mannheim's all too easy "adaptationist" approach to the role of knowledge in society is the product of two distinct conflations: (a) between a culture's system of beliefs and its beliefs about those beliefs; and (b) between the consequences of one's beliefs regarded abstractly as a system of thought and the consequences of one's beliefs regarded concretely as the product of linguistic transmission and other forms of social interaction. In the case of (a), the inquirer's "clarity" about a culture's system of beliefs may give a highly misleading picture of what members of the culture make of those beliefs. The image is further occluded if the "metabeliefs" of the inquirer and the culture differ sufficiently. Thus, Mannheim and other methodological relativists fail to consider why they alone (and not the cultures they study) enjoy the privilege of being relativists. The case of (b) points to Mannheim's tendency to ignore the material, unintentional (sometimes counterintentional) character of knowledge-based action. This point highlights the empirical ambiguities involved in trying to demarcate a region of space-time "relative" to which a certain knowledge system is "legitimate" or simply just "operative."

Both (a) and (b) appear most noticeably as a blind spot about the critical role of intellectuals in society. Thus, Mannheim was prevented from appreciating the Frankfurt School's normative project. More broadly, because the relativist thinks of culture as a historically and geographically well-bounded unit, every epistemic standpoint must be either "inside" or "outside" the culture under study. The former is said to be "naive," the latter "critical." Taking the metaphor of standing "outside" a culture to its most literal extreme, Mannheim (1940) ultimately characterized the intelligentsia as "free floating." Although not normally regarded as the most realist or materialist of Marx-inspired intellectual movements, the Frankfurt School's reliance on a reflexive or embedded conception of critique offers an antidote to Mannheimian relativism.

Here is what I take the Frankfurt critique of relativism to be: On realizing that knowledge is embodied in action (or, more precisely, in the disposition of people to act), and that action has consequences that transcend the original agents' intentional horizon, one can gain critical leverage over the members of one's own culture by virtue of having come *after* them in history. Of course this circumstance does not preclude the possibility that today's critic will be surpassed by one in the future who can comprehend the first critic's blind spots. The point is, rather, that one cannot underestimate the epistemic advantage that accrues to someone who stands at the end of a sequence of events. Sometimes in a Popperian vein, this state is said to enable one to "learn from mistakes." But this way of putting matters is too strong. It suggests historically invariant performance standards, completely accurate recall, and other implausible assumptions. More modestly, the critic need only say that she sees things her predecessors did not. In any case, the relativist's burden is to explain how history is incorporated into societies that have existed for any length of time. That is to say, relativists typically forget to include a notion of *institutional memory* (Douglas 1986) in their conception of culture. They, as a result, tend to treat all moments in the history of a culture as epistemic equals.

One conclusion that emerges from this argument is that something empirically misbegotten goes on in epistemological disputes between relativists and realists or rationalists. Do particular communities devise standards for evaluating knowledge claims? The answer is, of course, yes. But, *pace* relativists, what does not follow is that those standards are used primarily to judge current members of that community. In other words, the context of *evaluation* and the context of *conduct* are quite different. If one is already a member of good standing in the community, then charity is more likely to operate in interpreting any disparity in the person's behavior. Thus, outrageous sounding hypotheses may be entertained by a scientific community a little longer when a PhD utters them than when a mere BA does. However, if one has yet to prove oneself, then stricter, more "official" standards of evaluation apply. Under those circumstances, accidents and innovations may be seen as products of ignorance and error. As our critique of Mannheim suggested, such official standards also figure in judgments made about one's predecessors. In any case, these standards may well be quite different from the norms that implicitly govern the behavior of the community's own members when they are not under especially tight scrutiny.

Since a community's official standards tend to be used to judge various sorts of people who had nothing to do with their design or ratification, the standards achieve an aura of "independence" that gives heart to the realist—especially if a very wide array of people are so evaluated. Here the relativist rejoinder is on target: "Independence" in this sense mainly reflects an absence of resistance to the evaluation made of the groups in question. Whether anything else is happening remains to be seen. Of course many possible reasons exist for this lack of resistance, including the relative powerlessness of the groups in question and the indifference of those who are in power. (Who ever speaks for the past but zealous exegetes?) But such powerlessness and indifference should never be confused with outright acceptance of an evaluation (Fuller 1988a: 207-32). From an empirical standpoint, the battle between relativists and realists is most fruitfully seen as being about how people come to speak for other people—not necessarily themselves.

A crucial antirelativist assumption in the foregoing analysis is that the principles governing a society need not coincide with actors' construals of what those principles are. This belief seemingly commits me to an especially virulent form of sociological realism—*eliminative sociologism*, as patterned after Paul Churchland's (1979) anti-psychologistic "eliminative materialism." Put another way, a fact of the matter exists about a society's epistemic practices that may elude that society's members. Consequently, members of the society may normally act on the basis of an empirically false "folk sociology" that functions as a kind of "false consciousness" (Fuller 1988a: App. B). The social sciences, apart from cultural anthropology, typically justify their existence with a claim of this sort. In any case, epistemologists must explain how knowledge producers continually do things with which other knowledge producers find fault—whether an error, a failure to persuade, or simply a failure to communicate.

One plausible way to cast this situation is to identify epistemic practices much like stock market trends: They are constituted in the course of being anticipated or "guessed at." The guesses pertain to what other relevant people will guess. As feedback from the guesses is often delayed and imperfect, the market displays considerable volatility. Such instability would lead to complete financial collapse if the government did not insure the legitimacy of the transactions.

This "Keynesian" perspective helps justify the office of epistemologist as someone who does something useful that individual knowledge producers or knowledge-producing communities could not do themselves. Moreover, we need a Keynesian—rather than a strictly socialist—approach to knowledge production. That all the knowledge producers do not have the same sense of what the epistemic practices are (indeed none may have a particularly good grasp) does not prevent the emergent result of their activities from turning out to have good epistemic effect. Yet to say that the knowledge enterprise often works by means of an "invisible hand" is not to downplay its social character. To the contrary, if everyone had the same epistemic practices, then one could study a randomly selected individual to understand how the entire knowledge production process works.

The last point, while seemingly obvious, nevertheless cuts against the desirability of a political stance traditionally associated with the brand of relativism advocated by Jean-Jacques Rousseau and Paul Feyerabend. The stance goes by a number of equally misleading names, including "libertarianism," "anarchism," and even "democratic communism." However, the outlines of the view are clear enough. Communities are portrayed as voluntary associations. They are sufficiently well bounded—perhaps even spatiotemporally isolated from other communities—so that both the possibilities and the outcomes of actions can be surveyed by their members. In this case action gets treated as a projection of the collective beliefs and desires of the community. On this view, if a community's actions have unforeseen negative consequences for other communities, then apparently the community in question is either too large or, at least, is having an impact on non-consenting members. The proposed remedy is for the community to restrain itself in some way, perhaps by splitting up into smaller, more homogeneous units that can survive without unwittingly involving the lives of others.

The flaw in the politics of relativism is twofold. It, on the one hand, overlooks the point that apparent uniformity in beliefs can mask real diversity that, when finally articulated in the political arena, turns out to be a major source of "betrayal" and "disappointment" (Hirschman 1982). Ostracism would become a routine activity, as in the Greek city-states. On the other hand, the politics of relativism neglect the fact that other people with beliefs radically different from one's own can do things in remote times and places that end up limiting, if not jeopardizing, one's own ability to act.

As sociologists turn increasing attention to the "globalization" of the human condition, some interesting analyses arise for the persistent popularity of relativism. One finding suggests that the "reactive" character of relativist epistemology and politics is partly born of resentment and partly of nostalgia. Additionally, these diagnoses point to the relativists' sense of losing control of their own fates to forces that they do not fully understand. Thus, Wallerstein (1990) interprets 19th-century nationalism, with its emphasis on a historically segregated, geographically bounded "homeland" or "society," as a backlash to the homogenization processes of the capitalist world system. Moreover, as Sztompka (1990) suggested, relativists try to foster the illusion of distinct peoples with distinct causal lineages by artificially maintaining local modes of understanding long after contact with other cultures has rendered them obsolete. Indeed the evolution of trade languages ("pidgins") into more generally applicable forms of communication might prove a useful source of models of how people from different communities come to understand, accept, and express their common fate. This ecologically minded ethic, rather than respect for local sovereignty, may foster the mutual calibration of interests and standards that characterizes the global consciousness appropriate for our times.

Previous criticism aside, a certain form of relativism is in fact quite necessary for "the pursuit of truth"—in the way a realist might understand that expression. First is Stich's (1990) point. Given the infinite truths possible for any domain of inquiry, to urge that one simply "maximize the truth" offers no guidance for action because virtually anything one might do complies with this injunction (including covering up errors in the short term in the hope that they will cancel each other out as one approaches the truth). Indeed the truths that philosophers generally regard as epistemically most valuable often emerge as the unintended consequences of attempts to satisfy local interests. Specifically, these results come out of the resistance that comes from the mismatching of means to ends (Popper 1972). One might call this notion the *counterpragmatic* or *disutilitarian* theory of truth. As a social phenomenon, truth first appears as individual disutility, but ultimately contributes to the maximization of group utility. Thus, *if everyone benefits from one person's error, then a truth has been produced.*

Should the naturalized epistemologist remain interested in prescribing methods for maximizing truth acquisition, then she should focus her energies on designing *knowledge maintenance systems*. Such systems can disseminate relevant information about an individual's error to those members of her group who are likely to be in a similar situation in the future. This information, in turn, improves what management theorists call the community's "living organizational memory." The sphere of computer software engineering known as "knowledge acquisition" already designs systems of this sort for such undervalued epistemic communities as the telephone company. I have laid some foundations for this field of "knowledge management" (Fuller 2002a).

Thus, although the pursuit of truth is best understood as a social practice, I do not draw from that the relativist conclusion that any social practice has to be accepted as it is. In fact practices advertising themselves as pursuing "truth for its own sake" may be the very ones whose social organization is most epistemically suspect, since they do not receive enough external resistance. One need not impute conspiratorial thinking to the forces of Big Science to observe contexts in which the rhetoric of autonomous inquiry has transformed even *de jure* realists and rationalists into *de facto* relativists.

One context is the widespread belief among science policy advisors that if an expensive scientific project does not actually harm the citizenry, and offers the vague hope of beneficial technologies, then it deserves, *ceteris paribus*, to be maintained at current funding levels. Another context is the subtle tendency of scientists to acquire "adaptive preference formations." These preferences arise as scientists identify the epistemically relevant aspects of their craft with aspects over which they have relatively direct control. Still these features can be manipulated by the political environment in which scientists find themselves (Fuller 1989: 161-62). Thus, would scientists continue to see such a sharp difference between the "intellectual" and the "economic" value of research if they were solely responsible for raising and distributing their own capital?

Both of these rhetorical contexts impede the pursuit of truth by encouraging inquirers to turn a blind eye to their social setting. The remedy requires social practices that counteract these rhetorics in the strategic manner of someone who truly believed that knowledge is a product of its social organization. Perhaps a good name for this remedy would be *counterrelativism.*

COUNTERRELATIVIST MODELS OF KNOWLEDGE PRODUCTION

General Ways of Thinking About the Interpenetration of Science and Society

In *Philosophy of Science and Its Discontents*, I advanced some proposals for overcoming the idea that the "cognitive content" of science is something other than its "social context." I tried to make good on Bruno Latour's (1987) insight that science has incorporated all of society into its networks. Science so fully intertwines with society that to claim that science is done only by laboratory technicians is just as misleading as to claim that finances are transacted exclusively by bank tellers. To capture the totalizing character of science, its sociocognitive identity, I suggested that the variety of solutions offered to the mind–body problem provides the appropriate model for understanding the possible interrelations of "cognitive" and "social" factors in science.

In terms of the mind–body debate, most philosophers and sociologists remain "dualists" of some sort. They presume cognitive and social factors to be separable entities. Thus, philosophers imagine that knowledge and reason subsist independently of any social embodiment, whereas sociologists still tend to see knowledge and reason as the epiphenomenal projections of social factors. By contrast, few confessed dualists remain these days in the philosophy of mind. Instead one finds *functionalists*, *reductionists*, and *eliminativists*, all of whom believe that mind is, in some sense, a property of certain arrangements of matter. Likewise social epistemologists hold the correlative view that knowledge and reason are ways to embody certain kinds of social relations. Consider these possibilities that follow from pursuing the mind–body analogy:

Functionalism: Any of a variety of social structures—although probably not all—can instantiate a given cognitive relation. For example, we are psychologically ill disposed to falsifying our own theories. Popper's falsification principle, then, would unlikely be instantiated in individuals. Equally unlikely is that criticism will be effective if individuals do not receive prompt unequivocal feedback from people whose judgment they respect. Cognitive relations, therefore, are sensitive to the spatiotemporal dimensions, or *scale*, of the social enterprise in which they are embedded.

Reductionism: The categories of social and cognitive accounts of science diverge simply by not having been developed in conjunction with one another. Just as psychological states can be more closely monitored and refined if we attend to their physiological correlates, so too the cognitive character of science may lose its seeming disembodiment if we attend to the social circumstances in which cognitive claims are made.

Eliminativism: Cognitive categories are the vehicles by which scientific claims and practices are officially justified. However, those claims and practices can, in fact, be best explained and predicted solely based on social categories. For example, one explains the widespread acceptance of a scientific theory not by a common perception of an underlying reality, but rather by examining the social mechanisms of belief acceptance. These mechanisms may be quite heterogeneous across cases, leading ultimately to a denial that some common cognitive content existed on which all sides agreed.

The first social epistemologist to break away from the dualist mindset was Karl Popper. He clearly envisaged scientific rationality not as a detachable abstract logic, but as an embodied community of "conjecturers and refuters." Interestingly, this more monistic mindset enables one to think of science either as transpiring throughout society (e.g. each purchase you make helps decide between rival economic theories) or as a site for reproducing all the major institutions in society (e.g. science contributes to both family breakdown and capital development). These options are in addition to the two that will be raised in the next subsection: to wit, the complementary character of *standardization* and *incommensurability* in knowledge transmission. Thus, on the one hand, society is reconstituted in the image of science through the introduction of standards for speaking and acting correctly. On the other hand, science acquires the marks of modern society's own diffuseness as science is communicated from context to context (see Fig. 9.1).

All four of the prior options, then, are distinctive contributions of STS. If we take them all equally seriously, then any residual "ontological" difference between something called "science" and something called "society" should disappear.

	SCIENCE SOCIALIZED	SOCIETY SCIENTIZED
SCIENCE/SOCIETY AS PLACE	Science is the site for reproducing social institutions	Society is the testing ground for scientific theories
SCIENCE/SOCIETY AS PROCESS	Science becomes diffuse as it crosses social contexts	Society becomes standardized as it comes under the rule of science

FIG. 9.1 How to make the science-society distinction disappear.

A Model of Knowledge Production Specific to Social Epistemology

Social epistemology requires an appropriate conception of the "social." In *Social Epistemology*, I pursued this issue in terms of an expanded reinterpretation of the incommensurability thesis. Unlike most recent philosophers of science, I believe that incommensurability is a real and unavoidable feature of modern knowledge systems. Indeed I regard the incommensurability thesis as Kuhn's most important contribution to the understanding of science. I hold that the main source of conceptual change is the emergence of undetected differences in the way words are used, which is in turn a natural consequence of the expansion and proliferation of epistemic communities. In short, as the knowledge system grows, normative control becomes diffuse, as it is exerted less through face-to-face interaction and more through the evaluation of official accounts. This ascendance of the written over the oral display of knowledge enables the standardization of scientific discourse at the cost of permitting an enormously wide range of activities to travel under a common rhetoric. This explains, for example, the appearance of "consensus" that characterizes a scientific community during its "normal" phases. Here my point reinterprets a cardinal tenet of scientific realism—namely, if a theory is true (or true to some extent), its truth (or the extent to which it is true) is the best explanation for its acceptance by the scientific community.

Why do realists take a theory's truth—insofar as the theory is true—as the best explanation for its acceptance? The concept of *acceptance*, like that of justification, presupposes a broader range of scientific contexts than the ones involved in the theory's "discovery." Since these contexts are generally embedded in rather diverse social circumstances, realists suppose that the theory must have some "content" that remains true across these circumstances. Moreover, invariance of this sort is often taken as necessary for the transmission and growth of knowledge. However, this line of thinking confuses the uncontroversial claim that the truth does not change with the more controversial claim that the truth is transmitted intact by reliable linguistic means. Whereas the latter claim is about the *medium* of communication, the former is, so to speak, about the *message* it conveys. Let us assume that the linguistic means at our disposal to transmit truths over time and space is less than reliable. Whatever invariance we then find in scientific theories accepted across sociohistorical contexts is not likely due to the invariant nature of the truth transmitted, but rather to cognitive mechanisms that mask the differences in interpretation that would have naturally resulted from the theory being *un*reliably transmitted to different times and places.

What I am doing here is reflexively questioning the fundamental assumption of Karl Mannheim's (1936) sociology of knowledge. Mannheim, as we have seen, is often mistaken for one of those self-defeating relativists that philosophers since Socrates have loved to criticize. But he, in fact, saw the need for a sociology of knowledge arising from the following mystery: If there is indeed one reality or nature with which we are always in contact, what accounts then for the differences in access to that reality implied by the existence of alternative knowledge systems? Again let us turn the tables and subject Mannheim's realist to the same test of epistemic access: If deeply diverse knowledge systems exist, which nevertheless affirm a belief in a common reality, why then should we think that instances of such a belief imply the existence of such a reality?

My model of the previous process—one in which the transmission of knowledge content is not invariant, but diffuse yet undetected—involves what cognitive psychologists now see as an interaction of *hot* and *cold* mechanisms in the knowledge process (Elster 1983; Elster 1985: Chap. 8, discusses this in the context of Marx's theory of ideology). Hot mechanisms are interests and passions that externally drive, or "bias," rational cognition (sometimes to self-destruction), whereas cold mechanisms are such internal cognitive liabilities as fallacious reasoning skills and limited memory capacity. By analogy, one can envisage these mechanisms operating on entire scientific communities. Here is an example.

Start with a scientific theory like Newtonian mechanics. What Newtonian theory primarily transmits to various research communities is a common language for transacting knowledge claims. In each community, however, differences exist in what counts as appropriate and inappropriate applications of the language. These differences reflect the local interests of the research communities, which may be quite far afield from the intended applications of the original Newtonians. Additionally, these differences would result from "hot" social mechanisms and would be conveyed in teaching and other face-to-face, oral transmissions within the theoretical language. Still each community will also have an interest in linking up its research with that of other communities and perhaps even with that of other periods (especially if there is a need to establish a “tradition”). Scientists will therefore jump at the opportunity to draw comparisons and analogies—a relatively easy task given that Newtonian mechanics is the theoretical language common to all the communities. The facility with which these similarities can be found, however, coupled with the need to budget one's efforts between doing one's own research and finding such similarities, will lead the scientists to neglect important local variation in how the Newtonian language has been applied. This dilemma will end up covering over real differences in these scientists' thinking. Such a problem is probably fine for legitimation purposes, but not for keeping an accurate record of the growth of knowledge. Thus, the need arises for the “cold” social mechanism, which may be found in written transmissions within the theoretical language.

Relativism Revived: Can Social Epistemology Survive the Reflexive Turn?

The status of “reflexivity” in STS is similar to that of “political correctness” in scholarship more generally in the 1960s. Both notions aim to situate the inquirer as an integral part of the world in which inquiry takes place. Both are ethical stances reminiscent of the Golden Rule or Kant's categorical imperative. The common intuition is that we should not apply analyses to others to which we would not first submit ourselves. Among the most artful and conscientious practitioners of reflexivity in science studies is Malcolm Ashmore (1989). Ashmore takes social epistemology to task for first deconstructing the cognitive authority of science, then apparently appropriating the very same authority for itself. This authority apparently lends a basis for suggesting ways that social epistemology can be “applied” to improve knowledge production in society. This criticism has been the most frequent and substantial made of social epistemology. In Ashmore's hands, however, it takes on a generality that enables us to see the philosophical sensibility that motivates the reflexive turn.

Ashmore’s (1989) two objections to social epistemology are really the negative and positive sides of the same point. Both turn on the social constructivist cast of STS research. This depiction reveals the inconclusive character of all claims to knowledge. Epistemic closure is always contingent and reversible—sometimes, seemingly, simply by providing an alternative account. Yet if social epistemology draws on this research, then it cannot be for purposes of “application.” To apply something presupposes that one has something conclusive to apply—which is exactly what STS appears to deny. The positive side of this point is that STS actually sets out to do something quite antithetical to social epistemology—namely, to call into question the very idea of knowledge, understood as a privileged representation of some reality outside itself.

Worth noting, at the outset, is that the form of critique that Ashmore calls "reflexive" is not necessarily politically the most interesting or potent. (I will address that form of critique at the end of this chapter.) Nevertheless, to his credit, Ashmore presents an empirically informed version of the problem of philosophical skepticism, which does suggest complete generality. Ironically, however, his reflexive critique would seem to have bite only for someone who is *not already* a constructivist. After all the classical skeptic is a disappointed realist—someone who seeks the truth, but believes that, in the end, all appearances may be *false*. By contrast, Ashmore's constructivism is antirealist; it aims to show the indeterminacy of the true–false distinction and, hence, as noted before the impossibility of social epistemology's "applying" anything from STS. Unfortunately, Ashmore's own reflexive strategy involves a second-order *application* of a first-order concern. That is, if Ashmore wants to show that the stuff out of which something is constructed is itself constructed, then he must presume that the meaning of "constructed" remains univocal across the two contexts in which construction is said to occur. But this presumption violates the constructivist point that no univocal meaning of "constructed" exists, only the meanings constructed from context to context. One interpreter's sense of paradox may be another's idea of distinct contexts of utterance. The first would be a reflexive realist, like the classical skeptic; the other the unreflexive constructivist. Ashmore's "middle way," the reflexive constructivist, should be seen not as standing between these two alternatives, but as its own orthogonal axis. He offers a plea to change the genre in which knowledge is written about from fact to "fiction," the realm that philosophers typically identify as consisting mostly of sentences whose truth values are indeterminate. Although perhaps eloquent or prudent, such pleas, in refusing to assert or deny, cannot *refute* the possibility of making knowledge claims.

Ashmore's attempt to escape realism also threatens the empirical cast of social constructivism. In describing the "openness" of our epistemic situation, reflexive constructivists tend to run together such notions as "historically contingent," "essentially arbitrary," and "logically possible." From reading this literature, one gets the impression that changing one's worldview would result simply by saying something different (Woolgar 1988b). I do deny that one's understanding can be changed substantially, but it cannot be changed into just anything else at any moment. Some possibilities for epistemic change are more immediate than others because of the changes that would need to be made in collateral practices. Indeed STS has provided much information about the nature of these possibilities largely by revealing the interdependent networks necessary to keep any social practice in place (Callon, Law, and Rip 1986; Latour 1987).

Standing back from Ashmore's particular critique, we see that reflexivity has been integral to most dynamic accounts of the history of science. Put most simply, a reflexive system applies something it has learned about its environment to its own internal workings. STS, then, differs from earlier accounts of science in terms of the general character of what it takes science to have learned. In many ways, these differences capture the issues at stake in the *modernist–postmodernist dispute* that cuts across the human sciences. On the one hand, such 19th-century "modernist" theorists as Hegel and Comte took science to be discovering order in the world—an insight that can then be applied to regulate science's own development. On the other hand, following Lyotard (1983), the "postmodernist" science practiced by STS has discovered disorder in the world, *especially* in the world where science is practiced. The reflexive histories told by Hegelians and positivists are of ever better methods that enable greater prediction and control of the environment. These stories are of increased *closure* and *inclusiveness*. By contrast, the reflexive histories told by STS are of ever greater discrepancies between universal principles and situated practices that are patched up in ever more opportunistic ways. These stories are of increased *openness* and *dispersion*. (Social epistemology, one might say, aims to "square the circle" of maximizing *both* inclusiveness *and* openness.) The reflexive consequences of modernism and postmodernism may be compared along three dimensions in terms of the ways they *broaden, deepen*, and *limit* the scientific enterprise.

To *broaden* science reflexively is to apply to all of science what has been learned about one science. This idea states logical positivism's "unity of science" thesis a different way. Crudely put, if a model or method enables order to be elicited in one domain of inquiry, then it should be extended to all domains. An example is the ubiquity of mechanistic models and experimental methods once they were shown to succeed in physics. In this way, the positivists and other modernists thought that laws governing each domain would be forthcoming. The postmodernists also have their version of the unity of science thesis, which may be called *panconstructivism*. If the appropriateness of a given attribute to a given case must always be negotiated by social actors, then in principle at least nothing prevents a nonhuman from being socially constructed as, say, a "scientist" or even a "person." Conversely, withholding rights and responsibilities from a computer is just as much a political act as withholding them from a human. The only difference, according to the panconstructivist, is that whereas suppressed human voices often find someone to speak on their behalf, computers typically do not. However, a general strategy for granting nonhumans voices would be to treat the technological interfaces between ourselves and the nonhumans as media of communication instead of control. A cloud chamber would thus be a means for communicating with microphysical particles, and not simply for tracking their motions. One paradoxical consequence of this version of broad reflexivity is that postmodernists now face the prospect of investing everything with personhood—a veritable "sociology of things"—and, in the process, diminishing the value of being a person. Indeed this consequence seems deliberate. Postmodernists have often remarked on the discriminatory consequences that ontologically inflated criteria of personhood have had not only for nonhumans, but also for humans who failed to meet those criteria by not looking or acting in the right ways. In short, devaluing personhood might be one of the best things to happen to people!

To *deepen* science reflexively is to divide a domain into parts that are then analyzed by the same principles originally used to study that domain. This process is familiar to modernists as the division of cognitive labor into special disciplines. This process typically involves adapting general principles, techniques, and instruments to ever more specific objects. For example, an experimental task that was originally used to test the problem-solving ability of humans in general can be refined to capture differences between, say, scientists and nonscientists, or men and women. The experimental method is not abandoned, but intensified. For their part, postmodernists become reflexively deep by intensifying the openness of their inquiry. In doing so postmodernists typically highlight the discrepancies in perspective that are already latent in any situation that is defined by more than one person. This action goes beyond, say, the ethnographer noting discrepancies in behavior. Indeed in this instance, the ethnographic method comes under attack for privileging the ethnographer's account of an episode at the expense of silencing the perspectives of those who participated in defining the episode. In that regard, the classical ethnographer is no less authoritarian than the experimentalist who dismisses her subjects' accounts of their behavior during an experiment. The reflexive remedy is to articulate the alternative perspectives without any attempt at resolving their differences. Ironically, such evenhandedness in representation often costs postmodernist research some credibility. This approach implies that the postmodernists are contributing to a body of factual knowledge, rather than to a "new literary form," as some of them describe their work.

Finally, the rhetorically most instructive feature of the reflexivity literature is its attempt to *limit* the scientific enterprise. The aspiring theorist formulates a position so that it covers her to the exact same extent as the people about whom she is theorizing. Thus, in its limiting mode, reflexivity demands that theorists think more democratically than habit allows. After all theories are usually developed on the assumption that the theorist occupies a privileged vantage point, as indicated by the special language she introduces—a language that makes either too little or too much sense to the people under study. In the former case, theory fails to descend from the ivory tower; in the latter case, theory strips people of their illusions, often with little to replace them (e.g. when Marxism or Freudianism are taken to heart). By contrast, reflexively adequate theories should be ones with which and from which both the theorist and the theorized can live and learn.

But is social epistemology irreducibly social and, so, without anything valuable to say to individuals? Critical Legal Studies theorist Roberto Unger

(1986, 1987; cf. Fuller 1988b) fully grasped the deep issue here, one that plagues any "scientifically" based social theory. If the theorist uses science to show that ordinary people radically misperceive the causes of their plight—causes largely beyond their immediate control—then in what sense can such a revelation free people from their chains? More than any other social theory of the modern period, Marxism has been dogged by this problem of calibrating explanations of the past with guidance for the future. Deterministic materialism cannot plausibly yield utopian idealism no matter how cunning the reason or how bloody the revolution. Moreover, the problem is not merely at the level of ontology, but also at the level of epistemology. Given human cognitive limitations, mastering the intricacies of the Marxist account of capitalist oppression will likely force one out of the arena of everyday life in which revolutionary practice ultimately takes place. Once the account has been mastered, the theorist realizes that ordinary language is a grand mystification to be deconstructed or simply avoided, but certainly not to be used to persuade the masses. Indeed such theory-induced political incapacity can result in contempt for the very classes with whom theorists are supposed to understand and even identify. This fate has befallen the Frankfurt School, the brand of Marxism that has managed to flourish most consistently in the academy.

The reflexivist diagnosis that Unger, himself both a Brazilian activist and a long-time Harvard law professor, provides of Marxism's failure as a political practice is that Marx formulated his theory by working in libraries or at home, by himself or in collaboration with Engels. In short, Marx merely theorized *about* people, but did not theorize *with* or *for* them. If the latter two prepositions had come more into play, Marxism would have been a totally different theory. Marxism may well have been adapted for its intended audience—workers lacking any formal training in either Hegelian dialectics or Ricardian political economy. Sometimes theorists think that the reflexive adequacy associated with rhetorical effectiveness would amount to "dumbing down" a theory to "manipulate" the audience. When lodging such a complaint, theorists often conflate signs of a theory's incomprehensibility with signs of its radicalness. As a result, a theorist may not deem her theory fit for ordinary ears ill equipped to hear the truth. Under those circumstances, reflexivity sounds like a call to either self-destruction or bland moderation in one's theoretical utterances. However, in truth, reflexivity calls for overturning the politics of theorizing by making theoretical language less authoritarian and more negotiable—that is, for theorizing to be done from a third- to a second-person perspective (Fuller 1988a: Chap. 11). Thus, social epistemology begins its descent from the Platonic heavens down to the agora where it belongs.

So, then, what concrete courses of action can the social epistemologist take to reveal her sensitivity to the dimensions of reflexivity just outlined? The social epistemologist wants to remain committed to the modernist ideal of an empirically informed, theoretically progressive social science. Yet the possibility of an integrated science studies depends, concurrently, on her addressing the postmodernist challenge posed by STS' sense of reflexivity. Here are some brief suggestions, covering each of the three senses of reflexivity:

> *Broaden*: Instead of, say, conceptually prejudging the issue of whether computers can be scientists, decide the issue empirically. For example, one can put, say, an expert system in a scientific setting and see how often and under what circumstances people who are recognized as scientists come to rely on the computer's judgment. As in the case of measuring the credibility of human scientists, the key indicator here is less a matter of whether the scientists consult the computer and more a matter of whether they actually follow its advice—especially in situations where there is competing advice from a recognized colleague.
>
> *Deepen*: Multiple perspectives undermine a piece of research only if they are allowed to diverge indefinitely. These perspectives must enter into dialogue with one another, specifically to encourage each view to articulate, in its own terms, the differences that it can detect in the others. The ultimate goal would be a more inclusive discourse that found a place for each distinctive position. Thus, the multiplicity of perspectives on a piece of research should be encouraged—but so too equally many attempts at their integration.
>
> *Limit*: If social epistemology is to have moral import, then its theories must empower the people who believe them. In the first instance, then, theories need to be comprehensible to their intended audiences. Failure to address this reflexive concern led the Frankfurt School to be cynical about Marxism's ultimate ability to liberate the masses. After all realism works effectively as a scientific ideology in large part because of its cognitive simplicity. Realism projects real-world objects from theoretical terms and promises that, in the long term, all the theories will come together to explain everything by the fewest principles possible.

THOUGHT QUESTIONS

☙ What is "Socratic Conflation"? How does Socratic Conflation turn relativism from a positive to a negative thesis? Rhetorically how does one distinguish between a positive and negative thesis? Can one have a particular or local understanding of standards that are apparently universal?

☙ Is relativism a form of antirealism? Do relativists assume that instrumental rationality is applied universally? How do conceptions of instrumental rationality inform relativists' conceptions of science?

☙ In what respect is nature always already a part in determining the theories we choose? In what respect is society always already a part in determining the theories we choose? How does a particular emphasis on nature or society account for differences of opinion on epistemic matters?

☙ What does holding one of the three relativisms that Fuller describes imply about one's understanding of culture?

☙ Is Mannheim's sociology of knowledge a case of naturalized epistemology? For Mannheim, what is the relationship of the observer of culture to culture itself? What objections might advocates of social change, members of the Frankfurt school for example, have to Mannheim's views? What important differences about individual knowledge producers are papered over in grouping them into knowledge producing communities?

☙ How does Fuller characterize the "pursuit of truth" in relation to relativism?

☙ What is the relationship among knowledge, reason, and social relations for social epistemologists? How do positions on the mind–body problem shed light on the relationship between science and society?

☙ What is the incommensurability thesis? Why, for Fuller, is this thesis important in understanding conceptual change in science? Why is disagreement, within the context of apparent consensus, necessary for a social epistemology? How does the standardization of written discourse in science help to diffuse normative intervention? What accounts for the seeming invariance of scientific theories across social, historical, and communicative contexts?

☙ What does Fuller see as the most frequent and substantial criticism of social epistemology? How does the sensibility of this criticism translate into reflexivity? What might be the elements of a reflexive history? A reflexive rhetoric? What does Fuller mean by deepening science reflexively?

☙ What courses of action might result from the incorporation of reflexivity into social epistemology?

10

Opposing the Antitheorist

In 1989, Stanley Fish, the original *l'enfant terrible* of U.S. literary critics, published a collection of essays, one of which provocatively asserted, "Theory has no consequences." This assertion crystallizes much sentiment in the more postmodern reaches of the STS community and, more generally, in the humanities and social sciences. In this chapter, I present Fish's defense of this assertion and then counter it with my own view of theory as a transformative rhetorical practice, one grounded in reversing presumption.

Fish's position presupposes that *practice* (or *doing* X) is one thing and *theory* (or *talking about* X) is something else entirely. According to Fish, failing to respect this distinction lends the source of most of the problems surrounding the cognitive status of the humanities. For example, "critical self-consciousness," a capacity much vaunted by radical theorists, illuminates our prejudices, but does little to eliminate them. Comprehensive knowledge of X leaves the performance of X unchanged. A close-up look at one example, drawn from Fish's critique of Roberto Unger, whom we encountered in our earlier discussion of reflexivity, highlights what lies in the balance of these dialectical maneuvers (Unger 1986, 1987; cf. Fuller 1988b).

For Fish, Unger's "transformative vision of politics" is ultimately of no political consequence. Although Fish grants Unger that overarching social structures arise and persist only in virtue of local political contingencies, he nevertheless cautions aspiring revolutionaries against taking comfort in Unger's premise because "the political efforts still have to be made, and the assertion that they *can* be made is not one of [those efforts]" (Fish 1989: 431). One would expect such advice to issue from the pen of some stuffy ordinary language philosopher keen on reinforcing the use–mention distinction—not from the pen of a dazzling postmodernist critic like Fish. Yet Fish's point is familiar: Being a theorist, Unger operates on a logical level once removed from the actual practice of politics. Being more commentator than participant seemingly implies that Unger's potential impact on politics, *qua* theorist, is decidedly limited. I say "seemingly" because Fish's line of reasoning—its familiarity notwithstanding— does not follow.

In brief, Fish is bewitched by a metaphoric conflation of conceptual and causal "determination" that has captivated most philosophers since Hobbes. The metaphor is launched by regarding the move from *X to talk about X*—the ascent to the "meta" level—as a means to construct an abstract representation of X. Like concrete representations such as painting a picture of X, abstract representations require that one stands at a certain distance away from the represented object. Doing so allows a full view of the objects' form and position in a field of other objects. But given the constraints of normal eyesight, this fuller view involves a loss of detail as well as a diminished ability to act on specific parts of the object. If one thinks this way, it should come as no surprise that talk always seems to be *mere* talk, and theorizing seems to be a practice, like teaching, designed expressly for those who can't do.

To follow the metaphor further, an abstract representation "underdetermines" the objects it represents. Underdetermination, as a relation, alludes to a tradeoff made between a representation and its objects. To stand for many things in many settings, a representation must be constructed at a sufficient distance from its objects so that most of their details drop out. Thus, when invoking such abstractions as "destabilization rights," "institutional reconstruction," and "deviationist doctrine," Unger represents a host of heterogeneous practices. The differences among these practices fade away against the commonalities that he wants to bring into focus. But do these abstractions actually enable Unger to make people see how they "deviate" in particular cases? Fish presumes that the answer is no, on the grounds that theory hovers too far above the world, and hence constrains the possibilities for practice too loosely. Still Fish fails to take into account that theory and practice, representation and its objects, exist on the same plane, in the same world—however much theories and representations may advertise themselves otherwise. To paraphrase Ian Hacking (1983): No representation without intervention!

In sum, *pace* Fish, theory *does* have consequences for the simple reason that language is part of the causal order. Only something *in* the world can be *about* the world (Fuller 1988a: Chap. 2). Unger's abstractions, insofar as they are embodied as utterances and embedded in social contexts, have *many* consequences. This point should be taken as trivially true, not denied, as Fish curiously tends to do. The interesting question is whether Unger's abstractions have the consequences that he either intends or would find desirable. That question is better treated *empirically* in a survey of what audiences do with Unger's utterances than *conceptually* in an analysis of the meaning of his bare words. To see the contrast here, consider the (entirely plausible) case of individuals who are inclined to take action only if they think that everyone else will as well. Lofty abstractions that speak to a common plight will likely do more to motivate such people than speech tailored to the particulars of each person's situation. From a rhetorical standpoint, such a unified message is doubly effective. It not only restrains people from abandoning the group effort as their particular needs are satisfied, but also encourages them to trust that the speaker aspires to do more than simply gain political advantage by mollifying special interest groups. Such dual rhetorical mastery lay behind the "philosophically inspired" French Revolution of 1789 and the Russian Revolution of 1917. Regarded as a set of abstract concepts, the slogan "Liberty, Equality, Fraternity" epitomizes the airy indeterminacy for which positivists from Auguste Comte to A. J. Ayer have derided metaphysicians. Yet regarded as a piece of rhetoric operating in the world, the slogan could hardly have been more to the point—disparate audiences moved in a focused and largely desired fashion.

If conceptually indeterminate speech can be—indeed, has been—an effective vehicle for bringing about change in the world, why has this point eluded Fish? I suspect that, in spite of his own deftness in deploying and detecting rhetoric, Fish is ultimately a *disenchanted logical positivist*. In the spirit of Ayer's verificationist principle, Fish apparently thinks that if the empirical consequences of a concept are not specified in the concept's definition, then the concept has no such consequences "by right or nature" (Fish 1989: 28). Whereas Ayer thought that scientific theories were uniquely verifiabile, Fish believes that no theory fits the bill:

> Again, I am not denying that theory can have political consequences, merely insisting that those consequences do not belong by right or nature to theory, but are contingent upon the (rhetorical) role theory plays in the particular circumstances of a historical moment. (1989: 28)

This telling passage appears as Fish is debunking the idea that feminist theory has anything to do with the success of feminism as a political movement. Still who else other than a disenchanted positivist would want to drive a wedge between theory and its consequences, only to show that the latter do not follow from the former? (Moreover, *where* exactly would Fish drive the wedge? Once the words leave the theorist's mouth? Once they enter the audience's ears?) The key concepts used by theorists of Marxism, feminism, and constitutional law are defined primarily in terms of other concepts in those theories. Fish concludes each theory's impact consists of the licensing moves—especially the shifting of the burden of proof—in the language games that center on the discussion of those theories. For Fish then, feminism, for instance, is little more than rules for conducting conversations in certain academic settings. To be sure, Fish goes to great lengths to defend these conversations under the rubric of "professionalism." Yet he will not allow conversation to wander beyond "conventional" boundaries. Again Fish takes words too much at face value.

Had Fish wanted to find evidence for the efficacy of feminist theorizing, he should have looked at the subtle but substantial shaping of the academic mind wrought by the enforcement of nonsexist language in the style manuals of most disciplines. For example, the detailed guidelines of the American Philosophical Association or the American Psychological Association constitute a corrective presumption to modes of thought that continue to pervade contemporary society. Of course legislating the substitution of *she* for *he* will not necessarily alter the predilections of male chauvinist philosophers. But such action places the burden on them to demonstrate why the feminine pronoun should *not* be used in particular contexts. Even in "accurately" referring to a group not including women, the chauvinist may give thought to why women were excluded in that case and, perchance, may consider whether exclusion ought to have been the case. Feminists are familiar with the cognitive detour that the chauvinist must make in this situation as part of their "consciousness-raising" tactics. But raising consciousness is no less appropriate to other forms of theorizing (a.k.a. "language planning") that aspire to greater influence than can be expected from the rounds of hermetic academic discourse. The last part of this chapter is thus devoted to a more general exploration of the kind of corrective presumption that nonsexist style manuals exemplify.

WHAT EXACTLY DOES "THEORY HAS NO CONSEQUENCES" MEAN?

Having now been exposed to some of Fish's seductive arguments, we would do well to step back and consider in some detail the ambiguity of the claim, "Theory has no consequences." At least three things can be meant by this claim even if we take "theory" in its most ordinary sense (which, as a matter of fact, Fish does not). These three senses can also be found in recent disavowals of theory in STS, and so are especially worthy of our consideration:

1. *Theory cannot, by definition, have any consequences.* This extension of the logical thesis—normally associated with the later Wittgenstein—entails that the definition of a concept does not determine its range of application. At most, the definition supplies the concept's relation to other concepts in a common framework. Thus, formulating a theory (i.e. a system of concepts) and specifying the contexts where it may be properly used are logically distinct activities. Indeed, the latter activity crucially relies on situated judgment calls on "hard cases" not anticipated in the original formulation of the theory. Armed, then, with a complete mastery of *Structuralist Poetics* (Culler 1975), but no knowledge of the history of applied structuralism, I would be liable to the same misconceptions as befell the medieval physicists who tried to reconstruct the Greek experimental tradition with only the texts of Archimedes on hand. In STS, this capacity of theory appears in the guise of the Duhem–Quine argument against falsifiability and scientific realism (i.e. any theory can be logically saved in the face of any negative experimental outcome, but the theory cannot be credited in light of any positive outcome).

2. *Theory does not, in fact, have any consequences.* This empirical thesis might be offered by a social scientist to show the lack of influence of

theoretical pursuits on other social practices. A social scientist may perhaps include practices that theories have been designed to influence as well as the subsequent pursuit of theory. A vulgar Marxist materialist may find such a notion attractive because it would render intellectual discourse entirely epiphenomenal. However, in these stark terms, the thesis flies in the face of our historical intuitions about the efficacy of certain theories, such as those offered by the figures of the French Enlightenment and, indeed, Marxists. Still the thesis may be stated more sophisticatedly. For example, the thesis may show that whenever a theory has seemed to have social impact, that impact has been due not to the theory per se, but due to something contingently associated with it (i.e. the status of the particular theorist). In STS, this point marks the turn to experiment as “the motor of scientific progress.” In this instance, theory appears to be a post hoc rationalization of laboratory practice.

3. *Theory ought not, as a matter of principle, have any consequences.* This normative thesis might be formulated if theory were thought to have, or could have, the wrong sorts of social consequences. Such concerns were clearly voiced by Edmund Burke and other conservative opponents to the political impact of the French Enlightenment. Allan Bloom (1987) expressed similar reservations. He argued that speculative theorizing can easily turn into dangerous ideologizing once unleashed from the cloistered colonies of cool-headed academics into the frenzy of the public sphere. Aside from preventing the gratuitous agitation of the masses, academics may want to contain the effects of theory out of a self-imposed intellectual modesty. Such an effort would both reward the efforts of data collectors, archivists, and the other “underlaborers” who supply whatever “real” content theories have, and make that content available for more direct critical scrutiny. In STS, a similar sentiment seems to inform Bruno Latour's (1988) critique of scientific theories as “acting at a distance” from the phenomena they purport to explain. Here Latour refers to the ability of a theoretical explanation to suppress important differences in the items it subsumes, all in the name of uniform standards of knowledge.

These three readings stand in a curious tension. For example, the prescription made in (3) seems to presuppose that there have been occasions in which (2) has been false, whereas the semantic character of (1) seems to render the claims made in either (2) or (3) beside the point. Nevertheless, all three readings are tightly woven into the fabric of Fish's argument, as in the following:

> Then this is why theory will never succeed: it cannot help but borrow its terms and its content from that which it claims to transcend: the mutable world of practice, belief, assumptions, point of view, and so forth. And,

> by definition, something that cannot succeed cannot have consequences, cannot achieve the goals it has set for itself by being or claiming to be theory, the goals of guiding and/or reforming practice. Theory cannot guide practice because its rules and procedures are no more than generalizations from practice's history (and from only a small piece of that history), and theory cannot reform practice because, rather than neutralizing interest, it begins and ends in interest and raises the imperatives of interest–of some local, particular, partisan project–to the status of universals. (Fish 1989: 321)

Fish's main example of theory, Chomskyan linguistics, provides another occasion for seeing how the three readings of his thesis are mixed to suit his immediate dialectical purpose. At first Fish pronounces on the conceptual impossibility of Chomsky's project. Then realizing that *something* that Chomsky has done has in fact become very influential, Fish turns to debunking theory's causal role. Finally, lest the reader think that, no matter how it happened, Chomsky's success was not such a bad thing after all, Fish warns against allowing theory to divert attention from genuine scholarly and critical practice. Thus, the moments of Fish's dialectic pass from (1) to (2) to (3). In so doing, he implicitly invites, respectively, the philosopher, the social scientist, and the politician to assess the merits of his claim.

FISH'S POSITIVISTIC THEORY OF "THEORY"

In locating the conceptual space occupied by theory, the key contrast that Fish has in mind—between *algorithmic* and *heuristic* procedural rules—is one familiar to mathematicians and computer scientists For Fish (1989: 317), the theorist aspires to the algorithmic. She would like to discover rules that can function as a guide to a humanistic discipline's practice in all cases by being sufficiently explicit and neutral for any practitioner, regardless of her particular interests, to follow the rules to the same result. By contrast, a rule with heuristic status can guide practice only in certain cases that cannot be determined in advance of practice. The rule, in this instance, can only be determined, in retrospect, once the practice has in fact been successfully guided. In short, the desired distinction is between "the foolproof method" and "the rule of thumb."

In *Doing What Comes Naturally*, Fish smuggles additional conceptual baggage into this distinction. The foolproof method turns out to be one that, at least in principle, can be derived *a priori*, which is to say prior to all practice. Consequently, the method is unaffected by the history of the practice it purports to govern. The rule of thumb turns out to be valid *a posteriori* in a rather particular way. The rule's validity is relative to the entire history of a practice and not simply to a practitioner's own experience in trying to apply the rule. These conceptual moves are properly characterized as "smuggling" because not only are they logically independent of Fish's opening moves (i.e. a foolproof method need not be knowable *a priori* and a rule of thumb is not normally thought of as a kind of social convention), but they also ensure the success of his central argument. Fish argues that (a) the pursuit of theory cannot succeed *because* all purported instances of theory are really generalizations from actual practice, and (b) this failure of theory does not impair the ability of practices to govern themselves locally through rules of thumb. The force of these two conclusions would not be so strong if their truth were not virtually deducible from Fish's idiosyncratic definition of the two kinds of rules. As a result, were the reader to wonder why foolproof methods could not be grounded empirically or why rules of thumb must have the force of social conventions, he would be at a loss for an answer from Fish.

Still for all their question-begging character, Fish's tactics have precedent—in the very movement he claims to oppose. For Fish's definition of "theory" as a "foolproof method" is nothing short of a positivist reduction. Before the rise of positivism in the 19th century, "theory" was generally used to describe privileged standpoints from which phenomena might be systematically inspected. A theory was thus typically "speculative" and "metaphysical." The attitude of the theorist was one of contemplative detachment. It would be fair to say that this is the ordinary sense of the word theory. However, this sense of theory was precisely the one attacked by the positivists. Comte, for example, argued that the only way to tell whether one's theory was any good was through an experimental test: Does the theory allow the inquirer to obtain what he wishes from the phenomena? A theory that could regularly give a positive answer to this question was "theory enough" for the positivist for it would then constitute a foolproof method for conducting one's inquiry. Like the metaphysical sense of theory, the positivist sense had to be articulated in a technical discourse. But the positivist did not refer to subtle underlying entities that unified the array of phenomena under study in ways not always transparent to the casual observer. Rather operational procedures were referred to which any inquirer could implement and to which he could be held accountable by some larger community.

In positivism's more virulent 20th-century form (which followed a brush with pragmatism), a camp follower like A. J. Ayer would argue that what distinguishes "scientific theory" from other forms of theory is *not* its ability to permit us a deeper understanding of reality. Rather a scientific theory provides an ability to permit us more substantial control over phenomena—hence, positivist philosophy of science is typically described as "antirealist and instrumentalist." This view's implication for theory in the social sciences, then, is clear: The better theory is the one better able to predict and control the behavior of people. Interestingly, ethical theories turn out to be crude theories of this kind. By contrast, ethics clothes its interest in controlling behavior in the metaphysical language of "values." Fish's own remarks about "the consequences of theory" dovetail nicely with Ayer's here, since Fish believes that whatever impact theory has on humanist practice is due not to its truth directing the way to better interpretations, but to its force directing interpreters into certain desired forms of discourse. However, Fish ultimately fails to see his ties to positivism. By infusing his conception of "theory" with *a priori* status, Fish conflates the older metaphysical sense of the term, which he clearly rejects, with the newer positivistic sense, which he seems—at least in practice—to embrace.

In failing to see himself as an instance of the positivism he opposes, Fish, perhaps unsurprisingly, also misses the point of the positivist's longing for a "value-neutral" method, which he dismisses as patently impossible to achieve. Value-neutrality, a term popularized by Max Weber after having provoked a generation of polemics among German economists at the end of the 19th century, was hardly ever used to characterize the activity of "constructing" a method (of, say, economic analysis). Instead value-neutrality more often characterized the activity of testing or justifying a method. Already, then, this fact about the term's usage concedes the point that Fish still thinks needs to be contested. To wit, that all theory construction is laden with the theorist's values, which are determined by the local nature of his own practices.

Open to debate, however, is whether a theory can be tested or justified in a value-neutral manner. For example, philosophically inclined practitioners of the life sciences (e.g. John Eccles, Peter Medawar, Stephen Jay Gould, but especially David Faust 1985) have been persuaded by Karl Popper's view that the scientific community collectively achieves value-neutrality for a theory by having the theory's tester be someone other than the person who first proposed the it—presumably someone who does not have a stake in the theory. This myth seems to fit the sociological data. The data suggest that the personalities of scientists polarize into two types; roughly the speculators and the experimentalists (Mitroff 1974). As long as the scientific community has a healthy mixture of these personalities, all of whom see themselves as engaged in the same inquiry, then Popper's picture appears quite workable. In any case, Fish does not address the possibility that value-neutrality is simply the mutual cancellation of individual values on the collective level.

Now the issue of whether a theory can be justified in a value-neutral manner is somewhat trickier to resolve largely for terminological reasons. The logical positivists frequently spoke of "theory-neutral observation." In so doing, however, the positivists used "theory" in the metaphysical sense mentioned earlier and "observation" to describe something already placed in a technical language. Less misleading, for purposes of criticizing Fish, would be for the reader to substitute "value-neutral theory." The motivation behind the positivists' desire for such neutrality was that the two most heralded, contemporaneous, physical theories—relativity and quantum—were each supported by scientists of an idealist (Eddington for relativity and Bohr for quantum) and a realist (Einstein for both) bent. Yet regardless of their position on the idealist–realist debate, the scientists could account for the same facts from within their respective metaphysical positions. The positivists tried to generalize this insight to the idea that a theory is more justified as it can be deduced from higher order theories, especially ones that would otherwise be mutually incompatible. Thus, the ultimately justified theory, the positivist's notorious "observation language," stands out by its ability to be deduced from all other theories. It, therefore, remains justified regardless of which of those other theories ends up getting rejected. In practical terms, the possibility that Fish neglects here is that value-neutrality may simply be a theory's ability to be endorsed by people having otherwise conflicting values.

TOWARD A MORE SELF-CRITICAL POSITIVIST THEORY OF "THEORY"

Increasing the conceptual stakes in our critique of Fish might prove instructive at this point. Instead of revealing Fish's errors by the positivist standards to which he secretly seems to aspire, let us turn to criticizing the standards. This tactic is not as unfair as it may seem as the positivists were among the first to realize the inadequacies of their own account of theory. Indeed their reservations arose from further thought about the alleged value neutrality or metaphysical indifference of empirically testable theories.

Carnap (1967, pp. 332-39) originally set the stage when he appealed to relativistic and quantum mechanics as evidence for the "pseudo-problematic" status of philosophical (or, in Fish's sense, "theoretical") disputes. He noted that such metaphysically divergent physicists as Bohr and Einstein could continue to add to the body of empirical knowledge while their philosophical differences remained unresolved. However, if true, the truth of Carnap's claim was very much an unintended consequence of the many famous exchanges held between the idealist and realist physicists. For Bohr, Einstein, and others interpreted their own empirical inquiries as attempts to vindicate their respective metaphysics—an impossible task by positivist lights. Still evidence could be found for a deliberate application of Carnap's thesis, and to great consequence —but in the less glamorous science of experimental psychology. In his 1915 presidential address to the American Psychological Association, John B. Watson called for the abandonment of the entire introspectionist paradigm. Its evidence, Watson claimed, was gathered simply by training subjects in the particular experimenter's response protocols and produced a hopelessly diverse array of data. Yet Watson quickly added that introspection's failure was behaviorism's gain. If nothing else, the diversity of data proved that learning was a robust phenomenon worthy of study in its own right, independent of the content that the subject was taught. Needless to say, behaviorism was destined to become the most important anti-theoretic scientific research program of the century.

After the late 1930s, when behaviorism became the academically most powerful school of psychology in the United States and the logical positivists had emigrated to this country, psychology was the science to which positivists most often referred for examples about the eliminability of theory. Not surprisingly, the positivists quickly found themselves pondering Fish-like thoughts, which culminated in what Carl Hempel dubbed the *Theoretician's Dilemma*:

> If the terms and principles of a theory serve their purpose they are unnecessary [since they merely summarize the known data]; and if they do not serve their purpose they are surely unnecessary. But given any theory, its terms and principles either serve their purpose or they do not. Hence, the terms and principles of any theory are unnecessary. (Hempel 1965: 186)

To his credit, Hempel solved the dilemma by abandoning a crucial positivist assumption. The assumption, rather prominent in Fish, is that algorithms and heuristics mark a distinction *in kind*. That is, positivists generally suppose that some rules work all the time and can therefore function as proof procedures, whereas other rules are more open-ended and work only occasionally. Against this, Hempel argued that heuristics are merely *imperfectly known algorithms*. So if a rule works only occasionally, one cannot immediately infer that the phenomena to which the rule applies are indeterminate or, in some fundamental way, that the phenomena escape rule governance. Rather this result simply means that the right rule has yet to be found. Another way to make Hempel's point is that positivists act as if a theory becomes somehow less theoretical, and somehow less worthy of scientific attention if it turns out to be *false*. Clearly this view is mistaken since a major role for theory is not merely to save, but to *extend* the range of phenomena. This aspect necessarily involves an element of risk given that we never know in advance whether our extensions will be correct. What, then, informs these extensions of theory? According to Hempel, as well as Popper and Quine, none other than the sorts of "local" considerations that Fish seems to think vitiate the epistemic status of theory. For all its intriguing character, *there is nothing self-contradictory about the possibility that certain universal truths may be discoverable only under quite particular historical circumstances.* This point, an oversight in Fish's argument, did not elude the positivists. And it did not escape Karl Mannheim (as we saw in the last chapter) whose sociology of knowledge took this idea as the guiding methodological insight.

THE UNIVERSALITY, ABSTRACTNESS, AND FOOLPROOFNESS OF THEORY

A related confusion into which Fish seems to have fallen as a result of making aprioricity essential to theory concerns the relation of *universality* to *fallibility* and *corrigibility*. Contrary to the spirit of Fish's definition, universality does not necessarily imply infallibility and incorrigibility. Admittedly, in the Western tradition, several philosophers have followed Plato in believing that whatever is universal cannot be false and thus need not be changed. However, those philosophers (and it is controversial whether even Descartes should be included among them) have also tended to think that universals are apprehended by a special mental faculty. Plato called this sense *nous*, whose workings are infallible in virtue of bypassing the potentially deceptive route of sensory experience. Curiously, Fish seems to think that Plato's idea continues to be a live option among contemporary theorists in the humanities who are no doubt familiar with the conclusions of *The Critique of Pure Reason.* Moreover, both Charles Sanders Peirce and Popper argued that as long as a universal principle is treated as a hypothesis under test, no contradiction follows in saying that it might be false and hence revisable. Isaac Levi (1985) has since gone further. He separates issues of corrigibility from those of fallibility: A discipline, simply because its interests have changed, may legitimately decide to revise a universal principle even if that principle has not been shown false.

When Fish claims that certain theorists believe they are possessed with "pure reason" or some other form of "intellectual intuition," he may be expressing latent skepticism about the sort of knowledge that can be gained from abstraction. Certainly this would make sense of his dismissive remarks about Chomsky's project of universal linguistics. Again to cite precedent, Aristotle talked about the abstraction of a universal in two sorts of ways. On the one hand, Aristotle described abstraction of a universal as the extraction of what is common to a set of particulars (*korismos*). On the other hand, he described it as what is left after the particularizing features of a particular are removed (*aphairesis*). Generally speaking, if a philosopher talks about abstraction in the former way (as *korismos*), she tends to be sanguine about its efficacy. If she talks about abstraction in the latter way (as *aphaeresis*), she tends to be skeptical. Joining Fish in the list of skeptics are William of Ockham, Bishop Berkeley, and F. H. Bradley. All believe that a conceptual distinction is legitimate only if it can be cashed out as an empirically real difference. (Even Bradley the Absolute Idealist buys this line insofar as he believes that if no empirically real differences exist, then no legitimate conceptual distinctions exist.) With this history in mind, let us now return to Chomsky (Fish 1989: 314-18).

Chomsky argues that our linguistic performance is a degenerate expression of our linguistic competence. As an anti-abstractionist, Fish will demand that there be some way to empirically eliminate the degenerate elements of *our* linguistic performance—not the performance of machine simulations—so as to reveal this underlying competence. Fish claims that experiments of this kind are bound to fail because language works *only* because sentences are always situated in a context of utterance, which consists of those very elements Chomsky calls "degenerate." In his aversion to abstraction Fish may, thus, be likened to the physiologist who thinks that examining dead bodies defeats the whole point of studying the human organism, which is, after all, to discover how life works (cf. Bernard 1964, for the absurdity of this argument in physiology). Although both sides of this analogy have some *prima facie* plausibility, can either side, especially Fish's, hold up under close scrutiny?

From within Chomsky's camp, an instructive way to diagnose the source of Fish's anti-abstractionism should make us think twice about what exactly is being criticized here. Jerry Fodor (1981: 100-126) distinguishes two reasons why a scientist—let us say a cognitive scientist—is interested in abstraction or "idealization." Initially, she might want to model the optimally rational thinker. In this case, in her object of study is indeed something closer to a computer simulation than a real human being. Instead, however, she might want to model real suboptimally rational thinkers. In this case, foolproof computational methods will have to be supplemented by other rules that don't work nearly so well and, as a result, account for the numerous errors that real thinkers make.

Both Chomsky and Fish confuse these two interests in abstraction. Chomsky claims to be interested in modeling real speakers, but his techniques suggest that he is really modeling ideal speakers. Fish catches on to this fact. But he mistakenly concludes that Chomsky's is the way of all abstractive projects and that, therefore, all should be rejected. Again the error here has probably been inherited from Plato. The problem arises if we understand the Platonic concept of "Form" to imply that any particular is a degenerate version of just *one* universal or, in more Aristotelian terms, that each particular consists of one essence and many nonessential features. After all Chomsky does not merely bracket considerations of context from his search for linguistic universals. He actually does not believe that context has much to contribute to a general understanding of language. Certainly, Fish is reacting at least as much to this devaluation of context as *nonessential* as to the fact that Chomsky restricts his interests in the essential features of language to whatever can be captured in a competence grammar. Therefore, both err in thinking that if there are universals to be found in a given set of particulars, then there are *at most* one. Yet in fact a more realistic representation may be obtained by supposing that particulars are governed by *several* universals—in the case of language, principles of *pragmatics* as well as ones of syntax and semantics.

A final set of confusions into which both theorists and antitheorists are prone to fall concerns the sense in which a method can be "foolproof." In many ways these confusions are subtle. They, in any case, most naturally lead to a discussion of the recent antitheorizing in legal studies. To get at these uncertainties, I introduce a distinction in types of rules first raised by John Rawls (1955) in an attempt to revamp Kantian normative theory, namely, between *regulative* and *constitutive rules.* A regulative rule is one drafted by a legislature and which appears as a statutory law: for example, "All wrongdoers must be punished." Notice that this rule is stated as a universal principle but does not mention which cases count as instances of the principle. The latter problem is the business of adjudication, which works by applying constitutive rules. These rules determine how and which particular cases should be constructed under the principle—say, that "Jane Doe is a wrongdoer"—usually on the basis of tacit criteria for which a judicial opinion provides ex post facto justification.

Rawls' distinction allows us to make sense of the idea that a rule can be universally applicable without specifying the universe of cases to which it may be legitimately applied. Put more succinctly, the distinction shows us that *a theory does not entail its practical applications*. This conclusion often turns out to be the point of many of the later Wittgenstein's examples of mathematical practice. For example, my knowledge of, say, the Peano axioms and all the theorems of arithmetic—the sorts of universal principles that mathematicians study and formalize—is never sufficient to determine which arithmetic principle I am applying in trying to complete a particular number series. Thus, when the mathematical realist claims that a fact of the matter exists as to how the series goes, she offers small comfort to the person counting, who wants to find out exactly *which* fact it is.

Likewise, Fish is probably guilty of confusing Rawls' two types of rules when he claims that "foolproof methods" and "rules of thumb" are incompatible pursuits for the humanist. For even if there were a computer algorithm specifying the steps by which one correctly interprets a poem, one would still need some other rule—perhaps a rule of thumb, perhaps another algorithm—for identifying relevant cases for applying the algorithm. In other words, knowledge of how to interpret poems still does not tell us how to recognize poems in the first place. Moreover, again contra Fish, Rawls' two types of rules are sufficiently independent of one another so that practitioners of a humanistic discipline could coherently agree on procedures for identifying poems without agreeing on procedures for interpreting them—or even vice versa. Where rules exist for interpretation but *not* identification, one can imagine the discipline agreeing to "If x is a poem, then x is read in this manner" and still disagreeing over whether a given x is in fact a poem. The example that seems to arise most often in the antitheory literature is E. D. Hirsch's (1967) strategy of "general hermeneutics," against which Fish inveighs.

CONVENTION, AUTONOMY, AND FISH'S "PAPER RADICALISM"

Before ending our catalogue of the conceptual problems facing the leading force of antitheory in the humanities, a word should be said about a term to which Fish attaches great importance in connection with how disciplinary practices are authorized: *convention*. In political and linguistic philosophy, conventions are contrasted with *contracts* and *grammars*. A convention is a practice that emerged largely without design. But the practice continues to be maintained in virtue of the beneficial consequences accrued by the individuals adhering to it. Fish usually understands "convention" in this way. Yet he sometimes means convention in the sense of "conventionalism." Conventionalism is a doctrine about what confers validity or legitimacy on a theoretical statement—namely, that it follows from some explicit earlier agreement about definitions and assumptions. This second sense of convention arises especially when Fish wants to devalue the kind of legitimacy supposedly claimed for a theory by its proponents and, therefore, stresses the similarity between theories and games.

However, as Hilary Putnam (1975: 153-92) has observed, conventionalism's metaphysical implications are really much stronger than someone like Fish thinks. A conversational implicature of the claim, "p follows by convention," is the claim "p follows *by virtue of nothing else*." The conventionalist, then, seems committed to what Putnam calls "negative essentialism." Thus, to legitimately argue that disciplinary procedures are *nothing but* game rules, Fish must access the same sort of "metaphysical" knowledge as his opponent, the theorist, who believes that such procedures really represent a part of how things are.

Fish's frequent appeals to interpretation's "conventional" character play a somewhat unexpected role in delimiting a theory's powers. Ordinarily, claiming that a practice is conventional amounts to denying its naturalness and, hence, to suggesting that the practice may be changed. But this is not really what Fish has in mind. On the contrary, he invokes the conventional to signal that practices are explicitly bounded, self-regulating fields of action. Thus, these practices do not become less authoritative simply because their origin and maintenance have been subject to a variety of local contingencies.

By retooling the rhetoric of the conventional in this way, Fish manages to cater to two opposing constituencies at once. On the one hand, he appeals to the anti-intellectualist streak in American thought, which suspects that the professoriate poses a threat to our folk mores once academic discourse is permitted to stray beyond its natural habitat in the Ivory Tower (M. White 1957; Hofstadter 1974). On the other hand, Fish champions esoteric humanists within the academy, who, armed with the insight that all forms of knowledge are conventional, can now honestly claim that their activities are no less legitimate than those of grant-guzzling natural scientists.

The political bottom line for Fish may be summed up as *I'm OK, you're OK—as long as each of us knows our place.* Separate and *therefore* equal. The original sin is overextension, and theory is the devil's artifice. Such particularly aggressive relativism reveals Fish's true sophistic colors. Specifically, Fish's profound ambivalence toward the Left in *Doing What Comes Naturally* marks him as the ultimate *foul weather* friend. To stay true to the sophistic ideal of making the lesser argument appear the greater, he can support the Left only when they are on the defensive, but never when they are on the offensive. A good way to position myself vis-à-vis Fish is in terms of contrasting anthropological strategies of "cultural diversity." One strategy preserves established differences, and the other strategy promotes endless hybridization. Fish opts for preservation, I for hybridization. Further contrast can be drawn regarding notions of autonomy. I associate autonomy with combinability, whereas Fish links it with *purity*—indeed, a "retreat to purity," to hark back to a humanist tendency first identified in *Social Epistemology* (Fuller 1988a: Chap. 8).

The appeal to the purity, or autonomy, of research is a *topos* common to scholars across the arts and sciences. Often this appeal is expressed in the phrase "pursuing knowledge for its own sake." The expression conjures up two images of how knowledge might be pursued. First, the forthright inquirer might follow the truth wherever it leads regardless of the amount of resources consumed in the process. For example, in the extreme case of certain high-energy physics projects, the cost of pursuing knowledge as an end in itself entails that virtually everyone and everything be incorporated as means, usually labor and capital, toward realizing that end. Second, the pure pursuit of knowledge might signal a call to modesty. In this case, the inquirer restricts her efforts to what knowledge can reasonably be expected to control—namely, the production of more knowledge. Traditionally, this more humanistic route has been informed by two quite opposing considerations.

Some, like Allan Bloom and other latter-day Platonists, are concerned about the effects on people (mostly students) whose minds are unprepared to receive knowledge in an unadulterated form mainly because they have not been directly involved in the knowledge production process. These humanists profess the cultivation of "sensibility" in their thoughts and "decorum" in their actions. The most interesting modern expression of this sentiment is the institution of "academic freedom" (German *Lehrfreiheit*) as a self-regulated guild right: Speak as you will, but only in your field. In contrast, other humanists like Fish aim to deflate the pretense that they must watch what they say and do because of their potential impact on society. A result of this stance is the denial of theory's consequentiality. Yet this camp, too, is committed to the modest sense of purity. This group also believes that the moral imperative of tolerance always outweighs any claim to epistemic privilege: The pure inquirer restrains her own totalizing impulses to enable the flourishing of others who have just as much a claim on the truth as she does.

The irony behind such magnanimity is that humanists started singing the praises of relativism and pluralism only after successive historical failures at gaining control over politicians, artists, publics, and each other. The increasing independence of general hermeneutics from the specifics of biblical and legal interpretation is just part of this process of turning adversity into virtue (Gadamer 1975; Grafton 1990). Put most cynically, humanistic knowledge must be pursued as an end in itself because it certainly hasn't been a reliable means to any other end!

I do wish to simply suggest that a certain measure of self-deception may be involved in attempts, like Fish's, to defend scholarly pluralism. After all, as a clever sophist, Fish could well concede the diagnosis yet deny the cure. He might counter: Even if the politics of academic tolerance is a defensive reaction against an embarrassing historical track record, that still does not speak against the "consequences" of being tolerant. But as I have tried to show here, treating academic disciplines as well-bounded language games blinds one to the consequences that disciplinary discourses have outside their intended fields of application. Relativism makes sense as an epistemological doctrine only if a community can be identified relative to which knowledge claims can be held accountable. The relativist presumes that the consequences of acting on a knowledge claim can be contained to just the members of that community. We know, however, that knowledge claims are continually imported and exported across disciplinary boundaries. These boundaries shift over time, changing their relation not only to other fields but also to society at large. Given such complex circulation patterns, some knowledge claims may have their most significant impact on people outside their discipline of origin, in ways neither intended nor anticipated by their originators. The evaluation and mediation of these effects is the normative challenge that awaits the humanistic inquirer who is willing to treat knowledge production as more consequential than Fish would permit.

I do not want to exaggerate the powers of theory to change the world. Rather, I simply urge that these powers, such as they are, be regarded as a matter for empirical inquiry and practical control. No one denies that, in the 20th century, the pretensions of theory—especially of Marxist origin—have been deflated in various ways. But is this result something that should have been anticipated, given what Fish would call the "nature" of theory? Or should one suppose, to be less cynical, as well as rhetorically more sound, that a good theory requires not only that its speaker be persuaded to keep speaking, but also that its audience be moved to act appropriately? Thus, the failure of theories may be due more to the failure of theorists as persuaders than to the failure of theory as such. What then is the appropriate rhetorical habitat for a theory? The answer lies in the realm of *presumption.*

CONSEQUENTIAL THEORY: AN ACCOUNT OF PRESUMPTION

Presumptions are normative instruments for injecting some make-believe into an all too real world, with the long-term hope that reality may become more like make-believe. A scientific community, for instance, may never know whether a given theory is really true. But by granting the theory paradigmatic status, members of the community are forced to act as if it were so, which causes them to frame their positions in terms of that presumption. A presumption is established on explicitly normative grounds. Therefore, one has difficulty claiming that if a community presumes certain things, then most of the community's members actually believe those things to be true. In fact if the presumption is doing any real normative work, and hence correcting people's prior beliefs, then individual members of a community should suspect the truth or appropriateness of the presumption in particular cases. Nevertheless, they ought to believe that the presumption should be upheld so as to force the relevant countervailing arguments to be mustered. The presumption of innocence in Anglo-Saxon law seems to work this way. It also captures the attitude that disciplinary practitioners have toward a "widely held" theory in their field.

One can understand this attitude in the Durkheimian sense of reinforcing a collective identity. For the presumptively true theory exemplifies the methodological standards that confer epistemic legitimacy on the field as a whole. This point stands even if the theory is eventually superseded and currently suspected by a large portion of field. Moreover, one may argue (*pro* Popper and *contra* Polanyi) that what distinguishes science from a community of faith is precisely that leading theories are presumed rather than believed. As a result, scientists are professionally mandated to treat presumptions not as positive accomplishments in their own right, but as way stations to be superseded on the road to inquiry.

A critic typically takes aim at a presumption as when a trial prosecutor interrogates the innocence of a defendant or when an innovative scientist questions the orthodoxy of a belief. The critic functions as an agent of *rationality* insofar as she clearly distinguishes the presumption's ability to be defended (its real probative strength) from its ability to prevent attack (its mere conventionality). To overturn the presumption (and thus in fully "bearing" the burden of proof), a critic must show that the presumption's unassailability serves only to mask its indefensibility. Such is the case if the presumption can be defended on no other grounds than the fact that it has traditionally been presumed. Among the more obvious candidates for overturned presumptions would, therefore, be various "folk" beliefs. These beliefs may have been warranted when first introduced, but now have only habit on their side against our current background knowledge.

A presumption overturned in one case is not overturned once and for all. In other words, the effects of criticism appear to be purely local, confined to the single challenged case. (Thus, one false prediction does not refute a theory—a point that Lakatos realized, but Popper did not.) For example, if a particular defendant is proved guilty, the general presumption of innocence remains unaffected. Indeed the presumption would not be overturned even if defendants were always proved guilty. The reason that innocence is presumed in trials is conceptually unrelated to the police's success rate at apprehending guilty parties. Extending this legalistic model to epistemic matters, proving, in one case, that folk psychology does not offer the best explanation for someone's behavior does not diminish the presumption in favor of folk psychological explanations.

However, in epistemic matters, the frequency with which the presumption is overturned *should* ostensibly play a role in determining the presumption's fate. Thus, the law should resemble more closely how science is intuitively thought to operate. Nicholas Rescher's (1977) account of presumption, for example, is closely tied to a claim's probability—each defeat of the claim increases its burden of proof. I too believe that the difference between legal and epistemic presumptions has been exaggerated. In my view, both are *normative correctives* to widespread beliefs. These correctives, in the long term, may cause those beliefs to change, but do not depend on that prospect for their validity (Fuller 1988a: Chap. 4). Not surprisingly, then, the critic has her work cut out for her! Let us consider how this idea applies in both the legal and the scientific cases. In what follows, I draw on my original doctoral work on "bounded rationality" in legal and scientific decisionmaking (Fuller 1985).

Presumption in Legal Matters

Richard Whately (1963: pt I, Chap. 3, sec. 2), the 19th-century Anglican bishop and rhetorician, clearly modeled the modern theory of presumption on what he took to be the conservative grounds for presuming innocence in Anglo-Saxon legal procedure. Contemporary rhetoricians commonly interpret this conservatism as a strategy for risk-averse institutional action (Goodnight 1980). Thus, the judge would presume the defendant innocent to minimize the worst possible trial outcome. This presumption acted as a safeguard against needlessly ruining the defendant's life should a hasty judicial decision subsequently be shown as based on a faulty understanding of the facts.

However, given this interpretation of presumption's "conservative" function, presuming that *innocence* is the most conservative course of action does not follow. This point especially holds if we judge that action according to its consequences rather than its intentions and, moreover, if we expand the scope of the parties potentially under risk to include both the individual brought to trial and the society at large. Both sorts of judgments are empirical in character, not merely the products of conceptual analysis performed on "innocence" and "conservatism." For example, the presumption of innocence may encourage, in effect, individuals to be more reckless in their actions. Accordingly, individuals may do the sorts of things that superficially resemble crimes knowing that even if they are brought to trial the plaintiff must show more than just a superficial resemblance between their action and a crime. More likely still is the increased risk that society will have to absorb as a result of the fact that the presumption of innocence will permit unconvicted felons to roam free. Thus, the social function served by a presumption of innocence probably has little to do with whatever risk-averse impulses may have prompted the good bishop.

But the presumption of innocence may discourage illegal activities in a somewhat different way. In American civil procedure, for instance, there are two senses in which the burden of proof must be borne in a case. In the first sense, the plaintiff must always bear the burden of *persuasion* in demonstrating the defendant guilty of the alleged wrongdoing. In the second sense, the defendant may have the *burden of producing evidence* that shows that the plaintiff has not interpreted the defendant's actions in the most natural manner. The idea behind the defendant's responsibility is that if the defendant is indeed innocent, then he or she will likely have access to some fact that recontextualizes the case sufficiently to defeat the plaintiff's charge (Conrad et al. 1980: 840-43).

The considerations just raised to show the possible risk-enhancing consequences of the presumption of innocence are the very ones invoked by French jurisprudents in justifying the presumption of *guilt* as the appropriate stance for the judge to take toward the defendant (see Abraham 1968: 98-103, for a comparison of the role that presumption plays in Anglo-American accusatorial and European inquisitorial legal systems). Thus, *both* presumptions—of innocence and of guilt—have been legitimated on the same conservative basis. Still there are probably no empirical grounds for believing that either presumption especially contributes to a well-ordered society. In that case, why should the legal system presume anything at all about the defendant?

The need to ground the persistence of a form of social life in a principle of sufficient reason plays a big role in Whately's thinking about presumption. On this line of reasoning, there is a prima facie reason for believing that anything that has been the case should continue being the case. Although what social good is served by the persistent social form might not be exactly clear, the fact that the form has persisted is evidence for its serving some such good. Thus, the defendant is presumed innocent of *this* wrongdoing because he was innocent of other wrongdoings prior to his appearance in court. Consequently, given the good inductive evidence against the defendant's being guilty, the Anglo-Saxon judge is instructed to proceed cautiously in his inquiries to ensure against an unnatural understanding of what took place.

The work of Alfred Sidgwick (1884: brother of the utilitarian Henry Sidgwick) marks the transition from Whately's "sufficient reason" analysis of presumption to the more modern "risk" analysis. Sidgwick argues that an inductive regularity probably points to some well-founded phenomenon against which action would be risky. This line of argument is generally based on John Stuart Mill's work on the relation between induction and utility. Still the principle of sufficient reason can cut either for or against the defendant. The outcome depends on the persistence of the form of social life the presumption supposedly underwrites. The image need not be of a particular defendant as a law-abiding citizen. Instead, as in French juristic reasoning, the image could be of law enforcement agencies as consistently doing a job that needs to be done, which would then justify a presumption of guilt.

Whately's intentions notwithstanding, we should hesitate before embracing the sufficient reason interpretation of presumption. After all the traditional appeal of sufficient reason approaches has been their psychologically compelling character. "Things just don't happen for no good reason," we are prone to say—but about *what* exactly in the average legal proceeding? Are we not more likely to think that the defendant, and not the law enforcement agencies, has done something socially deviant (criminal or otherwise) to bring about the need for a trial in the first place? Taking psychology as our guide, then, sufficient reason would seem to weigh on the side of a presumption of guilt rather than a presumption of innocence. This conclusion follows if we assume that presumption must operate to *conserve* our intuitions about our fellow persons rather than to *correct* them. If, however, we go the route of correction, then we must turn our gaze from the *short-term* role that the presumption of innocence plays in *impeding* the judge's *actions* against *particular* defendants to its *long-term* role in *revising* the judge's (and society's) *attitudes* toward defendants in *general.*

If nothing else, the presumption of innocence implies that the law enforcement agencies that bring an individual to trial are likely to be in error, and that the defendant, unless proved otherwise, has acted within the confines of the law. The level of fallibility attributed to the legal system on this view not only runs against our ordinary intuitions, but is also quite foreign to the considerations that lead, once again, the French to presume guilt of the defendant.

Equally misleading, however, would be to say that the French presumption merely reinforces the intuitions that the Anglo-Saxon presumption seeks to correct. For in the French system, the presumption of guilt licenses the judge to suppose that, regardless of whether the defendant is indeed in the wrong, something strange has been afoot worthy of further examination. What follows, then, is an exhaustive inquiry into the facts of the case, which continues until the judge feels that he has achieved an accurate understanding of what took place. Therefore, the judge can subsume the case under the appropriate law. The judge's investigative powers are so extensive that he may freely suspend the rights of citizens (e.g. by wiretapping or opening their mail) in pursuit of crucial bits of evidence. By so conferring a greater value on the thoroughness than on the swiftness of the legal proceedings, French law supports an attitude of objectivity, impartiality, and ultimately the sort of certainty classically associated with an unhampered search for the truth. In the long term, then, the presumption of guilt manages to tinge the workings of legal conventions with the aura of scientificity. The effect commands greater respect of the citizenry.

As suggested earlier, the presumption of innocence in Anglo-Saxon law serves a strikingly different purpose. It serves as a constant reminder of the *mere* conventionality and, hence, likely fallibility of law enforcement agencies. This purpose acts against the natural psychological tendency, on the part of both the judge and the onlooking citizenry, to make too easy an identification between being a defendant and being a wrongdoer. Still the long-term effect is to increase the judge's capacity for fairness. The capacity for fairness, however, is not tied in any empirically clear way to increased social stability or even more correct verdicts. In that case, the presumption of innocence is perhaps best interpreted as simply a mental discipline undertaken by the judge and onlookers to correct what is taken to be an inherently bad psychological tendency. Such tendencies can, in various indirect ways, inhibit the administration of justice and, more generally, the wholesomeness of the defendant's subsequent interaction with his or her fellows.

Instead of seeing presumption as a conservative force in legal reasoning, and hence an *object of criticism*, our reinterpretation illustrates a sense in which presumption may function as a *tool for criticizing* and revising beliefs. These beliefs are widespread among judges and other legal functionaries, and yet not conducive to promoting the goals of the legal system. We can extend this new view of presumption to epistemic matters. So doing helps to spell out much of what is involved in radical conceptual change in science.

Presumption in Epistemic Matters

In keeping with the new view of presumption, then, radical conceptual change directly brings about a change in the orthodox beliefs of the scientific community and only indirectly a change in the actual beliefs of individual members of that community. The difference suggested here between *orthodox beliefs* and *individual beliefs* is reflected in the different answers that would be given to the following two questions:

1. What should the members of a community take to be the dominant beliefs of their community?

2. What should each member of a community believe for herself?

Radical conceptual change is possible because the answer to question (2) places no necessary constraints on the answer to question (1). Yet the answer to question (1) can be deployed as part of a strategy for altering the answer to question (2).

Question (1) considers how members of the community think that the burden of proof should be distributed among their beliefs. That the vast majority of members of the community happen to hold a certain belief does not indicate whether they would allow it to pass in open forum without strenuous argument. Legal functionaries, for instance, want to inhibit their natural tendency toward believing that all defendants are probably lawbreakers. Similarly, for *methodological* and *ideological* reasons, members of a scientific community might have an interest in keeping certain of their widely held beliefs from achieving the status of an orthodoxy. Doing so means holding those beliefs accountable to standards of proof that they clearly cannot meet (see Harman 1986: 50-52, for an attempt in dealing with the difference between beliefs "held for oneself" and those "held for others").

Among the methodological reasons may be that the belief, although widely held, is held only on the basis of indirect evidence or on the purely pragmatic grounds. Such a belief remains implicit in the assumption so that the standing beliefs of the community form a maximally coherent set. To elevate a belief of this sort to the status of orthodoxy would inhibit further testing and prevent the scientific community from discovering whatever falsehoods it may contain. Indeed Popperians would be especially suspicious of such a belief since its official acceptance promises to close critical inquiry on the set of beliefs it renders maximally coherent.

Another methodological reason for restricting the set of orthodox beliefs is to keep domains of inquiry separate. For example, most natural scientists believe, unsurprisingly, not only in the existence of God, but also in the occurrence of supernatural causation at some point in the history (most likely at the origin) of the universe. Yet the extent of the burden of proof that such a belief must bear (especially as measured by the number of alternative orthodox explanations that must be first ruled out) ensures that it will never again become part of the scientific orthodoxy. Indeed Jeffrey Stout (1984) has argued that the Scientific Revolution in the 17th century marked not the beginning of a decline in the belief in God, but a decline in the social recognition of such a belief as rational. Consequently, the burden of proof shifted from nonbelievers to believers. Two less exotic examples of the same phenomenon include: 1) the presumption against folk psychological explanations on the part of social scientists as a means of keeping their disciplines separate from common sense, and 2) the presumption against explanatory appeals to the unconscious and class interests on the part of classical humanists as a means of keeping their disciplines separate from the social sciences.

As for the ideological reasons, a commonly held belief may remain unorthodox because it instills a "bad" attitude toward scientific inquiry. For example, few scientists would deny that sociologists have accurately captured the extent to which scientific research agendas are opportunistic. To be sure, scientists openly admit to manipulating *ex post facto* the significance of research findings to fit currently popular theoretical debates. Yet these same scientists would resist the suggestion that they, henceforth, justify knowledge claims in terms of their opportunistic agendas. Their resistance would likely remain even if the sociologists turn out to succeed in showing that what Karin Knorr-Cetina (1981) calls "the logic of opportunism" better explains (predicts) scientists' behavior than appeals to the allegedly univocal relation in which evidence stands to theory.

More than just setting high probative standards, the scientific community has erected many purely *conceptual* barriers that serve to make the sociologist's stance difficult to articulate. Many of these barriers appear as distinctions that have been canonized by positivist philosophy of science. Of note are distinctions between reasons and causes as well as the theoretical and the practical. These distinctions are drawn precisely enough so that each pair of terms is jointly exhaustive. However, their applicable range is sufficiently malleable so that anything "inherently" a feature of scientific reasoning can always be made to appear on the "reasons" or "theoretical" sides of the distinction. In this way, scientists can project, to themselves and to the nonscientific community, an image, and ultimately an attitude, of detachment from thoughts of career advancement and other forms of self-interest.

We have looked at the use of presumption (and, correlatively, burden of proof) in scientific reasoning as a means of *arresting* change that the canon of orthodox beliefs would naturally undergo if all commonly held beliefs were granted orthodoxy. However, perhaps the more interesting use of presumption is in *facilitating* a change in the canon. Doing so necessitates granting orthodoxy to beliefs that have yet to be widely held by members of the scientific community. An analogue to the presumption of innocence in Anglo-Saxon law lies in the attempt to facilitate such a change in the scientific community. However, my model for the structure of this change is drawn from a determinedly nonscientific source: Pascal's Wager on the existence of God. In a crucial respect, Pascal's problem is similar to that faced by a scientific revolutionary such as Galileo: Both want to believe something that, for the moment at any rate, is unwarranted. Seemingly, then, they must scotch either their criteria of rationality (which says to have only warranted beliefs) or their desired belief (as rationality would demand). But there is a third way out: *They can cause themselves to arrive at a situation in which their desired belief is warranted.*

Now as Bernard Williams (1973: Chap. 9) noted, this third situation can arise either because the belief is indeed true (the change in situation would thus have resulted from some improvement in our cognitive powers) or because appearances have been manipulated so as to make the belief seem true (the change in situation would thus have resulted from some form of deception). Clearly, truth is preferable to manipulation. But if Feyerabend's (1975) account of Galileo is to be believed, manipulation will occasionally do as well. Williams also makes the interesting point that a simple sentence-uttering mechanism may have knowledge without having beliefs. Whereas what one knows can be read off what one says (because the truth of the sentence resides in its correspondence with reality), what one believes cannot be similarly read off the sentence since the individual decides what he will say given what he believes. This possibility allows for *intentional* falsehood that is lacking in the machine. If we regard the change in presumption that occurs during a scientific revolution as a decision to say what one does not necessarily believe, at least in the short term, then Williams provides a way to identify a "collective will" of the scientific community in terms of a presumption in favor of certain knowledge claims that its members do not yet individually believe.

In any case, a Pascal or a Galileo would need to reorganize their environments so that the sort of reasons that would be needed to warrant a desired belief could become available. The first strategy may simply involve fabricating some evidence that is tailor-made to the belief. The second strategy may require an extended crucial experiment. The experiment would be especially designed so that if the evidence warranting the belief does not arise under those circumstances, then the belief is probably false. Whereas the first strategy is set up to be foolproof, the second strategy clearly is not. It depends on the state of the world regardless of one's own beliefs. Now whatever Galileo may have had in mind, Pascal thought of his wager, with its attendant requirement that he conduct a thoroughgoing Christian life, as a crucial experiment. Thus, Pascal felt the risk of the wager as the strength of God's signs to him vary on a day-to-day basis. Interestingly, this second strategy of presumption formation also approximates Charles Sanders Peirce's use of the term presumption (otherwise called "abduction" or "hypothesis"), which he regarded as the motor of scientific progress.

Of course the distinction between merely fabricating evidence and positioning oneself to acquire genuine evidence can be easily erased from the prospective believer's mind. However, she must be able to cause herself to forget her interest in wanting to hold the belief, as Jon Elster (1979: Chap. 2) suggests in his account of presumption as "precommitment." Annette Baier (1985: Chap. 4) offers an interesting slant on this topic. She accepts that changing one's mind consists of rethinking the evidential relations of what one already knows and is thus not tied to a specific piece of evidence or argument, as ordinary belief revision is. But Baier argues that this rethinking occurs by remembering what was earlier forgotten—*à la* Plato's *Meno*—instead of forgetting what was recalled. Needless to say, either proposal—but especially Elster's—is a tall order as forgetting is something done not deliberately, but only as a by-product of some other activity. A good candidate for this sort of activity is the restructuring of discourse that is necessary for articulating any presumptively new relation between language and the world: the introduction of new terms, new meanings for old terms, and new inferential moves within the language in general. As one plays this new language game, the player naturally becomes convinced that she has been implicitly playing all her life. This tendency describes not only Pascal's adopted Christian lifestyle (in e.g. *Pensées* 252), but also the Whiggish characterization in terms of which revolutionary scientists come to see their predecessors once they have presumed a new paradigm.

Moreover, social psychological evidence suggests that the mere articulation of the new language game, on a regular and elaborate enough basis by enough people, will have the long-term effect of changing the beliefs of individual scientists. The evidence (admittedly drawn from studies of how political factions tend to gain dominance) indicates that at first any splinter group (say, an inchoate paradigm) is presumed by the public (say, the scientific community at large) to be a minority voice that would not have needed to speak up had its views been adequately represented by the dominant party (Noelle-Neumann 1982). As Whately would have put it, under those circumstances, only he who asserts must defend. However, in time, the presumption starts to shift away from the dominant party if it refuses to answer the claims made by the splinter group. In that case, the public may interpret the silence as tacit acceptance of the claims and, hence, ideological capitulation to the splinter party. In turn, the majority of voters often move to the splinter group's side as well, creating a bandwagon effect.

The spiral of silence is the expression that public opinion researchers use to characterize this frequent phenomenon. Yet readers of Kuhn (1970) may recognize it as the *Planck Effect,* which baldly claims that a new paradigm triumphs once voices of opposition from the old are silenced. If this link is apt, then we see the beginnings of a theory of long-term rational conceptual revision that incorporates much of what is distinctive in Kuhn's work into a general account of presumption. I made some opening moves in that direction (Fuller 1985) which I have pursued further (Fuller 2000b).

THOUGHT QUESTIONS

☙ What is the difference between theorizing about a thing, politics for example, and engaging in political practice? Is theory too distant from actual practice or are theory and practice conducted in the same world? How does rhetoric bridge the seemingly distinct activities of theory and practice?

☙ What examples does Fuller give of efficacious theorizing? How do correctives, such as legislating pronoun use in journals sponsored by professional organizations, illustrate the relationship between theory and practice?

☙ What bearing does Fuller's three readings of "theory has no consequences" have on Fish's position? How does Fuller characterize

Fish's conception of theory? How is Fish's view positivistic? What does Fuller achieve in portraying Fish as a positivist?

☙ How does the possibility that universal truths can be observed in particular historical circumstances support or undermine Fish's argument?

☙ What does ascribing universality to a given claim mean? According to Fuller, what aspects of the definition of universality does Fish get wrong? Historically and philosophically, what errors seem to follow in determining the relationship between particulars and universals?

☙ How does Fish's conception of practice appear to preserve the integrity of specialist humanist research?

☙ What is the rhetorical function of presumption? What are the similarities between legal and epistemic presumptions? How does the process of American civil legal procedure compare to the Fuller's proposals for a civil process for judging scientific knowledge claims? Ought one of the social epistemologist's jobs be the adjudication of competing knowledge claims? Historically, what social presumptions have shaped the conduct of science? How does science legislate itself?

☙ What is the difference in beliefs "held for oneself" and beliefs "held for others"? What are the rhetorical differences between personal beliefs and communal beliefs? On what basis should one hold unwarranted beliefs? What is the difference between a warranted and an unwarranted presumption? On what basis might the social epistemologist determine warrant? On what basis does a community grant an unwarranted presumption?

Postscript: The World of Tomorrow, as Opposed to the World of Today

In the world of tomorrow, scientific breakthroughs are regarded as triumphs of applied sociology and political economy, rather than extraordinary feats of the special sciences such as physics, chemistry, and biology. A distinctive knowledge product is presumed to reflect an innovative form of social interaction among knowledge producers and their publics. The languages of the special sciences are taken to cut the world up spatially rather than conceptually. However, the relevant sense of space is that of one filled by a transnational corporation, whose parts are distributed throughout the globe, more than that filled by a nation-state, which occupies one well-bounded place. Thus, the metaphor of "disciplinary boundaries" suggests a misleading sense of space in the world of tomorrow. These spaces are not absolute, preexistent realms of being waiting to be discovered by science; rather, they are relative spaces constituted by structured social interaction. In today's world, one recounts a scientific breakthrough by focusing on the laboratory where a discovery took place. But in tomorrow's world, one focuses on the arguments, both in person and in print, that were used to convince various constituencies that the artifact constructed in the lab warranted specific responses that empower certain people at the expense of others. In tomorrow's world, this differential empowerment, this redistribution of resources, is what, in today's world, would be called the "empirical content" of science.

In today's world, language is a "thin" phenomenon generally confined to the well-ordered noises that come from people's mouths, pens, or keyboards. This idea of language is counterposed to external reality, which is portrayed as having a mind of its own that often resists attempts to represent it. In tomorrow's world, however, language is a "thick" phenomenon that exists only in and through social interaction, of which formal syntax and phonology are simply abstractions—convenient for some purposes, but misleading for most. Here language does not merely "represent" the structure of reality, but is already embedded *as* the structure of reality. "Structure," then, is less an imaginative projection and more a mnemonic recovery. Those aspects of tomorrow's world that will be called "external" refer to cognitive liabilities—namely, whatever we cannot predict, recall, or otherwise structurally incorporate without great effort. The remedy is to restructure our environment so as to enable the perception of new things and the ignorance of others.

In the world of today, self-styled "holists" in the philosophy and sociology of science say that certain theories are preferred to others because they demand a change in fewer of the beliefs that we already hold. The effort implicitly conserved by this preference is that of conception or imagination. However, in the world of tomorrow, the relevant quantities conserved are labor and capital. Theorists will become more prone to argue about the social and material dislocation that comes from the implementation of alternative theories: Who would be dis/enabled to speak authoritatively? For what? When and where? And to what effect, for whose good? Put most crassly, any theory can be *made* true if we are willing to pay the price.

The love affair that Western thought has had with the idea of truth as something that is "discovered" or "revealed" finally comes to an end in the world of tomorrow. Today's talk of knowledge as gradually emerging through a process of "decontextualization" sounds odd to tomorrow's ears, just as 18th-century talk of the chemical process of "dephlogistication" sounds strange to today's. In both cases, the oddity lies in the image of something becoming more substantial as it suffers a loss. In dephlogistication, the loss of phlogiston supposedly added to the weight of a burnt piece of metal. In decontextualization, the loss of context-specificity supposedly adds to the validity of a knowledge claim. But in tomorrow's world, gains of both sorts will be seen, quite reasonably, as due to gains: Just as dephlogistication turned into oxidation, decontextualization will turn into standardization. Whereas in today's world the scales fall from one's eyes when one faces the truth, in tomorrow's world one must learn to see the world aright. Thus, emancipation comes to be known as a subtle form of imposition. Emancipation enables the transaction costs of knowledge production to come into view, which is to say epistemologists learn to ask who had to pay how much for knowledge that is nevertheless advertised as being the property of all.

Today civic-minded people commonly worry about the impact of science on social policy. In particular, they fear "necessities" and "essences"—that science will arrive at some ultimate facts about ourselves and the world that will trump democratic values of liberty, equality, and progress: Are intellectual differences attributable to racial ones? Is personality determined in infancy? Such questions encourage otherwise enlightened liberals to argue that some things are better left unknown or that science needs to be held tightly in check by our common humanity. The world of tomorrow does not deny the need for a humane science, but it will be more receptive to a scientifically changed conception of humanity. Scientophobia is a thing of the past as people come to realize that the determinateness of reality—that there are facts of the matter—does not imply a cosmic sense of determinism. That facts exist implies *the very opposite*—especially once we take seriously the idea that claims to truth or falsehood are impossible without linguistic and other technologies capable of enforcing the true–false distinction.

The possibilities for human action are expanded enormously once the bounds of the "human" are taken to exceed the capacities of the unadorned body and, more specifically, once the innate is no longer seen as unchangeable. This point is especially instrumental in overcoming the threat to our political sensibilities currently posed by the specter of, say, a genetic basis of intelligence. The model for handling such possibilities tomorrow is today's attitudes toward myopia. That a wide, and probably innate, variability exists in people's visual abilities has led neither to the devaluation of the social contributions made by the nearsighted nor to remedial courses for improving myopic vision. The answer was to make eyeglasses generally available at a nominal cost. In short, by technologically extending the body, today's brute biology is converted to tomorrow's consumer economics. This solution does not make matters any less controversial: After all who pays for producing and distributing the prosthetic devices that directly benefit only a portion of the population? But economization makes the questions more tractable by opening them to negotiation.

Currently, we see alternative research programs as "competing" to explain, or otherwise "save," roughly the same range of phenomena. This idea makes the history of science seem like a series of winner-takes-all jousting matches. The losers either scramble for cover in the enemy camp or disappear altogether. No such zero-sum games are to be found in the histories of science written in tomorrow's world. Rather, the cost of conducting the joust is taken more seriously, as each rival research program is portrayed as trying to outdo the other in its ability to incorporate its rival's interests without losing its own original focus. In tomorrow's world, the model for epistemic change is no longer war—"scientific revolutions" will lose their *Sturm und Drang* quality—but democratic party politics, in which we all win or lose together. Grantsmanship becomes the art of coalition formation. As a result, potentially affected third parties, who in today's world would be overlooked by the grant proposers, may tomorrow become decisive in swinging grant money from one team to another. These third parties may be openly courted. Indeed research funding may start to be seen as a form of "campaigning" that envelopes the entire populace in discussions over the consequences of pursuing competing lines of research.

Science policymakers, in tomorrow's world, will periodically rearrange the scientists' incentive structure so that they are motivated to team up with members of different disciplines or research traditions. Current concern and interest with how individual scientists conduct their research will shift to focus on the patterns by which the products of such research are combined and distributed. In this respect, the local autonomy that scientists and their well wishers jealously guard today is gladly granted in tomorrow's world. Tomorrow, what really matters is what happens once science ventures forth from the laboratory.

One factor that facilitates tomorrow's image of epistemic pursuit is a closer link between the material scarcity that gives rise to budgets (i.e. we cannot afford to fund every project) and the cognitive dissonance that gives rise to theory choices (i.e. not every theory of a given domain can be true). Left to their own devices, with limitless time, money, and energy, scientists can entertain a variety of incompatible theories indefinitely. Hard decisions—the stuff of which paradigm shifts are made—do not naturally arise in the pursuit of pure inquiry, but must be occasioned by the intrusion of a world of action on pure thought.

For example, there are two ways to look at the superabundance of funds for science. Today's way looks at more funds as leading to better science. On this view, waste helps foster the serendipitous character of good research (i.e. that breakthroughs can happen by accident or seemingly tangential work). By contrast, tomorrow's way looks askance at superabundance as promoting inefficiency. Such abundance offers no incentive for scientists to prioritize their research or to consider how they might work with others to mutual advantage. From today's standpoint, tomorrow's way looks utopian insofar as tight budgets alone will not solve the problem of conceptualizing knowledge production in cost–benefit terms. The problem remains of identifying the relevant epistemic outputs and assigning values to them. Yet from tomorrow's standpoint, today's worry appears beside the point, as fuzzy outputs are retrospectively seen as indicative of an inquiry without clear decision points. For example, to say that biology and physics evince incommensurable values that make comparisons impossible is simply an artifact of their drawing from separate pools of funds, which prevents these fields from ever coming into direct competition. Specify the parameters of the decision that needs to be made—by whom, for whom, between what, for how long—and the relevant sorts of outputs and value dimensions will come into focus. In any case, the outputs in tomorrow's world are not likely the ones that policymakers currently fall back on to make decisions. An author's citation count, for example, reflects scientists' attempts at maximizing their opportunities in the existing disciplinary structure. Such a forum does not register views on the worth of that structure, which is what policymakers will need to know in tomorrow's world.

In today's world, the idea that science is a public trust has merely presumptive status: That is, people presume without proof that they are somehow served by science. This presumption has delivered unto science a passive consumer culture that has no formal mechanism to account for what scientists do. Not so in tomorrow's world, where knowledge production absorbs a still greater share of human and material resources than it does now. Under these new circumstances, science is more integrated into the political structure. Accordingly, the lay public is routinely called (in the manner of jury duty today) to participate in research projects, during which the public has a say in at least the interpretation, and quite possibly the conduct, of the research.

Scientific expertise in tomorrow's world is not treated with the uncritical respect that it is often accorded today. Rather, appeals to expertise are seen primarily as a means to end debate. Such appeals are to be tolerated as expedient in the short term, but to be suspected in the long term. Recalling the original spirit of positivism and pragmatism, "method" in tomorrow's world is regarded as something *opposed to*, not in league with, expertise. This characterization turns on the idea that method implies a publicly accessible procedure for evaluating testimony, whereas expertise suggests the elite authorization of testimony. Once this point is realized, the social scientific understanding of knowledge production is no longer feared as supplanting natural science expertise with a yet more pernicious form of scientism. The success of a "science of science" depends on whether the inner workings of knowledge production are fathomable by people who are clearly nonexpert and, thus, in a position to empower society at large with knowledge of these workings.

As might be expected, one consequence of this penetration of science's internal mechanism in tomorrow's world is a shift in what counts as a "hands-on" understanding of science. Nowadays, such an understanding is conveyed by expert scientists in their disciplinary jargons. In the world of tomorrow, however, jargon is regarded as an abstraction—an abstraction that hovers above the micromechanics of text production and social interaction that captures science "as it actually happens."

Appendix: Course Outlines for STS in a Rhetorical Key

SCIENTIFIC AND TECHNICAL COMMUNICATION

STS research is "constructivist" in that the objects of scientific inquiry—and, indeed, the separateness of science from other social practices—are assembled by introducing and enforcing certain ways of communication or "conventions." But even when these conventions seem to have universal status, their application to a specific case needs to be negotiated with the audience you are addressing. For example, scientists know that they must operationalize their concepts for colleagues to test their hypotheses. Yet how do you manage to convey this in the paper you are about to write? Your answer determines the group of people who are empowered to hold what you say accountable. If you write in the standard technical prose of your discipline, then only other people trained in that field will be able to evaluate what you say. The repeated occurrence of this phenomenon gives the science–society boundary the sharpness that it has today. However, as with other forms of discrimination, whether this boundary is warranted remains unclear.

More people than just you and your colleagues have a stake in the conduct of your research, although the way in which scientists typically write obscures that fact from both yourself and those people. Jargon is not the stuff of which the public interest is made, yet the activities hiding behind that jargon are maintained largely through taxes and corporate sponsorship. You may look at this challenge to write accessibly (or "accountably") as either an *obligation* or as an *opportunity*. In one important sense, the two are linked. Given the increasing percentage of public and private funds devoted to research and the increasingly public character of the consequences of such research, it is only a matter of time (perhaps the occurrence of one high-tech "accident" too many) before greater accountability will be *demanded*. Rather than appearing to be forced to do something that you would otherwise not do, why not take the opportunity to develop a style that enables the interested lay reader to ask critical questions of your work?

In this course, we stress the opportunities that accountable writing can open up for you. You may have assumed that accountable writing will involve diluting the scientific content of the prose and further corrupting it with the extrascientific concerns needed to attract the lay reader's attention. Perhaps the chief goal of this course is to disabuse you of these preconceptions by persuading you of the constructivist premise of STS research. In other words, whenever you say that writing for a larger public forces you to "dilute the content" of your prose, you are simply expressing resistance to the idea of having certain groups in society—on whose goodwill you *already* depend—ask you critical questions. What you may initially see as the public's "extrascientific" concerns are, in fact, attempts to draw the science–society boundary somewhat differently from the way it was drawn in the past.

One thing you need to realize at the outset is that you and the public have more in common than you realize. In particular, neither of you spends much time thinking about science in society or the political dimension of knowledge more generally—until it affects you personally. As a result, you share some rather naive views. Because spotting our own faults in what others do is somewhat easier to accomplish, we start by critically examining published works that foster the naive view, even in their attempt to make science more publicly accessible. These works will serve as "bridging texts" that can increase your awareness of the interdependency of science and society.

We simulate a technique developed by the psychologist Jean Piaget. The idea was to get children to see contradictions in their thought so that they were then able to develop a more comprehensive framework for resolving those contradictions. Whereas Piaget had his subjects perform experiments designed to make the contradictions vivid for them, you will be asked to locate paradoxical turns in a text's argument that arise as a result of its promoting conflicting images of science. Typically, an unwittingly placed word or expression will reveal the paradox. For example, most of the talk about the "self-governing" or "autonomous" character of scientific research has been generated during the period when it has been increasingly subject to federal and corporate sponsorship. You will then be asked to rewrite the text so as to bring out the conflicting images of science that this suggests. The point is to make the conflict visible for public discussion.

PHILOSOPHY OF SCIENCE

Philosophy used to be the discipline that tried to explain everything within one system. In those days—which only disappeared in recent memory—philosophy was principally identified with metaphysics, which was in turn distinguished from the more limited missions of the special sciences. The turn from "philosophy" to "philosophy of science" began when the positivists projected this traditional systematic function of philosophy onto the idea of "unified science." The search for underlying principles was replaced by a scheme for translating and reducing the phenomena of all the special sciences into one master science. In this scheme, philosophers would not explain anything. Instead they would articulate standards of explanation and pass judgment on the adequacy of particular explanations. In effect, the positivists put the philosopher in the role of referee of the knowledge process, monitoring the flow of information between the disciplines (e.g. to ensure that one discipline was not relying on ideas or data that some other discipline had rendered problematic, obsolete, or in some other way unwarranted).

Postmodern thinking suggests that there is nothing valuable about either metaphysical explanation or its positivist successor, reductionism. Consequently, most contemporary accounts of explanation deny any overarching need for explanation—even in science—aside from particular requests that people have for knowing why certain things happen. The point of this course is to counteract this "settle for less" mentality that has beset recent discussions of explanation—and much else in the philosophy of science.

STS practitioners should take an interest in this topic because the most distinctive conceptual moves made by STS researchers (e.g. the Edinburgh school, critical Marxism, constructivism, actor-network theory) involve showing that seemingly disparate phenomena are in fact instances of the same deep and general principles (e.g. that no really sharp difference between science and the rest of society exists, initial appearances to the contrary). One does not need to be Bruno Latour to believe that explanations necessarily have both a political and an epistemic character, and that the two are not easily separated. For example, in defending a contemporary version of the metaphysical search for deep explanations, Robert Nozick (1982) considers why so much intellectual and sometimes even political power is gained by being able to redescribe disparate features of reality in terms of One Big Picture. You will be asked to be on the alert for this duality in the weeks that follow. My own slant is that explanations are claims to intellectual property, so that the successful claimant is socially acknowledged as having authority over the "disposition" of the thing explained (or *explanandum*, in positivist lingo). Thus, if someone else wishes to make use of the *explanandum*, she implicitly holds herself accountable to the person(s) socially acknowledged as having provided an explanation for it.

For the term paper, you should take something that is normally explained by one discipline and explain it in terms of the theories and data of another discipline. The approach does not matter. You may, for example, provide a sociological explanation for a phenomenon in physics or vice versa. In any case, you would need to redescribe the phenomenon so that it can be discussed more fluently in the second discipline. What you come up with might look quite strange to a practitioner of the first discipline. In fact you should document the resistance you meet in trying to make the translation between the two disciplines, both in terms of finding the relevant words and principles and in terms of persuading practitioners of the first discipline that your proposed explanation actually illuminates something that interests them.

HISTORY OF SCIENCE

Take an explanation of some historical episode that you find reasonably convincing or at least that you are willing to defend for purposes of this paper. Describe this account in detail, especially why you find it stronger than competing accounts. Then explore how you would persuade the people accounted for by this explanation that it does, indeed, make the most sense of what they were doing. Imagine that this act of persuasion requires that you go back to the original moment in time so you can work only with information that the people had at the time. Clearly, then, you won't be able to simply give them the reasons why *you* bought the explanation because your reasons were informed by later research, to which they do not yet have access. In that case, what conceptual/empirical obstacles would you have to overcome to show that your account of them makes sense as a "natural extension" of what they already believe? Would you have to change some of their fundamental beliefs? Could you do that in a relatively nonobtrusive manner by working with other things they know?

For example, suppose you were interested in persuading Newton of Frank Manuel's (1969) psychoanalytic explanation of his work. Although you wouldn't be able to appeal to the interplay of such Freudian mechanisms as ego, superego, and id, you could nevertheless appeal to such 17th-century analogues as the intellect, the will, and the passions. However, translating the etiological side of the Freudian explanation would require some ingenuity, as early childhood encounters with parents had yet (in the 17th century) to acquire the significance for adult behavior that Freud bestowed on them. Yet even here it shouldn't be too hard to find a 17th-century belief that could serve as a touchstone from which to start to convince Newton that, say, unresolved tensions about his mother decisively influenced his scientific work. Perhaps your best bet would be to take advantage of Newton's intimate familiarity with biblical doctrines of sin.

This exercise will force you to integrate the following concerns: What is a good historical explanation? Can the historian "dialogue with the past" in some interesting sense, or is the expression just idle humanist rhetoric? How do you determine what people at a given time knew? Can the difference between your own "third-person" and your interlocutor's "first-person" accounts of the event be reconciled by some "second-person" acts of persuasion, or must you as historian choose between the two perspectives?

SCIENCE POLICY

Since STS will survive as a field only by constant outreach to other disciplines and the general public (e.g. in teaching or policy jobs), you must learn to argue your case in a clear and incisive manner. Toward this end, the course will depart somewhat from the usual heavy emphasis on writing.

Instead the class will prepare debates on the merits of some controversial issue in STS, for which the materials covered in this course will provide a general framework, but little specific guidance. Here is a list of possible "resolutions" from which to choose:

- Since "objectivity" is illusory, STS should be explicitly oriented toward a political agenda.
- Feminism has the theoretical resources to radically revise our understanding of science.
- Almost everything interesting about modern science can be explained in terms of the larger cultural issues dominating our time.
- What passes for "science" these days is so big that it is better seen as a kind of transnational corporation than a knowledge producing enterprise.
- STS gains strength from not having a clear disciplinary identity.
- STS research can refute certain claims that philosophers have put forth about science.
- STS researchers should study the natural and social sciences in the same way.
- Knowledge is powerful only because a few people have it.
- STS researchers generally understand the nature of science better than practicing scientists do.
- Successful scientists have a special psychological makeup.

In typical academic debate, resolutions are presented in fairly vague terms, and the affirmative's opening move is to give the resolution a more precise interpretation. In that case, the negative side must address that interpretation of the resolution. In practice, this means that you will find a partner and select a resolution, one person arguing the affirmative case, and the other the negative case. Although you will be arguing for opposing viewpoints, you should do your research collaboratively so that you can make each other's arguments more effective. In fact both members of the same team will receive *the same grade* for their debate. Think of these debates as staged events. The affirmative side will have 20 minutes, the negative 10 minutes for rebuttal, then 20 minutes to provide her own position, and then the affirmative gets the final 10 minutes to rebut the negative's position, with another 15 minutes devoted to unrehearsed questions from the audience (which may include invited members of the faculty). The professor will remain quiet during the event, debriefing each team afterward on the strengths and the weaknesses of the arguments presented. This exercise is designed to simulate three features of real-world encounters that STS researchers increasingly face:

1. An audience potentially receptive to what you have to say, but initially uninformed about the issues involved (a point will be made of

not assigning the class any specific readings in advance of a given debate);

2. A time constraint that will force you to say less than half as much as you would were you preparing an adequate term paper on the topic of the debate; and

3. The need to take a clear stand instead of vacillating between positions (as "proper" academic writing all too often encourages) in a more or less thoughtful manner (by taking a clear stand that might be demonstrably wrong, your audience will be encouraged to engage you).

In addition to these virtues, one general sensibility that this course aims to instill is what the Greek rhetoricians called *kairos* or "timeliness"—an art that seems to have been lost as soon as rhetoric moved out of the forum and into the classroom.

Because academic writing is increasingly treated (by both its authors and its readers) as intended mainly for the archives, university life provides few incentives for communicators to urge the timeliness of their arguments. Unfortunately, this lost art is crucial to persuading people in policy settings. In these settings, you need to show not only that your case has merits, but, more importantly, that a certain course of action should be taken—and soon —in light of those merits. For an academically trained person just entering the policy arena, the challenge is to insinuate one's abstract concerns (for empowering disadvantaged groups, for instilling global consciousness, etc.) in concrete issues that are *already* on the minds of policymakers. Lobbyists do this by convincing legislators to set up a free-standing agency to deal with problems of the sort that the lobbyist has successfully highlighted in a well-publicized case. The lobbyist succeeds by showing that the case at hand exemplifies certain general concerns that deserve systematic treatment.

Students in this course will be required to engage in the "casuistic" thinking and research that lobbying requires (Jonsen and Toulmin [1988] is the best philosophical history of this topic). This practice means learning to integrate academic and journalistic sources, as well as scientific and political agendas, in forging a persuasive argument.

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