Acknowledgments

Although I wrote this book, I cannot take full credit (or, for that matter, blame) for its contents. The book explores the possibility of developing what I am calling a *postanalytic* approach to the study of scientific practices. As should be obvious throughout, this orientation is strongly influenced (perhaps infected) by Harold Garfinkel's ethnomethodological approach to situated practical action and practical reasoning. For the past twenty years, I have had the benefit of reading numerous unpublished drafts of Garfinkel's writings and attending many lectures and seminars in which he and his students discussed and demonstrated novel ways to investigate the production of social order. The specific references I have made in this book to published and unpublished writings can cover only a small part of what I learned from Garfinkel, his colleagues, and his students, including Eric Livingston, Albert (Britt) Robillard, George Girton, Ken Morrison, Ken Liberman, Richard Fauman, Doug Macbeth, Melinda Baccus, and Stacy Burns. My initial efforts to understand ethnomethodology were aided immeasurably by close friends and colleagues, including David Weinstein, Alene Terasaki, Bill Bryant, and Nancy Fuller, with whom I shared a preoccupation with the question "*What in the world* was Harold talking about?" Garfinkel also read an earlier draft of this book and gave me specific and helpful comments on it.

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manuscript, and for teaching me most of what I know about Wittgenstein's later writings. My debt to David Bogen is both pervasive and detailed, especially in Chapter 6, which includes arguments, examples, and some revised passages from coauthored publications and conference presentations. Although the relevant passages are written "in my hand," there is no separating what those passages say from what I have gained from our many conversations and collaborative studies.

My access to the issues and critical debates in the social studies of science has been helped by collaborations, editorial advice, critical exchanges, and many enjoyable conversations with John Law, Steve Woolgar, Sam Edgerton, Gus Brannigan, Andy Pickering, Trevor Pinch, Steven Shapin, Joan Fujimura, Bruno Latour, David Edge, Susan Leigh Star, Harry Collins, and others whom I have neglected to mention. I am also grateful to David Bloor for his role in a critical exchange, parts of which I have incorporated into Chapter 5. Although it came late in the course of my preparation of this manuscript, I also benefited from a visiting appointment in the science studies program at the University of California at San Diego in 1991–92. I was especially informed by the debates and discussions among students and faculty in the core seminar in history, philosophy, and sociology of science that I cotaught with Robert Marc Friedman, Jerry Doppelt, and Chandra Mukerji.

Much of what I wrote in this book was dredged up from a computer hard disk on which I had deposited files, drafts, and notes for various projects and papers. As a result, I intermingled the contents of this book with parts of several papers that were published separately. While doing so, I selected passages, examples, and arguments that were relevant to the overall aims of this book, and I reshaped them accordingly.

I would also like to thank the editors and staff at the New York office of Cambridge University Press for expediting the publication of this book, and I am especially grateful to the three anonymous reviewers contacted by Cambridge University Press who gave me helpful advice and constructive criticism. Finally, I am indebted to Nancy Richards for her loving support, patience, and tolerance while living for countless hours with an asocial writer of social texts.

Introduction

Nobody doubts the significance of science in modern society. Science is often held responsible for spurring the technological transformations, the rises in population, and the shifts in economic production and sources of inequality that characterize the modern landscape. At the same time, nobody seems to have figured out just what science is, and how it is distinguished from other modes of knowledge. Debates persist in the philosophy, history, and sociology of science about how science differs from more commonplace modes of reasoning and practical action. Many participants in these debates have grown doubtful about whether it even makes sense to speak science as a coherent method, separate from the economic interests, material culture, and specialized skills that distinguish the different subfields of biology, chemistry, astronomy, physics, and the like. The once unquestionable conviction that science must be different from "mere" political opinion, untested speculation, and commonsense belief has recently taken a beating, and the defenders of science are nowadays asked to account for how science is not patriarchal or to explain how it is not an extension of Western colonialism.

In this volume I do not intend to add fuel to such debates so much as to suggest how we might develop more differentiated conceptions of the sciences, scientific methods, and the relationship between scientific and commonsense knowledge. I do not try to solve the problem of defining "science" or the problem of demarcating science from other modes of reasoning and practical action. Instead, I suggest a way to investigate the sciences and to respecify[1] the topics that so often come up in discussions of science, topics like "observation," "representation," "measurement, "proof,"

[1] This term is taken from Harold Garfinkel, "Respecification: evidence for locally produced, naturally accountable phenomena of order, logic, reason, meaning, method, etc., in and as of the essential haecceity of immortal ordinary society (I) – an announcement of studies," pp. 10–19, in G. Button, ed., *Ethnomethodology and the Human Sciences* (Cambridge University Press, 1991). Briefly, I understand a "respecification" of these topics to mean not a redefinition of the meaning of terms but a way of investigating the different activities in which "order," "logic," "meaning," and so forth are locally and practically relevant.

and "discovery." This agenda derives from my interest in two specialized modes of investigation – ethnomethodology and the sociology of scientific knowledge – that are usually considered to be subfields of sociology.

Considered as "parts" of sociology, ethnomethodology and the sociology of science are relatively minor fields. Ethnomethodology is commonly said to be the study of "micro" social phenomena – the range of "small" face-to-face interactions taking place on street corners and in families, shops, and offices – and the sociology of science is said to investigate one of the several modern social institutions. Neither is given much space in conventional sociology textbooks. In the heartland of sociology, far more attention is given to the "larger" social and historical forces that give rise to and maintain systems of economic production, labor markets, bureaucratic organizations, religious and political ideologies, and social classes. Ethnomethodology and sociology of science also are marginal to the cutting edge of social science methodology. Neither area is noted for using the most recently developed quantitative methods of data analysis.[2] More often, they use "soft" modes of research, such as historical case study, ethnography, interviewing, and textual criticism.

Ethnomethodology and the sociology of science also happen to be the two fields in which I work, so naturally I am inclined to argue for their importance, and I do so in this book. Although I believe that professional sociologists should pay more attention to the two areas, my primary objective is not to persuade sociologists to allot more space on the program to them. Rather, I am more interested in arguing for the transdisciplinary relevance of ethnomethodology and the sociology of science. I propose that they are of interest not because of the "parts" of society they investigate, but because of their overridingly *epistemic* focus. They offer distinctive empirical approaches to investigating the production of knowledge, and they enable a refinement of contemporary discussions on the nature and consequences of scientific and technological rationality.

Sociology and transdisciplinary critical discourse

Sociology currently faces an interesting set of circumstances. With the emergence of a transdisciplinary critical discourse in numerous historical, philosophical, and literary fields, many academic scholars and researchers

[2] There was a time when sociologists of science helped develop applications for sociometric methods of network analysis. Sociologists such as Nicholas Mullins, Diana Crane, Derek De Solla Price, and many others developed bibliometric maps of "invisible colleges" in various scientific fields, by systematically representing the patterns of citations between research reports. For example, see Y. Elkana, J. Lederberg, R. K. Merton, A. Thackray, and H. Zuckerman, eds., *Toward a Metric of Science: The Advent of Science Indicators* (New York: Wiley, 1978).

have begun to appreciate the thematic importance of social practices. For lack of a better term, I use the phrase *transdisciplinary critical discourse* to speak of the various antifoundationalist and "post-ist" movements – poststructuralist, postmodernist, postconventionalist – in philosophy, law, literary studies, and social science. These are associated with various appropriations and criticisms of the writings of Foucault, Habermas, Derrida, Gadamer, Rorty, Barthes, Deleuze, Lyotard, and, from an earlier generation, Wittgenstein, Heidegger, Merleau-Ponty, Benjamin, and Dewey.

An interest in "epistemology" is often said to unite the various lines of antifoundationalist research and debate, although it can fairly be said that the legacy of Wittgenstein and Heidegger might best be characterized as antiepistemological. In any event, with the eruptions of feminist and other politicized modes of textual criticism in every humanities and social science discipline (and, to an extent, in biology, archaeology, and some of the other natural sciences as well),[3] textual criticism has merged with social criticism, and (anti-)epistemology has become deeply textual and sociological.[4] Sociology's traditional topical concerns – race, class, gender, power, ideology, technology, symbolic communication, and the social conditioning of language – have been taken up in countless discussions and debates throughout the humanities and human sciences.

At the same time, participants in these debates rarely seem to think that it would be worthwhile to consult the pages of the *American Sociological Review* and related professional journals. This is understandable, since the latest sociological models of status attainment and the advances in rational-choice theory are worse than irrelevant; they are *symptoms* of the very mode of discourse criticized by antifoundationalist philosophers and literary theorists. Moreover, vernacular concepts like *race, class,* and *gender* are featured in highly contentious public discourses, so that a strategy of de-politicizing

[3] See Sandra Harding, "Is there a feminist method?" *Hypatia* 2 (1987): 17–32; Donna Haraway, "Situated knowledges: the science question in feminism and the privilege of partial perspective," *Feminist Studies* 14 (1988): 575–99; Evelyn Fox Keller, *Reflections on Gender and Science* (New Haven, CT: Yale University Press, 1984); Alison Wylie, "The constitution of archaeological evidence: gender politics and science," in P. Galison and D. Stump, eds., *Disunity and Contextualism: New Directions in the Philosophy of Science Studies* (Stanford, CA: Stanford University Press, forthcoming); and Athena Beldecos, Sarah Bailey, Scott Gilbert, Karen Hicks, Lori Kenschaft, Nancy Niemczyk, Rebecca Rosenberg, Stephanie Schaertel, and Andrew Wedel (The Biology and Gender Study Group), "The importance of feminist critique for contemporary cell biology," *Hypatia* 37 (1988): 172–87.

[4] What I have called *transdisciplinary critical discourse* is widely regarded as a position of the "left," since it seems most compatible with criticism of the political and cultural status quo ante. Whether this is so, however, is itself a contentious matter, and some proponents of antifoundationalism argue that it is mistaken to assume that "radical" epistemology and "radical" politics are part of a common enterprise. See Stanley Fish, *Doing What Comes Naturally: Change, Rhetoric, and the Practice of Theory in Literary and Legal Studies* (Durham, NC: Duke University Press, 1989), p. 350.

these concepts in order to treat them as variables in explanatory models has limited appeal for participants in the political and intellectual debates of the day.

Of course, not all sociologists go along with the scientistic style of research that dominates American sociology. Quantified and rationalized approaches to social phenomena are anathema to many sociologists, and the discipline is presently undergoing an intensification of its chronic crisis. As always, the crisis concerns whether sociology should continue to conduct itself as a late-blooming "infant" science or to take a more radically interpretive and humanistic approach. But even this debate tends to get caught up in archaic antinomies that no longer have a place in antifoundationalist discourse. Debates about micro versus macro orders of analytic scale, structure versus agency, science versus humanism, and quantitative versus qualitative methods tend to reiterate the familiar conceptual oppositions that many contemporary philosophers and literary scholars have endeavored to put aside. Somewhat late in the game, a growing number of sociologists have begun to appreciate postconventionalist, poststructuralist, or deconstructionist modes of writing, but their efforts too often amount to weak imitations of the longer-running exercises conducted in other fields. This is a particularly ironic development for sociology, a field that should be in the forefront of the "sociological turn" experienced in so many other disciplines.

For different reasons, ethnomethodology and the sociology of science are exceptions to what I just asserted about the irrelevance of professional sociology. Long before it became fashionable, ethnomethodologists took up the writings of Husserl, Heidegger, Merleau-Ponty, and Wittgenstein and developed a distinctive approach to discourse and practical reasoning, and more recently, sociologists of science have become embroiled in debates associated with "new wave" history and philosophy of science. The writings of Kuhn, Popper, Lakatos, Feyerabend, Polanyi, Hanson, Toulmin, and, more recently, Hacking, greatly influenced the current research programs in the sociology of science, and to a considerable extent, sociologists have contributed to transdisciplinary interests in scientific rhetoric and practical "skills" that have emerged in the science studies field.

Like other contributors to transdisciplinary critical discourse, ethnomethodologists and sociologists of scientific knowledge confront "an ancient tension between a notion of truth as something independent of local, partial perspectives and a notion of truth as whatever seems perspicuous and obvious to those embedded in some local, partial perspective."[5] For the most part, they opt for the latter – antifoundationalist – position by seeking to describe the "achievement" of social order and the "construction" of social

[5] Ibid., p. 5.

and scientific "facts." They explicitly renounce the use of transcendental standards of truth, rationality, and natural reality when seeking to describe and/or explain historical developments and contemporary practices.

Although ethnomethodologists and sociologists of science are often well informed about contemporary philosophical movements, their investigations tend to be more "empirical" (whatever might be meant by that term) than is usually the case for philosophical and humanistic scholarship. They conduct case studies of actions in particular social settings; they pay attention to detail; and they try to describe or explain observable (or at least reconstructible) events. Terms of the trade like *empirical observation* and *explanation* are problematic, given their association with empiricism and positivism, but it should be clear that ethnomethodologists and sociologists of science are especially attuned to "actual" situations of language use and practical action. Their studies enable a more differentiated understanding of language, science, and technology than can be gained by making sweeping generalizations about the nature and development of modernity or examining the published reflections of scientists and inventors.

With the "linguistic turn" in postwar philosophy and the renewal of interest in rhetoric and practical action, philosophers and other scholars have begun to appreciate that the traditional epistemological topics of rationality, practical reason, meaning, truth, and knowledge cannot be isolated from the immensely variable linguistic and practical circumstances in which reasons are given for actions, rules are invoked, meanings are explicated, and truth is demanded. Going beyond the ideal-typical investigations of earlier generations of pragmatist and ordinary language philosophers, contemporary scholars now are paying more attention to "actual" usage. For instance, contemporary philosophers of science are increasingly relying on historical and sociological investigations,[6] and some analytically inclined philosophers have turned to cognitive science and artificial intelligence for inspiration and guidance.[7]

In a development that is particularly relevant to my concerns, philosophers like Richard Rorty and Thomas McCarthy suggest that philosophical investigations should draw on ethnographic and related empirical studies of "language games." This is concisely summarized by McCarthy in a discussion of Rorty's "new pragmatism": "Explicating rationality and epistemic authority is not, then, a matter of coming up with transcendental arguments but of providing thick ethnographic accounts of knowledge-producing ac-

[6] See, for instance, Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Science* (Cambridge University Press, 1983); Larry Laudan, *Progress and Its Problems: Towards a Theory of Scientific Growth* (Berkeley: University of California Press, 1977).

[7] See Paul Churchland, *Scientific Realism and the Plasticity of Mind* (Cambridge University Press, 1979).

tivities: 'if we understand the rules of a language-game, we understand all that there is to understand about why moves in that language-game are made.'"[8]

As McCarthy goes on to say, ethnomethodological studies offer an especially appropriate resource for antifoundationalist investigations of practical action and situated rule use.

Fragmentary programs and complex interweavings

My task in this book would be much easier if I could simply present coherent lessons from the literatures in ethnomethodology and sociology of science. Unfortunately, I cannot pretend to do this, and so I am compelled to carry out immanent critiques of both approaches while reconstructing them for expository purposes. Thus far, I have characterized these two fields as though each exemplified a unified approach to a subject matter. This is far from the case. Although both fields are small enough that most practitioners know, or at least know of, one another and although both include specialized journals and commonly recognized landmark writings, neither is integrated by a single set of epistemic commitments. Both ethnomethodology and the sociology of science include confusing arrays of research programs, and both fields harbor an entire range of epistemic commitments. Virtually all of the familiar divisions between formalist versus antifoundationalist, value-free versus politicized, and positivistic versus reflexive modes of inquiry appear in the disputatious literatures of ethnomethodology and the sociology of science, and virtually every familiar position in the philosophy of language, science, and action has been expounded at one time or another.

To compound the expository difficulties, both fields, and especially ethnomethodology, can be notoriously difficult to understand. This is especially the case for some of the best work in these fields. It is also very difficult to do ethnomethodology and the sociology of science in an innovative way. Numerous studies pass themselves off under the banners of ethnomethodology and the "new" sociology of science, without strongly exemplifying the radical initiatives in those areas. Consequently, I need to be selective when I characterize ethnomethodology and sociology of science. But more than that, I need to do a great deal of critical preparation before recommending the research in either or both of these fields to scholars in a transdisciplinary community.

My expository task is also made difficult by the complex interweavings among different programs in ethnomethodology and sociology of science. As

[8] Thomas McCarthy, "Private irony and public decency: Richard Rorty's new pragmatism," *Critical Inquiry* 16 (1990): 355–79, quotation on p. 359. Quotation from Richard Rorty, *Philosophy and the Mirror of Nature* (Princeton, NJ: Princeton University Press, 1979), p. 174.

I explain in Chapter 1, ethnomethodology was founded in the late 1950s by Harold Garfinkel, and shortly thereafter it became familiar as a phenomenologically inspired program for studying ordinary discourse and practical reasoning. From the outset, Garfinkel and his colleagues became infamous for their criticisms of established theoretical and methodological approaches to sociology. These criticisms, along with some of the conceptual themes developed by ethnomethodologists, influenced subsequent research and argumentation in the sociology of science.

In the 1970s, a group of British scholars broadened the scope of the sociology of knowledge and began to investigate the social production of knowledge in the "exact" sciences and mathematics. The early social studies of scientific knowledge were programmatic or historical in focus, but by the mid-1970s a few researchers hit on the idea of treating contemporary scientific laboratories as workplaces in which knowledge and facts were "constructed" or "manufactured," and they began to conduct what came to be known as *laboratory studies:* observational studies organized around some of the themes that had been raised earlier by ethnographic and ethnomethodological studies of other practical activities.

Roughly at the same time, and in an independent development, Garfinkel and a few of his students began to pay serious attention to the discourse and practical actions of laboratory scientists and mathematicians. Although there were, and continue to be, affinities between these studies and the larger body of studies in sociology of scientific knowledge, they differ in a number of important respects. To examine these differences can be very confusing, among other things, because Garfinkel and his students have developed an approach that differs in significant respects from other programs in ethnomethodology.

The term *ethnomethodology* has, to a large extent, taken on a life of its own, and it is often used casually to describe any of a variety of ethnographic or hermeneutic approaches to situated social practices. While recognizing that an attempt to distinguish an authentic "ethnomethodology" from various pretenders would be a tendentious exercise in hairsplitting and internecine rivalry, I think there is a need to clarify what the approach does or can promise. Although as I have suggested, ethnomethodology and sociology of science offer distinctive empirical – although not necessarily empiricist or foundationalist – approaches to epistemology's traditional topics, their radical potential has been undercut by recent developments in both fields. Just as their studies are beginning to be appreciated in the wider field of science studies, constructivist sociologists of science have become caught up in skeptical questions about their own research. This concern with what is sometimes called *reflexivity* has worked to the detriment of the naive energy

that once inspired studies of "actual" scientific practices.

At the same time and especially in the United States, the older programs in functionalist and institutional sociology have coopted many of the radical initiatives raised by the constructivists. A similar fate has befallen ethnomethodology. At the very time when philosophers like McCarthy and literary critics like Stanley Fish mention ethnomethodology as an exemplary antifoundationalist approach to discourse and social practice, much of the research in the field has taken a decidedly foundationalist turn. The spin-off program of conversation analysis has become the most visible exemplar of ethnomethodology in the fields of sociology, linguistics, and communication studies. Conversation analysts have advanced increasingly formalist and foundationalist claims about the organization of language use, and many of them treat Garfinkel as a distant "father figure" whose radical initiatives are now mainly of historical interest. At the same time, as I discuss at length, Garfinkel's continuing program of studies offers a strong alternative to the formalist and foundationalist approach advocated by many of the more influential conversation analysts.

Given these complications, I do not want to construct an overview of ethnomethodology and the sociology of science, in the sense of presenting a comprehensive taxonomy of the different styles of research represented in the two fields. Rather, my endeavor is far more tendentious and destabilizing. I argue that studies in ethnomethodology and sociology of science not only offer critical purchase on topics in epistemology and social theory but also provide leverage for an immanent critique of the modes of explanation and analysis that are employed in both fields. The sociology of science offers critical leverage against some of the scientistic tendencies expressed in many ethnomethodological and conversational analytic studies. At the same time, ethnomethodological studies offer what I believe is a more sophisticated understanding of language use and practical action than is found in constructivist sociology of science. Consequently, although I recommend ethnomethodology and the sociology of science as research fields that offer empirical approaches to epistemology's traditional topics, I devote a great deal of critical attention to questions about just how these fields can more effectively address those topics.

It also has become clear that there is no one-way street between empirical studies of practical actions and philosophical approaches to discourse and practical reasoning. I cannot simply insist that ethnomethodology and the sociology of science provide empirical foundations for discussions on epistemological issues. Nor can I simply attribute developments in these fields to a priori philosophical commitments. It is certainly the case that a great deal of (often dubious) philosophy is advanced under the banner of empirical sociology, but philosophers and humanities scholars are no less likely to

advance dubious claims about society, language, technology, and science when addressing the condition of "modern" or "postmodern" knowledge.[9] The systematic differences between the ways that ethnomethodologists and sociologists of science take up epistemology's topics demonstrate that there is no unequivocal standard for what counts as "empirical" sociological research. Consequently, rather than ending familiar philosophical debates about meaning, rationality, objectivity, and the like, the programmatic claims and "empirical" research strategies in ethnomethodology and sociology of science position themselves within those debates.

Ethnomethodologists and sociologists of science tend to draw on phenomenology and Wittgenstein's later writings, but even though their philosophical commitments remain significant, their research is not philosophical in any established sense. Although there is no clear-cut basis for separating the sense and adequacy of the empirical claims advanced by these two research programs from various discursive accounts of language and knowledge advanced by philosophers, I argue that they offer treatments of epistemology's topics that are neither philosophical nor sociological in the usual sense.

The plan of this book

This book provides the theoretical policies for a set of empirical studies and exercises that I intend to publish in later work. It is a review of research in ethnomethodology and the sociology of science that sets up a critical dialogue within and between the two fields. The first three chapters focus mainly on developments in sociology. Chapter 1 discusses the "invention" of ethnomethodology and reviews some of the themes and developments associated with the research program. Chapter 2 traces the development of a "new" sociology of knowledge that attempted to broaden the application of Mannheim's "non-evaluative total conception of ideology" and to displace Merton's functionalist program for studying scientific norms and institutions. Chapter 3 presents a critical discussion of the more prominent programs in the new sociology of scientific knowledge: the "strong program" in the sociology of science, the "empirical relativist" program, the ethnographic "laboratory studies," and others.

[9] For example, see Heidegger's essay, "The question concerning technology," pp. 3–35, in Martin Heidegger, *The Question Concerning Technology and Other Essays,* trans. William Lovitt (New York: Harper & Row, 1977). Heidegger offers some illuminating conceptual rubrics, but his pronouncements are launched from such abstract heights that they beg a more differentiated examination of the history of science and technology. Lyotard's much celebrated "report" on the postmodern condition is another conspicuous example. Although Lyotard does draw on the literature in the social studies of science, his claims are extraordinarily sweeping and unsubstantiated. See Jean-François Lyotard, *The Post-Modern Condition: A Report on Knowledge,* trans. G. Bennington and B. Massumi (Minneapolis: University of Minnesota Press, 1984).

The next three chapters broaden the scope of the discussion by examining some of the problems associated with the empirical approaches to language, practical action, science, and technology discussed in the previous chapters. Although many ethnomethodologists and sociologists of science assert that their studies are empirical and that they no longer need to address "philosophical" considerations concerning them, I believe that we cannot so easily put aside the chronic problems associated with skepticism, scientism, and linguistic representation. These problems often are peremptorily "solved" by programmatic claims to the effect that an accumulation of empirical findings justifies calling an end to "metatheoretical" debate. Although I do not claim that definite solutions to such problems can be discovered by more careful study of the philosophical literature, I do contend that many ethnomethodologists and sociologists of scientific knowledge hold dubious and self-contradictory preconceptions of language, science, and practical action. As a sociologist I am in no position to advance a philosophy of science that corrects such "deficiencies," but I hope to establish that many of the topics of epistemology can be addressed in an interesting and informative way by examining contemporary scientific practices. My recommendation is not to adopt a "new and improved" set of assumptions about language, practical action, science, and knowledge but to suggest how these and other epistemic matters should be *topicalized* for empirical investigation. This recommendation, of course, can itself be criticized for making undefended assumptions or for setting up an infinite regress, but I argue that epistemic matters can be reviewed without falling into the aporias of an endlessly skeptical "reflexivity."

Chapter 4 discusses ethnomethodology's (and, to a lesser extent, the sociology of scientific knowledge's) debt to phenomenology and existential philosophy. After a brief discussion of Husserl's phenomenological explication of the mathematization of nature, the chapter lays out an ethnomethodological conception of the "local production" of technical actions. The latter part of the chapter then criticizes the way that phenomenological research (particularly that of Alfred Schutz) has been incorporated into "protoethnomethodological" studies that draw a distinction between "scientific" analysis and "everyday" knowledge.

Chapter 5 examines the significance for research in ethnomethodology and the sociology of scientific knowledge of Wittgenstein's later investigations of language and mathematics. The chapter begins with a discussion of how a skeptical interpretation of Wittgenstein's argument about rules in arithmetic has become an established tenet in the sociology of science. I then look at some of the criticisms of rule skepticism in post-Wittgensteinian philosophy while arguing that Wittgenstein's writings problematize the aims of an explanatory sociology of knowledge just as much as they undermine foundationalist philosophy. I finish the chapter by suggesting how ethnomethodology offers a way out of the paradoxes of a relativist or

skeptical sociology of knowledge.

Chapter 6 describes and criticizes the program in "molecular sociology" that became established in the field of conversation analysis (CA). CA was once closely affiliated with ethnomethodology, and it is often considered to be ethnomethodology's most successful empirical program. I believe, however, that CA's descriptive program has taken a formalist and foundationalist path that differs profoundly from the orientation to practical actions that is taken in ethnomethodological studies of science. By critically expounding on these differences, in reference to the analytic language and communal research strategies in CA, the chapter introduces a proposal for a "postanalytic ethnomethodology" that is developed in Chapter 7.

Chapter 7 addresses what I believe to be a common problem faced by ethnomethodology and sociology of scientific knowledge: how to analyze particular settings of social practice without trading on the terms by which members make partisan claims and conduct their disputes. I contend that there is no escape from this problem, but that the problem arises out of a misunderstanding. The idea that there is a general problem in the first place implies the possibility of such an escape, and if we recognize that the possibility of escape is an illusion, the problem vanishes. I suggest that ethnomethodological studies of science provide a way to examine epistemic activities without buying into dualistic oppositions between scientism or subjectivism.

In later work, I intend to build on the program outlined in this volume and to present a series of studies and exercises that demonstrate how a postanalytic ethnomethodology can *respecify* selected topics in philosophy and history of science. These topics include observation, representation, measurement, discovery, and explanation. By respecifying them, I hope to treat these familiar epistemological topics as terms that gloss over immensely varied practical phenomena.[10] The aim of such respecification is to provide a set of detailed and vivid cases for describing the locally organized production of epistemic language games, thus enriching our understanding of the complex fields of activity called science.

[10] My initiatives in this regard are taken from an unpublished source informally known as Garfinkel's "blue book": Harold Garfinkel, Eric Livingston, Michael Lynch, Douglas Macbeth, and Albert B. Robillard, "Respecifying the natural sciences as discovering sciences of practical action, I & II: doing so ethnographically by administering a schedule of contingencies in discussions with laboratory scientists and by hanging around their laboratories," unpublished manuscript, Department of Sociology, University of California at Los Angeles, 1989. For published sources that include some of the arguments from the "blue book,"see Garfinkel, "Respecification: evidence for locally produced, naturally accountable phenomena"; and Harold Garfinkel and D. Lawrence Wieder, "Evidence for locally produced, naturally accountable phenomena of order*, logic, reason, meaning, method, etc., in and as of the essentially unavoidable and irremediable haecceity of immortal ordinary society: IV two incommensurable, asymmetrically alternate technologies of social analysis," pp. 175–206, in G. Watson and R. Seiler, eds., *Text in Context: Contributions to Ethnomethodology* (London: Sage, 1992).