CONTENTS

Introduction to the Second Edition  ix
Foreword by Thomas Nickles  xxxv
Preface  xxvii

PART ONE
ISSUES IN DEFINING THE FIELD OF SOCIAL EPISTEMOLOGY

1. An Overview of Social Epistemology  3
   1. Social Epistemology as the Goal of All Epistemology, 4
   2. Social Epistemology as the Pursuit of Scandal and Extravagance, 10
   3. Nonnormative Social Epistemology and Other Accommodating Banalities, 17
   4. Social Epistemology Rendered Normative and Epistemology Rendered Interesting, 24

2. Social Epistemology and Social Metaphysics  31
   1. Drawing the Distinction, 31
   2. Transcendental and Naturalistic Approaches to Representation, 36
      2.1. Naturalism among the Savages, 45
      2.2. Naturalism among the Systems, 47
   3. Explaining Transcendentalism Naturalistically: Bloor on Popper, 51

PART TWO
ISSUES IN THE LANGUAGE AND HISTORY OF KNOWLEDGE PRODUCTION

   2. Why Is It Now So Difficult to Defeat the Realist? 69
   3. Putting Scientific Realism to the Historical Test, 73
   4. Kuhn and the Realism of Many-Worlds, 85
   5. Regulative and Constitutive Realism in the Human Sciences, 89
   6. The Ultimate Solution to the Problem of Realism, 96
<table>
<thead>
<tr>
<th>PART THREE</th>
<th>ISSUES IN THE SOCIAL ORGANIZATION OF KNOWLEDGE</th>
</tr>
</thead>
<tbody>
<tr>
<td>7. The Demarcation of Science: A Problem Whose Demise Has Been Greatly Exaggerated</td>
<td>175</td>
</tr>
<tr>
<td>1. Laudan and Gieryn on the Demarcation Problem</td>
<td>175</td>
</tr>
<tr>
<td>2. The Two Histories of Science: Of Role and Player</td>
<td>178</td>
</tr>
<tr>
<td>3. Science and Its Kindred Roles</td>
<td>182</td>
</tr>
<tr>
<td>4. Conflating Role and Player as an Historiographical Strategy</td>
<td>185</td>
</tr>
<tr>
<td>5. New Demarcation Criteria for Science</td>
<td>188</td>
</tr>
<tr>
<td>8. Disciplinary Boundaries: A Conceptual Map of the Field</td>
<td>191</td>
</tr>
<tr>
<td>1. The Boundedness, Autonomy, and Purity of Disciplines</td>
<td>191</td>
</tr>
<tr>
<td>2. Three Techniques for Detecting Disciplinary Boundaries</td>
<td>193</td>
</tr>
<tr>
<td>3. Are Disciplinary Boundaries Necessary for the Growth of Knowledge?</td>
<td>195</td>
</tr>
<tr>
<td>4. When Disciplines Collide: The Bernard Principle</td>
<td>197</td>
</tr>
<tr>
<td>5. Disciplinary Ambivalence: Popperian and Foucauldian Versions</td>
<td>201</td>
</tr>
</tbody>
</table>
9. The Elusiveness of Consensus in Science 207
   1. Two Pure Types of Consensus and Four Mixed Ones, 208
   2. The Elusive Object of Consensus in Science, 216
   3. Consensus Rigging By Disciplinary Realignment, 221
   4. Implications for the Historiography of Science, 226

    1. The Social Historian in the Grip of Moral Psychology, 233
    2. Toward Cognitive Sociology and the Problem of Objectivity, 239
    3. Implications for Rewriting the Forman Thesis, 244

Appendix B: Having Them Change against Their Will--Policy Simulations of Objectivity 251

PART FOUR
ISSUES IN KNOWLEDGE POLICY-MAKING

11. Toward a Revival of the Normative in the Sociology of Knowledge 263
    1. Normativity Lost, 264
    2. Normativity Regained, 267
    3. Freedom and the Administration of Knowledge Production, 270

12. Social Epistemology and the Problem of Authoritarianism 277
    1. The Lure and Avoidance of Cognitive Authoritarianism, 277
    2. Expertise Politicized and Depoliticized, 283

Appendix C: Notes toward Designing a Core Curriculum for a Graduate Program in Knowledge Policy Studies 289

Bibliography 295
Index 313
INTRODUCTION TO THE SECOND EDITION

There is no doubt that social epistemology is taken much more seriously now than when this book was first published in 1988. It has even made it into the most recent edition of the Norton Dictionary of Modern Thought, which is published every ten years (under the imprint “Fontana” in the U.K.). Here is the entry, including the cross-references in italics:

social epistemology. An intellectual movement of broad cross-disciplinary provenance that attempts to reconstruct the problems of epistemology once knowledge is regarded as intrinsically social. It is often seen as philosophical science policy or the normative wing of science studies. Originating in studies of academic knowledge production, social epistemology has begun to encompass knowledge in multicultural and public settings, as well as the conversion of knowledge to information technology and intellectual property. The institutional presence of the field began with the quarterly, Social Epistemology (Taylor & Francis, 1987- ). Despite their many internal differences, social epistemologists agree on two points: (1) classical epistemology, philosophy of science and sociology of knowledge have presupposed an idealized conception of scientific inquiry that is unsupported by the social history of scientific practices; (2) nevertheless, one still needs to articulate normatively appropriate ends and means for science, given science’s status as the exemplar of rationality for society at large. The question for social epistemologists, then, is whether science’s actual conduct is worthy of its exalted social status and what political implications follow from one’s answer. Those who say “yes” assume that science is on the right track and offer guidance on whom people should believe from among competing experts, whereas those who say “no” address the more fundamental issue of determining the sort of knowledge that people need and the conditions under which it ought to be produced and distributed.

I count myself among those social epistemologists who continue to say “no.” To understand why, I need to explain how the expression “social epistemology” acquired general currency in philosophy (Remedios 2000). This is due to a special issue of the journal Synthese explicitly devoted to the topic, which gave me the idea of founding the journal and writing the book with the name Social Epistemology. In late 1984, while completing a Ph.D. in history and philosophy of science at the University of Pittsburgh, I was invited to contribute to the special issue, probably on the strength of my article that appeared in Erkenntnis earlier that year. The issue consisted entirely of Anglo-American philosophers trained in the analytic tradition, most of whom had already provided influential accounts of what it is for an individual to know something.

The origin is not accidental. Analytic epistemology still takes the Cartesian individual to be the paradigm case of a knower, with social epistemology an embellishment on that core model. In contrast, the French and German philosophical traditions—be they influenced by Comtean positivism or Neo-Kantianism—have generally stressed the shared and systematic character of episteme in “epistemology,” rather than the evidential support of the beliefs possessed by the individual knower. Thus, they have focused on the distinctive methods of the special sciences, especially as these reflect basic value differences in society at large. Recent projects in this “always already socialized” epistemology include Michel Foucault’s inquiries into the scientization of social judgments about “normal” and “pathological” behavior and Juergen Habermas’s attempts to identify a distinct
“emancipatory” interest for knowledge whose societal relevance transcends the parochial interests of positivist and interpretivist social science.

But these continental concerns are not entirely alien to Anglo-American philosophy. Versions have flourished in the field called “philosophy of science.” The field arose from continental European epistemologists migrating to Britain and America with the rise of Nazism. The Golden Age of the philosophy of science is captured in Lakatos and Musgrave (1970), which motivated my entry into the field. In the long term, Hilary Putnam and Ian Hacking have probably been the most distinguished North American beneficiaries of this European migration. The original Austrian, German, and French migrants never hid their belief that the fate of civilization hinged on the coordination and regulation of scientific inquiry. Similar views have animated American pragmatism, which explicitly defined itself against what John Dewey derided as the “spectator theory of knowledge” adopted by the Cartesian approach to epistemology. However, pragmatists have tended to envisage the social regulation of inquiry more in terms of a self-organizing community than the top-down legislative style of the logical positivists, with Karl Popper occupying an intermediate position. My own social epistemology is rooted in these concerns.

From the standpoint of ordinary English usage, the Cartesian attempt to reduce knowledge to some variant of “justified true belief” is an artificial specification of what counts as knowledge, in which the “belief” condition has privileged creatures with a conscience, or consciousness, as knowledge-bearers. This ontological assumption is rarely questioned by analytic epistemologists. To be sure, some notion of “truth” and “justification” (at least in the weak sense of demonstrating correspondence to a standard) is ordinarily implied by “knowledge.” But this condition alone permits knowledge to be attributed equally to brains, books, and databanks. Thus, I have been less interested in identifying what people “really believe” (whatever that means) than in how knowledge operates as a principle of social organization—for example, by motivating people to act in certain ways with regard to each other and their environments. Moreover, I have distanced myself from the ever popular “consensus theory of truth,” which works by some weighted aggregation of beliefs. Rather, I have been drawn to Foucault, behaviorism, and rhetoric—all of which share a preoccupation with knowledge as a means to produce certain effects, regardless of the agents’ beliefs, unless those beliefs contribute to the production of the relevant effects.

The English language easily obscures the issue because “know” and “knowledge” are made to cover too much semantic ground. But philosophically, it makes a big difference whether one’s inquiries into the nature of knowledge are anchored in the verb “to know” or the noun “knowledge.” Thus, we say that books contain knowledge but they do not know things. French and German helpfully distinguish between these two senses of “knowledge.” Each language has a word for “knowledge” that is more like our word “cognition,” i.e., the result of being in a certain state of mind: connaissance and Erkenntnis. Epistemology, in this sense, is then clearly aligned with philosophy of mind. But each language also has a word more like our word “science” which implies the control of something outside the mind without implying that one is in a particular mental state: savoir and Wissenschaft.
For example, the French translation of "knowledge" in Francis Bacon's "knowledge is power" is *savoir* (e.g., in Comte, Foucault), whereas Descartes and Sartre were concerned with *connaissance*. The result of this linguistic division of labor in French and German is that both philosophical traditions have discussions that anglophones would class as philosophy of mind (*connaissance/Erkenntnis*) and philosophy of science (*savoir/Wissenschaft*), but these occur in relative isolation from each other, and there is no field of "epistemology" that overlaps with both of them.

In continental European philosophy, the philosophy of mind is basically phenomenology, whereas the philosophy of science is focused on disciplinary practices. Depending on the context, "epistemology" is identified with one or the other tradition, but with neither consistently. For example, in the early twentieth century, who is more of an "epistemologist": Edmund Husserl or Ernst Cassirer? An argument can be made for both, yet neither their works nor their legacies overlap as much as one would expect—except negatively, as with Martin Heidegger, who came to reject both. The expectation that there should be overlap may be due to the conditions under which "epistemology" was coined in English by the German-trained Scottish philosopher, David Ferrier, in the third quarter of the nineteenth century. Ferrier was interested in mind-brain relations and the possibility of a scientific study of how we know. Indeed, he would probably recognize what he called "epistemology" in today's cognitive science. The problem was compounded with the influx of European philosophical traditions into the United States, especially the logical positivists. They were clearly striving for some holistic sense of knowledge that bridged the difference between *Erkenntnis* and *Wissenschaft*, either by reducing the former to the latter (physicalism) or the latter to the former (phenomenalism).

One problem that may have nagged the logical positivists in trying to square belief with knowledge is the following: On the one hand, the stress on belief means that a universal feature of knowledge pertains to its "subjective" character—that is, one's mental state, regardless of what that state is about. On the other hand, the objective features of knowledge appear to be particularized to patterns in the world which, once grasped, can be used by someone in any number of mental states, including those who regard the thing grasped as an error in need of elimination. Yet, the idea that knowledge is a universal subjective mental state about particular objective things does not quite ring true to either ordinary usage or a more generalized "normative" perspective that includes moral and political concerns. For example, it does not take into account what has often been called the "value-neutrality" of science, whereby one can competently use the laws of physics to produce either nuclear energy or nuclear bombs. But equally, the idea fails to account for how a tribe may deal with its environment in sophisticated and systematic ways, while refusing to accept scientific explanations for their own success and even scientific efforts at improving on that success.

If anything, these two examples point to the contrary lesson, namely, that a universal sense of knowledge may accompany a diversity of mental states, including ones oriented to mutually exclusive ends. Not surprisingly, then, philosophers as different as Popper and Foucault have denied that one must believe what one
knows. I stand with them here. Thus, I have argued that the normative mission of social epistemology is not met until the ends of knowledge are explicitly addressed, since it is all too easy for knowledge to be embedded in ways that we would consider wrong or contestable.

Another way to capture the difference between my own social epistemology and the versions featured in that original issue of *Synthese* is to consider two strategies for generating philosophically interesting “problems of knowledge”:

**Strategy A**

1. The thing I know best is the thing with which I have had the most direct acquaintance, namely my own mind. After all, without it, I could not have made this very observation. But my mind is possibly not all that exists.

2. How, then, do I determine whether other possible things exist, and, if they exist, how can I know them, given that they seem quite different from my own mind?

**Strategy B**

1. We ordinarily experience everyone (and everything) as living in the same world. Yet, as people articulate their experience, it becomes clear that there are significant differences in the aspects of the world to which we have direct access.

2. What, then, accounts for these differences in access to our common reality, and what enables us to ignore them in everyday life, as we suppose that our own access is the one shared by all (right-minded) people?

Whereas Strategy A captures the tradition of inquiry that unites Descartes’ and Quine’s progeny in analytic epistemology, Strategy B extends from Augustinian theodicy (How can divine harmony be compatible with day-to-day strife?), through Leibnitz to Hegel—as well as Karl Marx, Karl Mannheim, David Bloor, and myself. There are some important differences in the sorts of epistemological problems that the two strategies generate. Strategy A poses the problem of knowledge _inside-out_: How do we get out of our individual heads and into some common reality? Strategy B poses it _outside-in_: How do we get beyond our common reality and into the mindsets that separate people?

According to the inside-out strategy, knowledge is posed as a problem for each individual to solve by approximating a standard to which the cognitive agent may or may not have conscious access. There is no sense that epistemic access may be a scarce good, with one agent’s access to knowledge perhaps impeding, competing with, or making demands on the epistemic access of some other agent. This would be more in accord with the outside-in strategy. Here the cognitive agent is portrayed as choosing between one of two or more alternative research trajectories, fully realizing that resources are limited and that other agents will be making similar decisions at roughly the same time. This image of the knower as a “bounded rationalist” engaged in “knowledge management” has been a thread running throughout my career (Fuller 1985; Fuller 2001).

Strategies A and B both operate with epistemological premises that are taken to be liabilities in advancing the search for knowledge. For Strategy A, a self-cen-
tered relativism is the initial liability that needs to be overcome: I am in my own
head, but I suspect that there are other things out there different from me. How do
I find out? Not surprisingly, this strategy stresses methods that are biased toward
realism, such as looking for ("primary") qualities that remain invariant under a
variety of observations and transformations. For Strategy B, on the other hand, a
totalizing realism is the initial liability: We all live in the same world, therefore
everyone must think like me, at least when they are thinking right. But why does
this not seem to be the case? (Are they crazy?) The relevant corrective here is a
dose of methodological relativism: Precisely because our reality is common, it
cannot explain our palpable differences. We are thus better off regarding claims to
common reality as disguised partial perspectives, or "ideologies," that may gain
certain local material advantage by capitalizing on our weakness for thinking in
terms of totalizing forms of realism.

Adherence to Strategy B has led to considerable philosophical misunderstanding.
For example, most sociologists—certainly Mannheim and Bloor—have been
realists and have believed that the people they study are also realists, at least in the
sense of believing that contact with reality is part of what Arthur Fine has dubbed
our "natural ontological attitude." However, this rather modest commitment to
realism is bound to seem strange to philosophers wedded to Strategy A. For al­
though adherents to Strategy B typically grant that there is a "fact of the matter"
about the norms governing a community of inquirers, because these norms govern
only that community and no other (until demonstrated by studies of other commu­
nities), the burden of proof turns out to rest on those who claim the norms are
generalizable across communities.

Once the generalizability of norms is questioned in this fashion, social episte­
mology shifts from logic and metaphysics to politics and ethics. Thus, for me, any
form of knowledge that purports to be universal in scope—that is, true for every­
one everywhere—must be subject to democratic governance. The burden of proof
rests on those heirs of Plato who believe that there is universally valid knowledge
that is nevertheless accessible only to an elite. I find this possibility incoherent, let
alone undesirable. Yet, if this situation exists as an empirical fact (which, of course,
it does now), then the job of the social epistemologist is to make up the difference,
either by spreading this supposedly universal knowledge to the unenlightened or
revising one's sense of universal knowledge to meet the needs of the supposedly
unenlightened. This is my concept of prolescence, which would grant education
a constitutive role in the universalization of knowledge claims (Fuller 1993b,
p. xviii; Fuller 1999).

In this respect, I consider myself an agent of Enlightenment and, in particular,
a follower of Auguste Comte, who coined both "positivism" and "sociology." Like Comte, I have culled the results of the sciences for insights into the conduct
of inquiry in general (Fuller 1993a). I even endorse the rather unpopular Comtean
rationale for sociology as the ultimate metascience, namely, that it comprehends
the less complex sciences in a complex whole that empowers it to direct the course
of society. The key point about this rather grandiose view of sociology is that it
presupposes that the older sciences—physics, chemistry, etc.—are "simpler" not
only in terms of dealing with more limited areas of reality under relatively
restricted conditions (e.g., laboratories), but also in their own limited understand­
ing of the methods of science. In other words, the Comtean vision is less one of
mindlessly applying what worked for physics to all the other disciplines than of
becoming more self-conscious and hence more open-minded about the nature of
scientific inquiry. Thus, I have preferred Popper over his logical positivist cousins
in drawing mostly negative metascientific conclusions that serve to level the dif­
ference between so-called “expert” and “lay” forms of inquiry.

Moreover, I am influenced by Hegel and the German idealist tradition in hold­
ing that the “natural” and “social” sciences are mutually alienated sides of a holis­
tic sense of inquiry. A good contemporary example is the relationship between the
discourses of “body” and “mind” with respect to a comprehensive understanding
of human beings. One discourse is not simply “better” than the other because
ultimately they are talking about the same thing from different standpoints. In­
deed, the “two cultures” problem that besets the natural and social sciences is not
based on any genuinely contradictory positions between the two sides, but rather
a 150-year-old communication breakdown (or “incommensurability”) that increas­
ingly occurs in environments with scarce resources, which then provides ground
for antagonism (Fuller 1993b, chs. 1-2; Fuller 1997, esp. ch. 5). At a finer-grained
level, one could perhaps make an analogous argument about the relationship be­
tween philosophy and sociology, which would explain why I originally remarked
that “social epistemology” first strikes many people as an oxymoron.

Thus, the features of knowledge that an analytic epistemologist is inclined to
consider conceptually necessary, I treat as default hypotheses that are subject to
revision in light of further inquiry. After all, there is no reason to presume that,
simply out of their own accord, the findings of the special sciences will eventually
add up to a coherent overall account of the knowledge system. To be sure, this
raises a delicate issue. The need for stable first-order inquiries has traditionally
inhibited second-order impulses to study the nature of inquiry. In Kuhnian terms,
the demands of normal science limit the incentives for revolutionary change only
to those periods in which a paradigm has accumulated enough anomalies to place
it in a state of crisis. “If it ain’t broke, don’t fix it” seems to underlie most normal
scientific practice. Even Thomas Kuhn realized that scientists who applied their
first-order methods to a second-order study of the grounds of their inquiries—a
“science of science,” so to speak—typically undermined their commitment to the
first-order inquiries. At the very least, they came to realize the potential legiti­
macy of alternative research trajectories that have been historically suppressed
(cf. Fuller 1999). Consequently, Kuhn held that it is just as well that scientists
acquire an “Orwellian” (as in the Ministry of Information in 1984) sense of disci­
plinary history that masks the contingency that would be otherwise revealed by a
more empirically informed account of that history (Kuhn 1970, p. 167). My re­
cent study of the origins and impacts of Kuhn expressly focused on the impact of
this Orwellianism on second-order studies of the nature of inquiry (Fuller 2000b).

Given the historic resistance to second-order studies of the nature of inquiry, it
should come as no surprise that the relationship between the empirical and norma­
tive dimensions of inquiry has been vexed. The two major models of this relation­
ship are the geometrical and the dialectical, so named after their roots in ancient...
Greek practices. According to the geometrical model, the normative dimension is cast as “basic” or “pure” inquiry, in which the inquirer’s value orientation is inscribed in a set of objects or concepts on which the empirical dimension is then constructed. This latter dimension is defined as “deductions” or “applications,” depending on whether the inquiry is science or technology. As in geometry, the first principles circumscribe the range of permissible inferences. In contrast, the dialectical model shifts the terms of the relationship between the normative and empirical dimensions from a hierarchical to a conflictual one. Specifically, the normative dimension appears as an ideal or goal that is then realized within constraints or in spite of resistance, which in turn define the empirical dimension. As in dialectics, what results from this tension is a “synthesis” that “realizes” the ideal in a sense that is more akin to completion than instantiation.

Philosophers have usually opted for either the geometrical or the dialectical model of inquiry, with a few attempting to integrate the two into one system; e.g., Kant’s, which specifies the terms in which objects of cognition are, respectively, “constituted” and “regulated.” Outside of philosophy, the difference between the two models is typically exemplified by the distinction between science (geometrical) and politics (dialectical). However, I have argued that the natural descendants of these models are better seen in purely sociological terms. In that case, the geometrical model is exemplified by the Kuhnian paradigm and the dialectical model by the social movement (Fuller 2000b, ch. 8). Both science and politics can adopt either form, such that science may be movement-like (as it arguably was during the eighteenth-century Enlightenment) and politics may be paradigm-like (as it arguably was in Marxist ideology). Nevertheless, for analytic purposes, it is useful to keep the geometrical-dialectical distinction sharp in order to track the fortunes of the normative and empirical dimensions of inquiry. Whereas the geometrical model treats the empirical as already normatively infused, the dialectical model treats the empirical as a challenge for the normative to overcome. In short, the geometrical model will tend to characterize the world in ways it can accept, whereas the dialectical model will tend to characterize it in ways it would correct.

At the meta-level this produces two types of social epistemology, as exemplified by (1) Goldman (1999) and (2) Fuller (1993b):

(1) Geometrical: The basic concepts and principles of social epistemology are developed and justified in a “pure” philosophical setting, that is, by a combination of intuition, logic, and some stylized examples that acquire rhetorical force from their basis in empirical settings, but which function philosophically as paradigm cases for a very broad class of phenomena. In this context, “applied” social epistemology is the art of finding or imposing the salient concepts and principles in concrete cases. Aspects of the cases that escape these strictures are treated as inconsequential or subject to ad hoc explanation.

(2) Dialectical: The basic concepts and principles of social epistemology are developed and justified in the actual contexts of knowledge production that concern the social epistemologist. Thus, one starts in medias res, treat-
In terms of normative theory (or meta-theory, more precisely), I am a "rule utilitarian." If the people subjected to an epistemic regime can live well with its consequences, then that is success enough. The difficult question is how long and widely should such a regime be in effect before its consequences are evaluated and its continuation questioned. Although little more than Kuhnian superstition allows a paradigm to continue indefinitely until it self-destructs, track records are a prerequisite to the rational comparison of alternative research trajectories. This is the old Lakatosian question of when a "problemshift" is "progressive," but now rephrased to give it more political bite. Thus, for me, knowledge policy improves over time by removing obstacles to both the expression of epistemic interests and knowledge of the results of actions taken on those interests. In democratic political theory, such matters are normally discussed in the context of voting, a topic conspicuous by its absence from treatments of "theory choice" in the philosophy of science. Nevertheless, whatever progress there is in science occurs at this meta-level of the increasing inclusiveness and transparency of decision-making processes—not at the object level of approximating some transcendental goal of inquiry, be it "truth" or "welfare," that remains fixed over time.

My normative orientation is generally that of the interested non-participant in the knowledge system, which is diametrically opposed to the disinterested participant of analytic epistemology, who wishes to acquire knowledge first-hand above all else. (As a point of reference, post-classical, postmodern epistemology tends to adopt the standpoint of the interested—a.k.a. situated—participant.) I generally regard knowledge as a means to other human ends (which themselves may be epistemic), but one's participation in the knowledge process is usually confined to the meta-level of inquiry, that is, the design and evaluation of knowledge production regimes that others carry out. These regimes encompass issues of fiscal and employee management, social responsibility, as well as specifically process- and product-based forms of quality control. Thus, my long-standing interest in presenting social epistemology as tantamount to "knowledge policy" and "knowledge management" is grounded in the idea that, generally speaking, the prescribers and evaluators ("policy makers" and "managers") of knowledge production are not the same—in terms of identities or interests—as the first-order knowledge producers ("workers").

There have been two historical models of interested non-participation in a collective endeavor: one ancient and elitist, the other modern and populist. The first model, drawn from ancient Athens, is based on the client whose praxis lies in the
guidance he provides the craftsman, who possesses the techne needed to actualize the client's ideas. The client is credited with the product's success, the craftsman with its failure. The second model, drawn from modern democratic elections, is based on citizens selecting on a regular basis the politicians most likely to provide a coherent policy orientation for their diverse interests. Here the distribution of credit and blame is less clear-cut, except for a tendency to credit success to the overall constitution of the polity (often the original social contract, in the case of the United States) and failure to particular individuals (either citizens or politicians).

The most popular general strategy in analytic philosophy for doing social epistemology subjects fairly traditional truth-oriented ("veritistic") epistemological considerations to a smattering of recent insights from more relativistic trends in the sociology of science, feminism, or multiculturalism. As in the constrained optimization model in neo-classical economics, these trends constrain the knower without fundamentally altering her basic epistemic orientation. The result typically smacks of syncretism, in which the philosophical and sociological parts of social epistemology are never properly integrated, let alone resolved in some higher synthesis. Rather, they sit uneasily in adjoining paragraphs or chapters, much as Tycho Brahe's world-system appeared to followers of Copernicus (cf. Fuller 1993b, pp. 70-84). Some influential members of this species include Longino (1990), Kitcher (1993), and Goldman (1999). Perhaps the most methodologically suspect feature of this tendency is that the sociological elements are introduced only on a "need-to-know" basis, namely, when the ideal epistemological conditions have failed to be met in concrete cases that happen to concern the social epistemologist. I have dubbed this feature "phlogistemic," recalling the eighteenth-century substance, phlogiston, whose existence could only be proved by its absence (Fuller 1996). Phlogistemics is usually afoot whenever the analytic social epistemologist invokes Quine's "underdetermination thesis."

To illustrate the difference from my own position, consider a recent well-placed analytic piece that claims to point to a "truly social epistemology" (Fricker 1998). Fricker proceeds by what she calls a "philosophical genealogy," which in reality is little more than the "original position" that has characterized social contract theories from Hobbes to Rawls. Whatever the value of such thought experiments, they are most definitely not genealogies. In calling his distinctive explanation of morals "genealogical," Nietzsche grasped the salient implication, namely, that current forms could trace their lineage back to earlier forms that may be surprisingly different, and indeed perhaps a bit embarrassing, as in an ancestor who founded the family fortune on piracy. Although Nietzsche lacked a Neo-Darwinian sense of the distinction between genotype and phenotype, he knew enough about the potential differences in the properties of parents and offspring to employ a biological rather than a strictly mechanical metaphor when discussing inheritance. In contrast, most philosophical thought experiments postulate causes to track effects, not trace causes from known effects. Thus, Fricker treats the effects of her thought experiment as mechanically either enhancing or impeding their putative causes, not radically transforming them.

According to Fricker, survival in her pseudo-genealogical state of nature
requires an ability to distinguish between true and false beliefs on a reliable basis. She immediately infers the need for distinct epistemic institutions whose reliability is measurable by publicly available means. Historically speaking, the emergence of institutions explicitly devoted to the pursuit of knowledge is roughly equivalent to the history of universities, academies, and related bodies. But the knowledge produced in these institutions has been rarely subject to the “real world” reliability judgments that Fricker’s “original position” requires. Indeed, historically most of the suspicion surrounding “scientific” approaches to practical matters has been due not to popular ignorance, but precisely to science’s failure to demonstrate its reliability in the relevant practical contexts. For example, do results from the artificial conditions of the medical laboratory improve on the general practitioner’s knowledge? Usually, the state has had to intervene to resolve this problem, motivated partly by wanting to increase its own power (vis-à-vis that of local practitioners) and partly by envisaging what science might accomplish with enough resources and discretion.

In any case, Fricker is mistaken in thinking that the capacity for distinguishing true and false beliefs for survival purposes naturally leads to the establishment of autonomous knowledge-producing bodies. On the contrary, we need to justify the promotion of autonomous inquiry in terms of specific goals it might serve. One such goal, which I support, has figured prominently in the rhetoric behind government support for science: adequate welfare for all humans, especially those whose interests are unlikely to be served by more egocentric inquiries. In this context, I introduced the concept of “epistemic justice” (Fuller 1992; Fuller 1993b, pp. 315-316). Fricker tries to deal with this problem in terms of her own notion of epistemic justice, which arises from the observation that some knowledge claims seem to carry more authority than epistemically warranted because they are backed by political power. To correct this injustice, Fricker would have knowledge claims evaluated by means that are independent of formal political structures, yet the resulting evaluations would structure the flow of power in society.

In marked contrast to Fricker’s Platonic strategy, I hold that an epistemically just regime would be in the perpetual project of preventing any form of knowledge from becoming a vehicle of power. To be sure, at any given point in history, certain forms of knowledge privilege certain sectors of society. But then the state needs to regularly redistribute the advantage that these forms of knowledge have accumulated over time—what I have called (with the universities in mind) “epistemic trust-busting” (Fuller 2001, ch. 1). In other words, “affirmative action” is not simply a temporary strategy for getting the balance between knowledge and power right once and for all. Rather, it is a long-term policy for disintegrating the power-effects of knowledge.

I have two models in mind here, one from economics and the other from politics. From economics comes the idea of knowledge as a “public good.” Unfortunately, economists seem to think that public goods are a reality rather than a regulative ideal of economic action. In contrast, I would say that in reality knowledge is a positional good, the value of which is directly related to restrictions on its access, be it through intellectual property rights or academic credentials (Fuller 2001, ch. 2). From politics comes the civic republican tradition of democracy,
which is founded on an ideal of liberty as non-domination (Fuller 2000a, ch. 1). Thus, in terms of epistemic justice, the only power worth acquiring from knowledge is the power not to be dominated by others.

I never cease to be amazed by the willingness of analytic social epistemologists to turn such knowledge-bearing properties as "competence" and "expertise" ("intelligence" would also be used, were it not so politically charged) into covert principles of social structure. The implied social order is sometimes akin to the mafia (i.e., the costs of not trusting the experts are likely to be higher than trusting them), other times to a royal dynasty (i.e., there are no legitimate grounds for a major change in perspective unless the current regime fails). Together they point to a political perspective that is relatively primitive, or at least pre-constitutional. The focus is on science as a self-organizing system whose differences are internally resolved and externally applied to the larger society. The conflict of scientific paradigms popularized by Kuhn is no more politically advanced than a feud between dynastic families that occurs (ideally) out of range from "ordinary folks."

But there is also probably wishful thinking at work here. My guess is that Fricker and other analytic social epistemologists adhere to Rawls' "difference principle" of justice, whereby power asymmetries are justified as long as the worst-off benefit more than they would under a more egalitarian regime. The wishful thinking lies in believing that the difference principle would roughly track the asymmetries that would result by apportioning political power according to epistemic merit.

My own position aims to be more politically sophisticated by leaving less to chance and wishful thinking. Indeed, a "truly social epistemology" would be an exercise in constitution-making. Specifically, how does one set up the forums for deciding science's research and teaching agenda, given the patently biased and otherwise limited nature of the participants? Unfortunately, the constitutionalist project has been mystified in recent times by its most ardent defender, Juergen Habermas, who has saddled it with transcendental appeals to the nature of the good society. I have found it more instructive to look at the conditions under which constitutionalism has historically flourished, namely, when a society's ideological differences have been sublimated in common projects that benefit the various sides differently. Such polities are the civic republican ones mentioned above, a far cry from more consensualist democracies that require a common mindset from those who agree to a common policy. Rhetoric plays a crucial role here in enabling many people to move in one direction, in spite of deep (even "incommensurable") disagreements. In modern societies, universities have played a vital role in the promulgation of rhetoric in this sense, a point I have increasingly pursued (Fuller 2000a, chs. 3-5; Fuller 2000b, ch. 8; Fuller 2001, ch. 4).

Put another way, there are two ways of understanding the knowledge = power equation. One supposes that more knowledge helps concentrate power, the other that it helps distribute power. Analytic social epistemologists adopt the former perspective, I the latter. Indeed, the best examples of "knowledge = power" in this dual sense may be found in the feats of civil engineering in the ancient empires (Egyptian, Incan, Indian, Roman, Chinese) that are normally seen as having lacked a proper respect for scientific inquiry. But even in our own time, civil engineering may be a good benchmark, since roads and bridges are usually criticized for the
differential access they provide to the population on whose behalf (and with whose funds) they are built. Analogously, if a scientific research program benefits only a fraction of those who paid for it, then it is “epistemically unjust,” regardless of the program’s ability to meet standard reliability and validity criteria. In this context, I have introduced the concept of epistemic fungibility (Fuller 1993b, pp. 295-300; Fuller 2000a, pp. 141-145).

In one last attempt to drive home my position, I shall end by providing a detailed response to a kind of claim that I have periodically encountered from realist philosophers and natural scientists in the ongoing “science wars”:

Contrary to your social epistemology, social interests are not always necessary for evaluating knowledge claims. After all, the round-earth theory is an improvement on the flat-earth theory, regardless of our interests in wanting to know about the shape of the earth. More specifically, the European scientific community came to be convinced of the truth of Newtonian mechanics because the planets really do move as predicted by Newtonian mechanics.

We are justified in believing that the round-earth theory is an improvement on the flat-earth theory because our theory turns out to be better by standards that have themselves changed, so as to render the flat-earth theory a non-starter. By “standards,” I mean the contexts in which we are most likely to want to know the shape of the earth. In this sense, interest is integral to the nature of knowledge claims. The people who found the flat-earth theory persuasive were generally not interested in the earth’s shape for the same reasons that now persuade us that the round-earth theory is true. In particular, they did not wish to embed the earth’s shape in a unified theory of physical reality, à la Newtonian mechanics. In any case, one need not be a skeptic or even a relativist to say that the standards for evaluating knowledge claims must be made explicit at the outset in order to argue sensibly about which claims are better than others. Moreover, if these standards are not to appear arbitrary, they must make reference to the reasons why one wishes to acquire knowledge, so that the knowledge so acquired turns out to be of the appropriate kind. “Justified true beliefs”—the classical definition of knowledge—are all too easy to accumulate, as long as we are not too fussy about the relevance of what is accumulated to what we care about. Indeed, the classical definition of knowledge seems to have been designed more for conservers than stakeholders or even funders of knowledge.

Admittedly, the reasons for wanting knowledge are not normally made explicit in the conduct of science. Unless that time-honored epistemic device—the budgetary constraint—intervenes, we do not normally worry much about the relative value of pursuing physics vis-à-vis biology. Both disciplines are simply taken to be worth pursuing as well as possible. That is because it is presumed that, at a certain level, we all have the same reasons for wanting to engage in scientific inquiry. Thus, positivist appeals to the “unity of science” presume a unified conception of both reality and the community of inquirers. And as an observation of our current epistemic condition, it is true that, in many contexts, the standards for adequate knowledge are common to most of those who would want such knowledge, regardless of their other personal and social differences. However, this convergence on epistemic standards is not, strictly speaking, the result of some con-
vergence of independently reasoned judgments. That members of a well-defined scientific community can usually agree on which theories to accept or reject is largely explainable by their common training and their desire to follow wherever the pack seems likely to go.

However, as I originally argued in chapters 9 and 10 of this book, only an Orwellian sense of history would have us believe that the scientists behind the acceptance of Newtonianism in the eighteenth century constituted a relatively unified, consensus-seeking community. Indeed, before disciplinary boundaries hardened in the second half of the twentieth century, a scientist would normally pursue multiple research agendas with varying degrees of enthusiasm, resources, and results. A formal vote would never be taken to ratify a paradigm. Rather, something closer to a statistical drift in scientific allegiances occurred over time, the phenomenon that Kuhn perhaps overdramatized as the “invisibility of scientific revolutions.” In such a diffuse field of play, in which many different parties pursue a variety of theories for only partially overlapping reasons, it is unclear a priori what might count as epistemic success. Each theory has its own way of prioritizing evidence and arguments, suited to its own particular strengths. Consequently, a successful scientific research program has typically had to score on two fronts at once: It has not only had to overcome rivals, but to do so according to rules that are biased to its strengths. These rules then become the basis for evaluating new players and are retrospectively used to explain the failure of old rivals. In short, any major success in science is simultaneously a meta-success. Indeed, the part of Newton’s success that is usually explained by appealing to “nature” or “reality” is nothing more than this meta-success.

Thus, once Newtonian mechanics had been widely accepted, textbooks gave the impression that everyone accepted it for the same set of “good reasons” that now allow the theory to be integrated into the larger body of scientific knowledge, on which the next generation of inquirers may build. Somewhat more credible cases of independent individuals converging on the same scientific judgments may be found in the widespread lay acceptance of the professional standards of engineers and medical doctors. But these cases usually presuppose that it is rational for individuals to defer to those who have been certified by the relevant disciplinary community, who in turn typically consist of those who have been subject to common training—which brings us back to the textbooks and what amounts to a blind trust in their Orwellian view of history.

This last point is related to two features of our epistemic predicament that philosophers often seem to regard as desirable but I treat as problematic. The first concerns what Aant Elzinga, the first Swedish social epistemologist, calls “epistemic drift,” that is, the tendency for epistemic criteria to drift from ones that are likely to push back the frontiers of knowledge to ones that are likely to serve some socially desirable ends. Elzinga introduced “epistemic drift” to highlight potential perversions of the research agenda that result from the existence of a state monopoly on research funding. However, the legacy of epistemic drift is far subtler, namely, the tendency for measures of reliability to be used as surrogates for measures of validity in the evaluation of knowledge claims. In other words, while scientists are officially concerned with whether their theories get closer to
their target realities, they nevertheless measure success in terms of the regularity with which they can achieve more limited goals that are said to “model” the target realities. This epistemic “bait and switch” is most familiar from biomedical research, in which laboratory experiments on rats provide the basis for claims about how to treat disease in humans. But the same also applies to particle accelerator experiments that are alleged to offer insight into the “Big Bang,” or to public opinion surveys that supposedly capture the “mood” of a nation.

Moreover, the most popular versions of analytic social epistemology (e.g., Goldman 1999) are based precisely on this overestimation of reliability. The overestimation lies in supposing that doing something well can trump whether it is what we want to do. In other words, validity can drift into reliability by shifting the goalpost for adequate knowledge over time. Such drifts and shifts are very much the stuff of politics and perhaps even essential for maintaining a stable social order. Social psychologists have coined the expression “adaptive preference formation” to capture this pervasive phenomenon, which enables people to minimize cognitive dissonance by coming over time to want what they are most likely to get. This applies to scientists no less than politicians. Moreover, a demonstrably reliable process has precisely the sorts of virtues that policymakers like. Thus, the hypodermic injection that alleviates the lab rat of an experimentally induced illness in most cases suggests the presence of a closed system that can be inserted—as one might a machine—into some larger healthcare system. Of course, there are complications but it is important in such matters to return periodically to the core intuitions that anchor the epistemological discussions.

The second problematic feature of our epistemic predicament is a generalization of the first. The drift from validity to reliability is part of a more global tendency toward what economists call “screening” and “signaling” criteria, whereby a readily accessed indicator is made to stand for the thing we really care about (Fuller 1996). The relevant euphemisms, “bounded rationality” and “heuristics,” popularized by the late Herbert Simon, were the subject of my Ph.D. dissertation (Fuller 1985). The classic example is letting academic credentials determine judgments of job suitability. In these cases, we imagine that credentials stand for some unknown track record; e.g., Harvard graduates who perform well at their jobs. Yet, this is no more than an endless cycle of hearsay, anecdote, and folklore. Despite all the talk of “reliability” in contemporary epistemology and philosophy of science, there really is nowhere to turn to learn the actual track records of competing research programs. In this respect, we continue to live in a “Baconian fantasy.”

However, Francis Bacon realized that if the state were to be in the business of commissioning scientific projects, then it would need to create public agencies for tracking the projects’ success rates—Consumer Reports of knowledge, so to speak. The sociology of scientific knowledge acquires much of its sting from taking the rhetoric of reliability seriously and then looking for the evidence—and finding that it is either lacking or ambiguous. Of course, some rather sophisticated, probability-based definitions of reliability have been advanced by philosophers, so that we now probably have a much better understanding of what reliability means. Nevertheless, these wonderful definitions have yet to be systematically imple-
mented in the record-keeping practices of scientific disciplines and liberal professions. In short, my anti-reliabilism is motivated less by a tedious skepticism than the simple failure of reliabilists to develop the appropriate institutions for assessing the reliability of knowledge claims.

In most general terms, we too easily allow matters of efficiency to override larger concerns about how, why, and for whom knowledge is produced. A failure to check the work of others because of a lack of resources to do so has metamorphosed into a transcendental justification of “trust” as the ultimate social bond (Fuller 1993b, p. 292ff). Never before has the “two wrongs make a right” principle insinuated itself so insidiously in social life. Perhaps the public is risk-averse not because it places such a high value on reliable knowledge (so as to want to avoid anything that smacks of unreliability), but because it is currently peripheral to the knowledge-policy process. Public caution then is more an expression of political disaffection than brute ignorance. Were the public more directly involved, and hence forced to find out more about what’s going on, they would probably be willing to take more risks, thereby dispelling the Baconian fantasy.

Steve Fuller  
Warwick University  
June 2001

Acknowledgments

Thanks to Alvin Goldman, Philip Mirowski, Francis Remedios, and Stephen Miles Sacks for providing me with opportunities to clarify my views on the foundations of social epistemology.

References


Fuller, Steve: 1993b, Philosophy, Rhetoric, and the End of Knowledge, University of Wisconsin Press, Madison.