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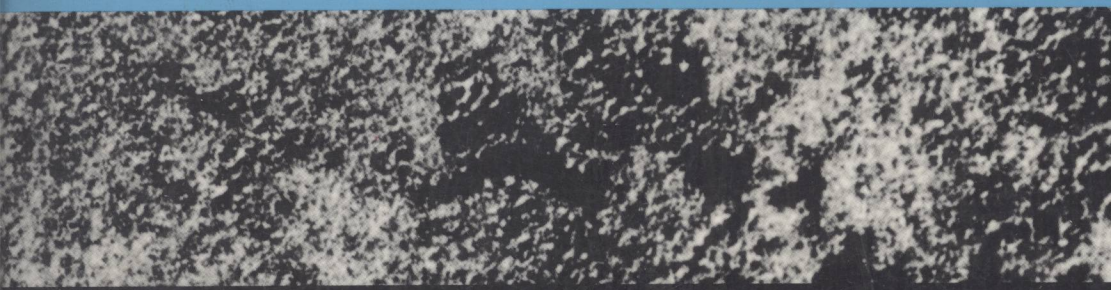

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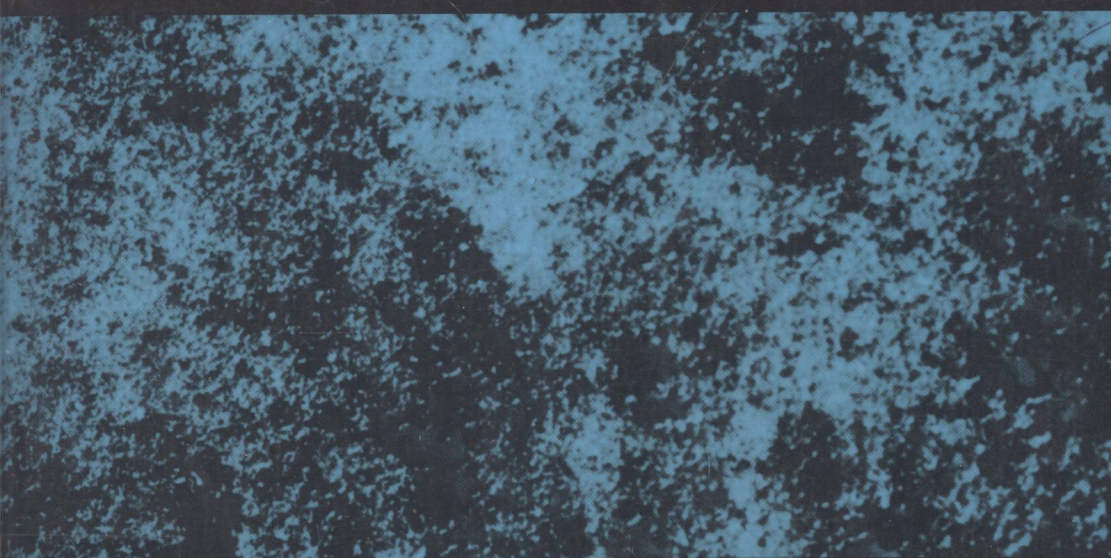

Steve Fuller

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SOCIAL EPISTEMOLOGY

Steve Fuller

Indiana University Press

Bloomington and Indianapolis

E9362583

First Midland Book Edition 1991

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Manufactured in the United States of America

Library of Congress Cataloguing-in-Publication Data

Fuller, Steve 1959-
Social epistemology.

(Science, technology, and society)
Bibliography: p.
Includes index.
1. Knowledge, Sociology of. I. Title. II. Series:
Science, technology, and society (Bloomington, Ind.)
BD175.F85 1988 001 87-31056
ISBN 0-253-35227-4
ISBN 0-253-20693-6 (pbk.)

2 3 4 5 6 95 94 93 92 91

SOCIAL EPISTEMOLOGY

SCIENCE, TECHNOLOGY, AND SOCIETY

Ronald N. Giere and Thomas F. Gieryn, General Editors

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FOREWORD

In this provocative book we see the future of epistemology, or at least one future. This is reassuring, for Richard Rorty, in *Philosophy and the Mirror of Nature*, argued that epistemology has no future; and too many of the dozens of replies to Rorty have defended the honor of the same old questions and answers that we studied at school a decade ago, and five or ten decades ago. To say that the glorious past of epistemology is future enough is really to acknowledge its death as a discipline. Both Rorty and Steve Fuller are intellectually at home on both sides of the Atlantic and even on both sides of the Channel, not to mention both sides of the divide between the two cultures; so this alone will not explain why one asserts what the other denies--the relevance of wider issues to epistemology of science, and *hence* the relevance of the latter to those wider issues. Although no one would mistake him for a quantum logician, Fuller is less poetic than Rorty, and he looks with more favor upon science and upon science policy and science-based public policy.

Disciplines are what Fuller's book is about. His social turn will "turn off" many philosophers, and the manner in which he negotiates his turn will appear reckless to many in the (other) science studies professions--history, sociology, and psychology of science and technology. But the book stands virtually alone in its detailed argument for a social epistemology. It is a rarity among philosophical works in that it *uses* a social conception of inquiry rather than abusing it or at best only "mentioning" it. Happily, the days are past in which philosophers and sociologists spent most of their time together beating up on one another. That is social progress of a modest sort. However, the present book goes well beyond those recent works which favorably mention sociological work and argue that cooperation is *possible*.

A more important "possibility" question is, to what extent are methodological proposals of philosophers socially (and psychologically) possible? Inquiry is a socio-historical process conducted by human beings with the aid of various tools. Many philosophers of science now grant that historical evidence can refute methodological claims. Only a few have seriously asked whether even the leading methodologies are compatible with what we know of the social organization of inquiry. For the majority, it does not really matter. For them methodology may be historically descriptive, to some extent, but it is socially normative in a preemptive way. As for the minority: compatibilism is fine, as far as it goes, but we need specific accounts of how some "causes" can also be "reasons," and vice versa. Fuller appreciates that we need to understand in detail the social realization of the "logic" of inquiry, and the logical upshot of the social organization of inquiry and its products.

In short, *Social Epistemology* lays some of the groundwork, or at least breaks the ground, for a new field of study (or the transformation of an old field). Fuller's endless remarks on disciplinary autonomy, demarcation, the

organization of knowledge and of its institutional vehicles, consensus, the locality of research, expertise, tacit knowledge, authority, and so on, are endlessly suggestive. It seems to me that anyone of epistemological bent looking for relevant, new problems to tackle and new fields to explore need look no further.

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PREFACE

This book is written by a philosopher of science on behalf of the sociology of knowledge. Since I believe that philosophy is primarily a *normative* discipline and that sociology is primarily an *empirical* one, my most basic claim is twofold: (1) If philosophers are interested in arriving at rational knowledge policy (roughly, some design for the ends and means of producing knowledge), then they had better study the range of options that have been provided by the actual social history of knowledge production--a field of study that I assume had originally been explored by rhetoricians and philologists, and more recently, of course, by social scientists. Moreover, if philosophers scrutinize this history fairly, they will then be forced to reconceptualize both the substance and function of their normative theories of knowledge. (2) If sociologists and other students of actual knowledge production wish their work to have the more general significance that it deserves, then they should practice some "naturalistic epistemology" and welcome the opportunity to extrapolate from *is* to *ought*. If these empiricists realized, following Max Weber, that the inferential leap from facts to values is no greater than the leap from our knowledge of the present to our knowledge of the future (a leap that the empiricists would risk in the normal course of their inquiries), they would be relieved of the peculiar combination of fear and loathing which normally prevents them from encroaching on the philosopher's traditional terrain. (A healthy step in the right direction has recently been made in Barnes [1986].) In any case, the alternative is the current state of affairs, whereby science administrators too often justify rather hapless decisions on the basis of some half-digested philosophy of science learned at university.

On the face of it, these claims seem rather reasonable, perhaps even harmless. Yet, the interaction between epistemology and the sociology of knowledge has, in fact, been largely antagonistic. From the standpoint of what I call social epistemology, the reasons for this antagonism are themselves quite interesting, since they raise a whole host questions having to do with the resolution of disciplinary boundary disputes. And not surprisingly, a good portion of this book is devoted to developing some ways of thinking about these questions. For an important decision that the knowledge policymaker will need to make is whether it is better to have one integrated study of our knowledge enterprises (a "Science of Science," so to speak) or the current state of affairs, namely, several mildly affiliated but generally independent fields of inquiry.

The reader should be warned at the outset that, generally speaking, I am not interested in "the problem of knowledge" as classically posed by epistemologists. In other words, the reader will find little in this book that considers whether our beliefs in an external world are veridical or justified. Rather, the key issues for me concern a fairly literal sense of "knowledge production," which includes how certain linguistic artifacts ("texts") become

certified as knowledge; the possible circulation patterns of these artifacts (especially how they are used to produce other such artifacts, as well as artifacts that have political and other cultural consequences); and the production of certain attitudes on the part of producers about the nature of the entire knowledge enterprise (such as the belief that it "progresses"). In fact, to draw the contrast with the classical epistemologist as starkly as possible, I would say that most of the issues that I consider would be *exactly* the sort of thing that a Cartesian demon would need to know in order to construct an illusory world of knowledge for some unwitting *res cogitans*.

No doubt, the classical epistemologist will cringe at the last sentence, concluding that there is nothing more to my theory of knowledge than an empirical account of what people in various communities call knowledge. In response, I would first note that our cringing epistemologist usually turns out to be a closet skeptic, for whom my theory of knowledge is inadequate only for the same reasons that everyone else's is, namely, that it cannot reliably demarcate "real" knowledge from mere opinion. But this global negative judgment alone is cause for suspecting that the classical epistemologist has missed the point of inquiring into the nature of knowledge--which is to define, extend, but surely not to *deny*, humanly possible epistemic practices. This must seem a rather obvious point to the nonphilosopher, yet the classical epistemologist's blindness to it may be excused by recalling that the *super*human (in a word, God) has traditionally set the standard of epistemic excellence. Still, I hope that after reading this book, the classical epistemologist will appreciate that I am sensitive to a basic fact that has often animated a skeptical turn of mind: to wit, that our knowledge claims cover less ground with less certainty than we ordinarily realize.

As currently practiced, the branches of philosophy devoted to the nature of knowledge--epistemology and the philosophy of science--rest on a couple of elementary fallacies. On the one hand, philosophers treat the various knowledge states and processes as properties of individuals operating in a social vacuum. They often seem to think that any correct account of individual knowledge can be, ipso facto, generalized as the correct account of social knowledge. For example, the assertibility conditions for a scientific claim are typically defined in terms of the evidential relation that the knower stands to the known, without taking into account the epistemic states of other knowers whose relations to one another and the known would greatly influence the assertibility of the scientific claim. And insofar as this slide from the individual to the social has been implicit instead of argued, philosophers have committed *the fallacy of composition.*

On the other hand, philosophical accounts of the individual knower are sometimes quite perspicuous, but not because they have isolated real features of individual cognition. Rather, these accounts have identified inference schemas, so-called logics of justification, and scripts that have persuasive force in the public exchange of information. Whether these schemas and

scripts constitute the structure of belief formation in all rational individuals is immaterial to their social import, which rests solely on members of the relevant cognitive community recognizing that such rationally displayed information commands their consideration. Consequently, philosophers can frequently slip into committing *the fallacy of division* by assuming that a feature of the knowledge enterprise that appears primarily at the level of social interaction is, ipso facto, reproduced (by some means or other) as a feature of the minds of the individuals engaged in that interaction.

Why do philosophers tend to commit these two fallacies when discussing the nature of knowledge? My own diagnosis points to a confusion between what is *intended* and what is *effected* in the course of producing knowledge. When epistemologists commit the fallacy of composition, they suppose that one can predict whether a claim is likely to pass as knowledge in a particular cognitive community on the basis of what most of the community's members believe. Likewise, when epistemologists commit the fallacy of division, they assume that the best explanation for why a cognitive community officially treats a given claim as knowledge is that most of the community's members believe the claim. However, both inferences greatly underestimate the influence exercised by each member's expectations about what is appropriate to assert in his cognitive community, as well as each member's willingness to discount his own personal beliefs and conform to these canonical expectations--if only as a means of maintaining his good standing in the cognitive community. In short, then, in my view epistemic judgment has much of the character of identifying and anticipating trends in the stock market.

Lest the reader think that I have an entirely consensualist approach to social epistemology, I should emphasize that what matters, from the standpoint of the smooth operation of the knowledge process, is that there *appears* to be a conformity in epistemic judgments. However, this appearance need not run any deeper than a similarity in the style in which those judgments are delivered, which can, in turn, be easily monitored by the various gatekeepers of the cognitive community. Not surprisingly, then, as cognitive communities such as disciplines expand in time and space, it becomes more likely that several teams of researchers will assent to the same set of sentences but apply them in ways that suggest that those sentences have quite different meanings. This leaves us with a picture of the knowledge enterprise which, on the textual surface, seems rather uniform and systematically regulated, but which, at the microlevel of actual usage, is revealed to be only locally constrained. The radical duality suggested here may be encapsulated by the thesis that, because of the ease with which it can conceal epistemic differences, *the communicative process itself is the main source of cognitive change*. When writing in a more "humanistic" idiom, I refer to the consequences of this picture as *the problem of incommensurability*, whereas I refer to it as *the elusiveness of consensus* when writing in a more "social scientific" vein.

The book has been organized in the interest of "today's reader," someone who rarely reads a book cover to cover in one sitting but dips into a chapter here and there (though, of course, the book *should* be read in order of presentation). Consequently, each chapter can be read by itself without too much loss of context, and there are periodic references to earlier and later chapters of relevance. Since particular audiences have particular needs, I also recommend the following reading plans. Everyone should read at least chapter 1, and preferably all of part one. *Humanists* should also read the chapters in part two and Appendix B in part three, while *social scientists* should read all of part three, and *administrators* should read the chapters in part four. *Philosophers of science* will, with some luck, find something of interest everywhere, though *epistemologists* and *philosophers of language* might confine themselves to part two, while *social and political theorists* might prefer parts three and four.

To make the most use out of this text, the reader should regard it, not as the usual monolithic monograph, but as a parcel of provocations, a sourcebook of ideas, and directions for further research. Needless to say, I welcome criticism so as to afford me the opportunity of getting it right in the next book, tentatively titled *Philosophy of Science and Its Discontents* (Westview Press, 1988/9). Footnotes have been eliminated to facilitate reading, though readers will hopefully find the references cited an aid to their own research, especially in suggesting conceptual links between fields of inquiry not normally drawn together. Finally, references to "he," "him," and the like are also a matter of convenience and should thus be understood in a gender-neutral manner.

This book began to emerge in 1983 and was largely completed by 1986. A version of chapter 1 appears in a special issue of *Synthese* devoted to social epistemology, edited by Fred Schmitt, who is undoubtedly the most careful and stimulating philosophical reader that I have yet run across. A portion of chapter 2 was a response to a paper by Margaret Gilbert, delivered at the American Philosophical Association meetings. Chapter 3 is an expanded version of a talk given in the Harvard History of Science colloquium series. Everett Mendelsohn is to be thanked for his generous invitation. (A note of thanks, also, to Hilary Putnam and the Harvard philosophy graduate students, for their challenging and illuminating remarks.) Chapters 5 and 11 were originally delivered at the University of Colorado History and Philosophy of Science Colloquium series. Parts of these two chapters have appeared in *Philosophy of the Social Sciences, Explorations in Knowledge,* and *EASST Newsletter*. Here I would like to thank Patrick Heelan, Gonzalo Munevar, Arie Rip, and Howard Smokler for their informative, encouraging, and sometimes critical, remarks. Part of chapter 6 was delivered at the annual meeting of the International Association for Philosophy and Literature. Appendix B was originally given at the annual convention of the Speech Communication Association. Foremost among my friends in this field has been Charles Willard. A version of chapter 7 has appeared in a special issue of *Pacific Philosophical Quarterly,* while a version of chapter 8

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has appeared in *4S Review*. Here I have found Steve Woolgar's writings invaluable. Chapter 9, the only one based on a chapter of my doctoral dissertation, has, in turn, been the basis of a symposium paper given at the Philosophy of Science Association. Ted McGuire and Ken Schaffner are to be thanked for much of the scholarship which graces that chapter.

More general thanks go to my long-standing cronies, David Gorman, editor of *Annals of Scholarship*, and James O'Brien, notes and reviews editor of *The Yale Law Journal*, both for their fierce independence and loyalty in many matters. Ron Giere and Tom Nickles did the most to get this book accepted for publication, while Bob Sloan and the editorial staff at Indiana University Press have since facilitated matters, in conjunction with Jim Roberts of Publishing Resources Incorporated, Boulder. Richard Steele, managing editor for Taylor & Francis Ltd., has indirectly promoted the writing of this book as diligent midwife to a journal I have recently started, also called *Social Epistemology*. The philosophy department at the University of Colorado has been the most pleasant academic environment in which I have so far worked. However, I could always count on Georges Rey to make sure that the pleasant atmosphere did not slip into a dogmatic slumber. In fact, I must confess that Georges has been the only person to make me doubt (albeit, for a few fleeting moments) the fundamental notions in this book. My students have also been a constant source of various forms of stimulation, though a special place must be accorded to my research assistant, Stephen Downes. Finally, my biggest debt is to my mother, who knew all along that this was going to happen.

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PART ONE

ISSUES IN DEFINING THE FIELD OF SOCIAL EPISTEMOLOGY

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