

## Chapter One

# Controversies about Scientific Texts

Researchers in philosophy, sociology, social psychology and anthropology have recently become interested in the discourse of their own and other disciplines.<sup>1</sup> Words such as *text*, *discourse*, *narrative*, and *construction* have become fashionable, and have taken on a number of different, and perhaps inconsistent, meanings. I shall try to place myself in this field of research, with its rapidly shifting disciplinary boundaries, by addressing at the outset two key questions: Why study *scientific* texts? And why study *scientific texts*? In each answer I draw some flexible, perhaps paradoxical, demarcations between the scientific and nonscientific, between text and praxis; other researchers, as I shall show, draw the lines in different places. I will not try to insist here that my approach is correct or even consistent, only that it is methodologically practical.

The questions about the value of studying a *scientific* texts could come from two different groups of people: (1) those who believe scientific knowledge must have a special status, so that scientific texts are, at least in their ideal form, exempt from rhetorical or literary analysis, and (2) those who see scientific knowledge as having no special status, so that the only goal of a study like this one can be to

1. See, for instance, on philosophy, Jonathan Ree, *Philosophical Tales* (London: Methuen, 1987); on biochemistry, Nigel Gilbert and Michael Mulkey, *Opening Pandora's Box* (Cambridge: Cambridge University Press, 1984); on history, Hayden White, *Tropics of Discourse* (Baltimore: Johns Hopkins University Press, 1978), and *Metahistory* (Baltimore: Johns Hopkins University Press, 1973); on anthropology, George Marcus and Michael M. J. Fischer, *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences* (Chicago: University of Chicago Press, 1986); on psychology, Michael Billig, *Arguing and Thinking: A Rhetorical Approach to Social Psychology* (Cambridge: Cambridge University Press, 1987), and Jonathan Potter and Margaret Wetherell, *Discourse and Social Psychology* (Beverly Hills and London: Sage, 1987); John S. Nelson, Allan Megill, and Donald McCloskey, eds., *The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs* (Madison: University of Wisconsin Press, 1987); James Clifford and George Marcus, eds., *Writing Culture* (Berkeley: University of California Press, 1986); Michael Lynch and Steve Woolgar, eds., *Human Studies: Special Issue on Representation in Science* 11 (July 1988).

show that scientific texts can be treated in the same way as literature or political oratory. Those who believe that scientific discourse is essentially different from other discourse—including some realist philosophers of science, some Marxists, and some practising scientists, at least in their polemical statements—point to a distinctive scientific method involving falsification or replicability, to institutions such as peer evaluation and publication, to the position of the scientist in historical processes, or to some quality of the subject matter studied. These distinctive characteristics of science are taken to separate science from the realm of rhetoric and of social processes, so that, however much social factors may enter in any particular case, *real* science always continues, or works best, apart from those factors. But as I shall show, the application of this demarcation is itself a question of rhetoric and social processes; such characteristics as replicability are invoked in order to persuade the audience that some fact or field lies beyond matters of persuasion. Science is like other discourses in relying on rhetoric; it just uses a different kind of rhetoric. Traditional literary critics draw the same sort of demarcation between what can and cannot be studied as discourse, but they draw it from the other side, resisting the application of literary criticism to anything but literary texts.

If science relies on rhetoric, it might seem that I could subsume this study under general studies of discourse formations. It would be convenient for those of us trained in textual analysis if all discourses could be reduced to one discipline, preferably our discipline, so that I would have completed my task if I could find in these texts the major tropes of Northrop Frye's *Anatomy of Criticism* or the categories of Aristotle's *Rhetoric*. But such a project, even if it were successful, would not help to explain why, in our culture, scientific knowledge has a huge authority, and literary criticism, for instance, does not. I shall argue (following some sociologists) that an understanding of the discourse of any discipline depends on a detailed knowledge of that discipline—not just a knowledge of its content, since the construction of that content is what is at issue, but a knowledge of its everyday practices. For my purposes, the crucial difference between articles by a psychobiologist and a literary critic lies not in some quality of the subject matter, not in the fact that one is writing about garter snake hormones and the other is writing about John Ruskin's symbols, but in the form of the article and the kind of rhetoric it allows. The psychobiologist, for instance, can make use of the work of ethologists, ecologists, and chemical assay designers to support his or her claim, whereas the literary critic relies on his or her own authority, and is

likely to invoke other critics mainly to challenge them. Bruno Latour makes this point.

Rhetoric used to be despised because it mobilised *external allies* in favour of an argument, such as passion, style, emotions, interest, lawyers' tricks, and so on. . . . The difference between the old rhetoric and the new is not that the first makes use of external allies which the second refrains from using; the difference is that the first uses only *a few* of them and the second *very many*.<sup>2</sup>

This view of scientific rhetoric is uncomfortable for the nonscientist studying scientific texts; to follow the scientist, as Latour suggests, one has to know about all these possible allies, and about the ways they can be invoked. I cannot become a biologist, but I do focus on just a few areas of research, so that I can deal in some detail with the practices of those specialties.

The question of why one should study *written* texts is raised by science studies researchers who believe that the reliance of historians of science on the published literature has led them away from the actual practices of science. The limitations of what Michael Lynch calls "literary" analysis have been pointed out, for instance, by Peter Medawar, Harry Collins, and Bruno Latour, who says,

No matter how interesting and necessary these studies are, they are not sufficient if we want to follow scientists and engineers at work; after all, they do not draft, read and write papers twenty-four hours a day. Scientists and engineers invariably argue that there is something behind the technical texts which is much more important than anything they write.<sup>3</sup>

These researchers advocate such techniques as ethnography, participant observation, and conversation analysis to get behind the written texts. Certainly studies of the talk and actions of scientists by these and other researchers are crucial for an understanding of science and of scientific writing. But written texts have great advantages as re-

2. Bruno Latour, *Science in Action: How to Follow Scientists and Engineers Through Society* (Milton Keynes: Open University Press, 1987), p. 61.

3. Latour, *Science in Action*, p. 63; Michael Lynch, *Art and Artifact in Laboratory Science: A Study in Shop Work and Shop Talk in a Research Laboratory* (London: Routledge and Kegan Paul, 1985), pp. 143–54; Peter Medawar, "Is the Scientific Paper Fraudulent?" *Saturday Review* 49 (1 August 1964): 42–43; Harry Collins, *Changing Order: Replication and Induction in Scientific Practice* (Beverly Hills and London: Sage, 1985), p. 73.

search material, advantages that have long been taken for granted by literary critics, but are perhaps not sufficiently appreciated either by them or by social scientists:

1. Texts hold still.
2. Texts are portable.

In this study, I want to make a virtue of the necessity of converting material into a written form. Because all the materials I use are written, even the informal comments in the margins of drafts, I can reread them slowly, again and again, display fragments, and read back through them to find other instances of a feature I've just noticed. Because I can use anything written, and need not choose and transcribe special moments, I have an endless stream of material in the overflowing filing cabinets of the scientists I study. This availability for close reading, and this wealth of material, means my reading of scientific texts is not like the normal reading processes of the scientific writers, referees, or readers themselves. But the strangeness of my position, as a literary reader of scientific texts, allows me to bring out features that otherwise pass unnoticed. Only written texts allow for such close reading, because they hold still while one goes over them, and hold still until one can come back to them.

The other advantage of written texts as research material is related to this: texts, unlike conversations or experience, are portable. When Michael Lynch studies a conversation about an electron micrograph between a lab director and a postdoctoral researcher, he is dealing with something local. He can make it available for discussion only by taping it and transcribing it according to conversation analysis conventions, transforming it into a written text that is an idealization of the fleeting moment and complex interaction that Lynch wants to discuss. When I quote a sentence as an example, it can pass from the author's word processor, through a photocopy machine, into my word processor, and into the typeface of this book, all, for my purposes, unchanged. When I quote a published text, anyone can go to a library and look up the same article in another copy of *Nature* or *Sociobiology*. A reader and I can argue about the same thing; the reader and author of an ethnography do not both have access to the same experience. There is certainly a rhetorical advantage in being able to point to an example and say, "There it is in black and white."

It may seem paradoxical that I would defend my use of written texts on the grounds that texts hold still and texts are portable, when in every chapter of this book I shall be arguing that texts must be read as processes, not objects, and that texts change meaning whenever

they change context. Surely, then, the sentence I point to as evidence does not hold still, and is not portable. In literary scholarship, bibliographical scholars could point out that the texts of canonical works hardly hold still, but change with each generation, and reader-response critics could point out that the meanings of works are not really portable between different contexts. These questions are all ways of calling attention to the processes of production and interpretation; I shall be stressing those processes but shall still draw on the practical advantages of words on the page. It remains true that the way one makes an argument in literary criticism, whatever one's approach, is to quote a fragment of a larger text, trusting that the properly guided reader will have the response the critic needs, and that this response will stand in for all the processes the critic is trying to bring out. I can make my sociological argument convincingly, opening up the processes of texts and showing the diversity of interpretations, only because written texts can function as evidence on this basic level. I use written texts, not because I hold them to be in any way primary, but for the practical reason that I can do things with them that I cannot do with other data.<sup>4</sup>

My answers to both these questions—why study *scientific* texts and why study scientific *texts*—are made in relation to the assumptions of someone trained in literary analysis. To approach the first question, I have to reject the assumption that literary and nonliterary texts are essentially different, while recognizing that the practices of science could be different from the practices of other discourses. To answer the second question, I have to make explicit the practical advantages of written texts in arguing for a view of science. Most of my references in this book will be to sociologists of science, but I find, rather to my surprise, that this book is still a literary and rhetorical study. It is literary not because it responds to the latest approaches in literary theory (it does not), but because it uses the skills and draws on the assumptions of someone trained in literature, rather than in the sociology of science or evolutionary biology. How, then, does my literary approach relate to those of researchers drawing on other disciplinary assumptions?

In the following sections, I examine examples, first, of literary and

4. My argument for the methodological advantages of written texts as sources for the sociologist of science parallels the argument for their importance in the history of science. The classic presentation of this relation is by Elizabeth K. Eisenstein, *The Printing Press as Agent of Change* (Cambridge: Cambridge University Press, 1979). Bruno Latour has developed it into the concept of "immutable mobiles"; see *Science in Action*, p. 223.

then of sociological approaches to science. If one considered only the research done before the 1970s, literary critics and sociologists would not seem to have much in common in their approaches to scientific texts. Literary criticism, though it allowed the study of science in relation to literary texts, as part of the cultural background, seemed to exclude any consideration of scientific texts in relation to science. And sociologists of science, though they were very interested in scientific communication and the institutions around publication, didn't seem to be interested in reading individual texts. In both cases the lack of interest in scientific discourse, the exclusion of the nonliterary from literature and of the noninstitutional from sociology, were not incidental oversights, but helped to constitute both disciplines, allowing them to ask their characteristic questions and evaluate the answers.

In examining the sociology of scientific knowledge, I focus on some persistent controversies that deal with the two questions I have already raised, about the demarcation between science and nonscience, and between text and practice. This review does not provide a broad and balanced introduction to the sociology of scientific knowledge,<sup>5</sup> but points out some of the tensions that will surface again and again through this book. I see each of the traditional approaches to scientific texts, through literature, history, or sociology, as avoiding questions of the relation between knowledge and its textual representation. As discourse analysts in various disciplines have shown, to challenge such exclusions is not to expand the methods of literature or history or sociology into some new material, but to transform the discipline.

### ***Traditional Literary Approaches to Scientific Texts***

I can illustrate the usefulness and the limitations of traditional literary approaches to scientific texts by considering a 1968 essay by Dwight Culler, "The Darwinian Revolution and Literary Form."<sup>6</sup> Culler's

5. For introductions to the Strong Programme, see Michael Mulkay, *Science and the Sociology of Knowledge* (London: George Allen and Unwin, 1979), a readable argument for the general reader; Barry Barnes and David Edge's anthology, *Science in Context: Readings in the Sociology of Science* (Milton Keynes: Open University Press, 1982), a selection of important studies; Mary Hesse, *Revolutions and Reconstructions in the Philosophy of Science* (Brighton: Harvester, 1980), a philosophical defense; and Steven Shapin's massive review article, "History of Science and Its Sociological Reconstructions," *History of Science*, 20 (1982): 157–211.

6. Dwight Culler, "The Darwinian Revolution and Literary Form," in *The Art of Victorian Prose*, ed. George Levine and William Madden (New York: Oxford University Press, 1968).

work is appropriate for showing some of the assumptions in the field and the craft skills that are admired in its practitioners. One might think, from the ferocious battles in *Critical Inquiry* or other journals, that literary criticism was an especially heterogeneous and contentious discipline. But while the theory of literary criticism is fragmented into competing schools, there is a powerful consensus in the practice of literary criticism, especially in the daily work of teaching and evaluation. Lacanian and Leavisite, Derridian and Althusserian show remarkable agreement in distinguishing an upper second class and lower second class examination paper (or, in the United States, an A- from a B+ student). Literature has a notion of craft skills more narrowly defined but less explicitly articulated than that of, say, linguistics or sociology. Culler offers a fine example of those craft skills. And I too employ those skills, though I do not employ them so well.

I have chosen this relatively dated essay from the huge bibliography on literature and science, not only because it is a fine essay, perceptive, witty, and surprising in the connections it draws, but also because it can be seen as the forerunner of later studies that relate science to literary form.<sup>7</sup> Culler notes that most earlier studies of the influence of Darwin on literature show how some ideas related to his evolutionary theory are treated in works of literature. In the kind of turn from the thematic to formal analysis characteristic of criticism of the 1950s and 1960s, he sets out instead "to inquire how the form of Darwinian explanation has influenced, or is analogous to, forms of literary expression in the post-Darwinian world" (p. 225).

In Culler's argument, "the form of Darwinian explanation" is the reversal of Paley's argument from the evidence of design in nature to the existence of God the designer. Darwin "has abandoned the teleological explanation, which looks to the future, for a genetic explanation, which looks to the past. . . . Where Paley has taken intelligence to be the cause and adaptation to be the result, Darwin has shown

7. Influential studies of science and literature are Gillian Beer, *Darwin's Plots: Evolutionary Narrative in Darwin, George Eliot, and Nineteenth-Century Fiction* (London: Routledge and Kegan Paul, 1983) and Sally Shuttleworth, *George Eliot and Nineteenth-Century Science: The Make-Believe of a Beginning* (Cambridge: Cambridge University Press, 1984). My own articles, "Nineteenth-Century Popularizations of Thermodynamics and the Rhetoric of Social Prophecy," *Victorian Studies* 29 (1985): 35-66, and "Science for Women and Children: The Dialogue of Popular Science in the Nineteenth Century," in *Literature and Science 1700-1900*, ed. Sally Shuttleworth and John Christie (Manchester: Manchester University Press, forthcoming), are typical examples of work in this area. The collection edited by Shuttleworth and Christie contains a number of historical studies.

that adaptation was the cause and survival the result—survival of those fittest to survive” (pp. 227–28). Culler compares this reversal to those performed by Malthus, Bentham, and Hume in their arguments with earlier thinkers. He finds, because of this juxtaposition of the pompous figure and the ironic questioner, “a fundamental analogy between the Darwinian explanation and the whole comic, satiric tradition” (p. 237). The entertaining turn to Culler’s essay is the application of this formula to a whole range of literary works, from those that might be expected in an essay on Darwin’s influence—*Erewhon* and *Man and Superman*—to those that are a surprise in this context—*Alice in Wonderland*, Pater’s *Renaissance*, and *The Picture of Dorian Gray*. All, Culler argues, share this pattern of reversal, this characterization of fixed old ideas and disruptive new ideas, this confrontation of explanation in terms of design with explanation in terms of chance.

Culler’s performance is a good example of the procedures literary critics take for granted. He focuses on matters of form, selecting a few telling features, organizing them into a pattern, and taking them to define whole texts. He uses comparison and juxtaposition to make these formal features stand out. He is interested in the use one text makes of another text, and in the possibilities for reinterpretation in these juxtapositions. He identifies texts with the authors as represented in the text, and imputes to these authorial personae various intentions and interests. In all this, Culler’s article exemplifies the procedures I will be following in this book.

Culler also exemplifies some habits from academic literary study that I am trying to break. He completely ignores Darwin’s text and its context in scientific discourse. His article is typical of literary studies in tracing influence only in one direction—from science to literary texts. For all its broad range of literary erudition, it refers to no other scientists beside Darwin. The questions of what conventions Darwin was following, what influences he might have felt, what rhetorical purposes he might have had, are not raised, the way they would be raised with any literary author. The scientific works Culler does refer to are all part of a literary canon; his selections are interesting, but all of them could be in a literature course on the Victorian or modern period. This kind of relevance seems to be necessary to justify excursions into the scientific literature. More recent studies of science and literature give greater attention to scientific discourse, but even in the excellent work of, say, Gillian Beer, the ultimate goal is to enrich our understanding of works in the literary canon. Very rarely do literary critics use their skills to help us understand science.

Like Culler and most literary critics, I reject the assumption of some

philosophers and historians that the analyst can abstract meanings from texts, and take these disembodied "themes" as the objects of discussion. But Culler has so little interest in the scientific literature, as opposed to canonical literature, that in an essay on form he does not need to refer to Darwin's text at all. "The form of Darwinian explanation," as it turns out, is not the narrative structure of *The Origin of Species*; it is the relation that Darwin as a figure has to Paley as a figure. Culler gracefully notes in passing that the text does not, in fact, illustrate the form he attributes to Darwin.

As a scientist he was the author of the greatest repartee in nature, but as a man he says he was without wit and that he had a fatality for putting his statements initially in a wrong or awkward form. Surely this is borne out by the form of the *Origin of Species*. Who of us, if we had the opportunity to write such a book, would not begin, as in a drama, by building up Paley and his argument by design with the whole range of existing plausible fact and then, by a quick reversal, bringing it all tumbling down with an explanation so simple and obvious that Huxley would slap his knee and say, "Why didn't I think of that?" and others would wonder and find the new view as satisfying as it was surprising? I do not say that this is the way to get the theory accepted, but simply in order to present it, as a brilliant and paradoxical theory, this is the way. (P. 232)

Culler also gives Darwin an epigram that he "might have said," and comments in a note that "I am not referring to the historical Darwin who in successive editions adopted a compromise position, but to an abstract 'Darwinism' which says sharply what he ought to have said" (p. 246n).

I enjoy the high-handedness with which Culler brushes aside the text that refuses to fit, but I also see a missed opportunity. It is not that this critic, one of the best analysts of Victorian prose, cannot analyze Darwin, but that somehow Darwin, a scientist, falls outside of his area of interest, where John Henry Newman, for instance, does not. I would argue that he cannot include Darwin without undermining the goals of traditional literary analysis, that is, appreciation and evaluation. The terms with which he dismisses Darwin are significant. In his view, Darwin is not a particularly good writer. Darwin should have found a better form to fit the aesthetic appeal of the theory, even if this form would not have fit his rhetorical purpose. Culler denies any interest in what form might have persuaded someone in that particular context to accept the idea; he instead imagines a

context-free presentation that would bring out the qualities admired by literary critics. Clearly it is necessary for such criticism to maintain an independent aesthetic realm, in which works from different contexts can all be evaluated, as for instance when Culler compares *Erewhon* to *Gulliver's Travels* at the end of his essay. Explicitly rhetorical texts suggest another standard of evaluation—the *Origin of Species* is a successful book because it worked, within the discourse of mid-Victorian biology. And any study of the discourse of which it was a part suggests a whole set of interrelations among texts that are not like those of a literary tradition, interrelations that lead us to the immediate context rather than to the timeless.<sup>8</sup> So this exclusion is not an oddity of Culler's, but is part of the structure of the field. If one breaks down this barrier, one cannot do the same kinds of evaluation. One can make subtle, close readings of scientific texts, but they always have to lead to literary texts and literary questions.

If issues in the analysis of scientific texts emerge most clearly in controversy, the issues involved in an analysis like Culler's can be seen most clearly by comparing it to a historical study in the same collection, Walter Cannon's brilliant, perverse essay on "Darwin's Vision in *On the Origin of Species*." It is brilliant in the way in which the historian Cannon performs the literary analysis that the literary critic Culler did not: analyzing the language and structure of the text in the content of the genre as defined in its period. Cannon comments on the form of treatises in the nineteenth century, compiles and compares words Darwin used in his descriptions of several other scientists, relates the rhetoric of his paragraphs to an unconventional notion of scientific logic, and explains the function of each chapter in the development of the book as a whole. Cannon is not, in fact, responding to Culler; he and Culler are both responding to what they see as the limitations of a famous and rather crude essay by Stanley Edgar Hyman. But Cannon's sarcastic swipe at Hyman could apply equally well to any literary critic who writes about scientific texts without considering their scientific and social context: "Perhaps my essay should be read as a test case as to whether a close reading of the given text, and a historical knowledge of the period in question, are useful tools in literary analysis" (p. 154).

Cannon's criticisms of Hyman and of literary criticism in general hit home, but the essay is perverse because the careful analysis of scientific prose leads only to the conclusion that the analysis of scien-

8. Robert Young shows this in his essays in *Darwin's Metaphor* (Cambridge: Cambridge University Press, 1986).

tific prose is not worth doing. For Cannon, as for many historians, the ideas of scientists can be considered apart from any textual representation, so specialists in textual analysis are left with nothing to do with science.<sup>9</sup> Here, ironically, he comes close to agreeing with Culler.

Scientifically, the *Origin* is a classic; biologists have been scrambling for a hundred years to catch up with Darwin's ideas. But verbally it is a rag-and-bone shop. Science took wings in the middle of the nineteenth century, imaginative wings that no other discipline can match even now. Words, logic, evidence, and mathematical consistency tend to strangle a scientist's idea before it can ever be born alive and gasping. . . . In the *Origin* Darwin sees that it is the *habit of looking at things in a given way* which a master scientist transmits to his disciples. How he does this, rhetorically, is of little importance. If this means that literary criticism as practised by professors of English literature at liberal arts colleges is not of much importance in understanding the important books of the world since 1859, I am sorry. (Pp. 172–73)

Cannon and Culler agree that Darwin is not a good writer, and that his ideas can be considered apart from their textual form. In part they are both reacting to an analysis like Hyman's that would focus on a few elaborate and atypical paragraphs as the rhetoric of the work. But they are also defending the cores of their respective disciplinary approaches. In praising Darwin the scientist and dismissing the writer, Culler preserves a realm of the aesthetic. In the same way, Cannon preserves a narrative of the progress of science in a realm of ideas apart from the social and the textual. To grant that these ideas exist only in their textual form would be to tie science to the limited culture of a particular period. Just as aesthetic criteria are the basis of Culler's conclusion, the narrative of progress is the basis of Cannon's, so the

9. For examples of historical work that does focus on textual issues, see Frederick Holmes, "Lavoisier and Krebs: The Individual Scientist in the Near and Distant Past," *Isis* 75 (1984): 131–42, and "Writing and Discovery," *Isis* 78 (1987): 220–35; Jan Golinski, "Robert Boyle: Skepticism and Authority in Seventeenth-Century Chemical Discourse," in *The Figural and the Literal*, ed. Andrew Benjamin, Geoffrey Cantor, and J. R. R. Christie (Manchester: Manchester University Press, 1987), pp. 58–82; and Martin Rudwick, *The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gently Specialized* (Chicago: University of Chicago Press, 1985). Steven Shapin argues that historians of science have never neglected the issues raised by discourse analysts in "Talking History: Reflection on Discourse Analysis," *Isis* 75 (1984): 125–28, his response to Nigel Gilbert and Michael Mulkay's "Experiments Are the Key: Participants' Histories and Historians' Histories of Science," *Isis* 75 (1984): 105–25.

*Origin* is good because "biologists have been scrambling for a hundred years to catch up with Darwin's ideas."

I use the methods of both the literary critic and the historian in this book, but I am trying to answer a question that is not addressed by either of their approaches—in what way do texts contribute to the social authority of science? What I need are not different methods, but a different disciplinary orientation—I need to look at studies that can lead from texts to the social structures of science, not always from scientific ideas to literature, or from scientific texts to disembodied scientific ideas.

### ***Sociological Approaches to Science: The Mertonian Paradigm vs. the Strong Programme***

One reason the sociology of science seems to have little to do with literary analysis is that it was long dominated by a paradigm that leaves the analyst of texts with little to do. And yet the creator of this paradigm, Robert Merton, raised many of the questions about the cultural authority of science that I am trying to raise here. He challenged the popular sense that science involves the confrontation of the individual scientist with nature and so prepared the ground for work like mine. A brief summary of the form in which he put his questions may help show the difference between the approach I am following (once largely British) and that of much of the sociology of science done in the United States:

Merton and his students developed, starting in the 1940s, a framework for analyzing the function of scientific institutions as the harnessing of the motives of individuals to serve the ongoing interest of the scientific community as a whole. The overriding interest of this community was in extending validated knowledge. Within Merton's framework there has been a vast literature on how institutional factors such as the reward system or hierarchies of status serve the progress of science. The Mertonian paradigm seems to allow sociology of science to become like a science, and both Mertonians like Norman Storer and critics like Barry Barnes agree on its influence in shaping the development of the discipline.<sup>10</sup>

10. For a review citing major articles in the British vs. American debate of the early 1970s on the Mertonian paradigm, see Charles Bazerman, "Scientific Writing as a Social Act: A Review of the Literature of the Sociology of Science," in *New Essays in Technical and Scientific Communication: Research, Theory, and Practice*, eds. Paul V. Anderson, R. John Brockmann, and Carolyn R. Miller (Farmingdale, N.Y.: Baywood, 1982).

One Mertonian study that deals with texts is an article by Harriet Zuckerman and Merton, "Patterns of Evaluation in Science: Institutionalization, Structure, and Functions of the Referee System" (collected in Merton's *The Sociology of Science*). For Zuckerman and Merton, texts are an important part of the institutions of science and can be studied statistically in the form of citation data and bibliographical records; nonetheless, but they are treated as if they are just vehicles for the communication and validation of technical knowledge, not in themselves shapes of that knowledge. Zuckerman and Merton separate one part of science that is social, and subject to analysis in terms of institutionalization, structure, and function, from another part of science that is technical and that relates to the natural world. The interpreter approaches the texts objectively, as data to be tabulated, rather than as part of discourse to be interpreted. I shall try to show that this framework limits the analyst of texts who wants to ask about the cultural authority of science. It seems to me that the end result of Merton's approach in this article is a defense of science and the uncritical adoption of its methods for sociology.

The first part of Zuckerman and Merton's article is a fascinating brief history of the earliest years of the refereeing system, at the founding of the *Philosophical Transactions* of the Royal Society. Merton, who did pioneering historical work on the social background of seventeenth-century science, is able to give a historical perspective which sociology sometimes lacks. The assumption that science starts in the seventeenth century is important to Zuckerman and Merton's approach. They can trace refereeing back to its origins because the institutions of science are part of a structure that carries through many periods and cultures, changing but retaining its identifying characteristics. This structure comes into being when it becomes institutionalized. For instance, they argue that "these institutions provided the structure of authority which transformed the mere *printing* of scientific work into its *publication*" (p. 402). These institutions are analyzed in terms of their systematic functions. "As with the analysis of any case of institutionalization, we must consider how arrangements for achieving the prime goals—the improvement and diffusion of scientific knowledge—operated to induce or to reinforce motivations for contributing to the goals and to enlist those motivations for the performance of newly developing social roles" (p. 464). For instance, the desires of individual researchers for recognition and for protection of their intellectual property rights, and the desires of individual readers for the sharing and prior assessment of knowledge, were both served

by an evolving system of refereeing, and this system served the accumulation of certified knowledge that is Merton's science.

After this historical vignette, Zuckerman and Merton go on to a series of studies more typical of other Mertonian articles—highly refined statistical analyses that relate patterns in some body of data to the functioning of some institution of science. So, for instance, they compare rejection rates of journals in various academic fields, find a pattern that the rejection rates are, in general, far higher in the humanities and social sciences than in the natural sciences, and relate this pattern to the degree of institutionalization of these fields. "This suggests that these fields of learning [in which many manuscripts fail to meet minimum standards] are not greatly institutionalized in the reasonably precise sense that editors and referees on the one side and would-be contributors on the other almost always share norms of what constitutes adequate scholarship" (p. 472). So this statistical pattern can lead to a demarcation of science from nonscience. The implication, as in the historical study, is that the institution is functional; the shared norms keep scientists from wasting their time on studies that will not be rewarded and will not further the accumulation of knowledge.

The rest of Zuckerman and Merton's article focuses on data from the *Physical Review* concerning the acceptance or rejection of articles, in relation to the status of the authors and of the reviewers, with this status assigned in terms of a three-tier hierarchy based on professional awards. So, again, the problem is to relate observable data to the functioning of a crucial scientific institution. For instance, they show that there was no tendency to give the manuscripts of high-ranking physicist authors to high-ranking physicist reviewers, nor were there other statistical patterns of bias in assignment. From this they suggest, albeit tentatively, "that expertise and competence were the principal criteria adopted in matching papers and referees" (p. 485). Similarly, they show that high-ranking reviewers did not reject low-ranking authors or favor high-ranking authors more than other reviewers did, nor were there any other patterns indicating bias in evaluation. "This suggests that referees were applying much the same standards to papers, whatever their source" (p. 491). The article concludes with a section discussing how the evaluation procedure allows science to progress by assuring that "much of the time scientists can build upon the work of others with a degree of warranted confidence. It is in this sense that the structure of authority in science, in which the referee system occupies a central place, provides an institutional

basis for the comparative reliability and cumulation of knowledge" (p. 495).

Since I offer this article as an example of what I shall *not* be doing with texts, I should in fairness acknowledge its relative strengths. In its careful relation of statistics to institutional structures, and institutional structures to functions, Zuckerman's and Merton's article responds to sociological criteria for explanation, and it clearly produces information, and certainty about that information, that my own study of the referee system in chapter 3, with just two cases, could not. The complexity and subtlety of the Mertonian system is apparent in the description of a tension between the hierarchy that defines the structure of the scientific community and the norms that define its ethos. The norms of science, the most controversial part of Merton's model, require that scientists apply universal standards, share their knowledge, remain disinterested, and approach claims with organized skepticism. There is a tension between these norms and the hierarchy in which some scientists are regarded as better, as more worthy of being listened to and believed, than others. The review procedure, as Zuckerman and Merton describe it, offers an institutionalized means of reconciling this tension, so that the existence of a hierarchy does not distort the normative behavior on which progress depends. As Zuckerman and Merton point out in a note, their view is rather more subtle than either the view that the scientists make judgments based solely on scientific criteria, or the view that scientists make judgments based on their status in the system.

But for all this subtlety and informative power, the approach of this article is remarkably unhelpful for the question that I have set out to answer: how do texts construct scientific authority? First, Zuckerman and Merton do not look at any texts. This is partly because they can produce more convincing claims with a large-scale study, covering hundreds of articles, that precludes analysis of individual items. But I would argue that their approach would not, in any case, lead one to texts. When they suggest an experimental design that might control for "papers of the same scientific quality" (p. 486), they seem to have in mind texts as simple entities that can be assigned a value. When they sum up a broad corpus of articles on all areas of physics, articles written in a variety of contexts, they seem to assume that the content of the articles does not matter. When they categorize responses of referees simply in terms of rejections, acceptances, and the category of "problematic manuscripts" for those that may be accepted if revised, they assume that what the referees' comments say and how

they say it, apart from their decisions, do not matter. It is not that I would require Zuckerman and Merton to do the study I have done—I'm glad they did not—but that I don't see their approach as following from the same assumptions as mine, even when we are looking at the same sort of materials. Actual texts and specific disciplinary knowledge are, in their approach, the noise that needs to be filtered out for the signal to come through clearly.

Also, Zuckerman and Merton make it very difficult to pose questions about scientific authority. Science is to be explained, not by analysis of the work of scientists, but by showing how scientific institutions produce science. The assumption is that there is such a thing as objective scientific knowledge, the accumulation of which may be furthered or hindered by various societies—furthered by seventeenth-century England, or hindered by Nazi Germany—but the propositional content of which is independent of any society. In the study of article refereeing, if they can show there is no statistical evidence of bias in decisions, then those decisions must be based on purely scientific "expertise and competence" or "standards." But the dichotomy of bias and objectivity makes it impossible to look at a social process in science that is neither an objective encounter with natural fact nor a dishonest departure from the fact. As the other sociologists I shall discuss show, the social *construction* of a scientific fact doesn't fit either of these categories. Remarkably often, other Mertonian studies, like Jonathan Cole and Stephen Cole's massive study of the grant review procedure, conclude that scientific institutions are most likely to produce scientific knowledge. But what if scientific knowledge is merely that which is produced by scientific institutions? This circular framework of argument grants the legitimacy and independence of scientific authority at the outset, when it is just the production of that authority that I want to investigate.

The Zuckerman and Merton or Cole and Cole studies constitute a polemic in favor of the unhindered functioning of scientific institutions. My disagreement with this polemic is political as well as methodological. In Merton's work in the early 1940s the need for the unhindered progress of science was one argument for democracy.<sup>11</sup> But it can be argued that today the unquestioned authority of science can itself be a danger to democracy. In the terms which Merton uses—bias or objectivity—science always remains unquestioned because it corrects its own errors. I would argue that closer study, although it will not show bias in the referee system, shows that the sense of the

11. See his influential 1942 essay, "Science and Technology in a Democratic Social Order," collected in *The Sociology of Science*.

objectivity of science is part of what science produces in scientific texts. So the objectivity of science is not an argument for giving scientific experts special authority in the political process.

Some of these limitations of the Mertonian paradigm are overcome in the recent work of Charles Bazerman, who takes the paradigm as the basis for his studies of scientific texts. In his 1984 article "Modern Evolution of the Experimental Report in Physics" (reprinted in *Shaping Written Knowledge*), Bazerman, like Merton, provides a broad-based study of changing scientific institutions. He selects representative articles from the *Physical Review*, now the most important journal in physics, focusing on those in the field of spectroscopy. But Bazerman approaches these articles in two ways: "using [stylistic] statistics to indicate gross patterns or trends but using close analytical reading to explore the finer texture, the meaning and the implications of those trends. The statistics indicate that something is happening, and the close readings are to find out what that something is" (p. 167). In the statistical portion of the paper, Bazerman relates the increasing length and the increasing number of references to increasing knowledge and the tighter links between work done in the field. He asserts that changes in clause structure and word choice reflect changes in argument. The changes in graphics and structure of articles also reflect theoretical integration. Bazerman discusses a chronological sequence of selected articles "to suggest a rhetorical history of the field" (p. 184). These close readings show in detail the growing awareness of the theoretical framework behind any experimental report.

I would argue that Bazerman's article goes far beyond the approach of Zuckerman and Merton. First, he is concerned with texts, which, as I have noted, are noticeably missing from Zuckerman and Merton. His analysis of citation data benefits from the increasingly close analysis of context and content of citations by Daryl Chubin and others.<sup>12</sup> Also, he limits his corpus to one field, so that he can make comments about the content of articles. For instance, he says of one early article. "The data presented are not selective concerning an issue at hand, but rather seem presented for their own sake" (p. 185), something he would not see if he had no awareness of the context of argument in the field. One may criticize details of Bazerman's article; for instance, the grammatical categories he uses are rather crude for the highly specific interpretations he wants to make of the results. But he tries to persuade textual scholars to look at social systems, and he tries to

12. See, for example, John Swales, "Citation Analysis and Discourse Analysis," *Applied Linguistics* 7 (1986): 39-56.

persuade sociologists to look at texts.<sup>13</sup> "As much as any of the other institutional arrangements of science, writing conventions are significant social facts for the communal operation of science" (p. 191). Bazerman concludes this study by observing that "The large-scale trends revealed here are consistent with the traditional view that science is a rational, cumulative, corporate enterprise, but point out that this enterprise is realized only through linguistic, rhetorical, and social means and choices, all with epistemological consequences" (p. 191). I do not think his work—or mine—need necessarily support this traditional view of science.

For my questions about the social authority of science, I have found the most useful guides in some of the studies that have led away from the Mertonian approach toward a model based on indifference to claims of truth, on the importance of the socially contingent, and on conflict. Barry Barnes named this approach the "Strong Programme" in the sociology of knowledge because it would find a social basis for all knowledge, not just for certain irrational beliefs. The key issue on which researchers in the Strong Programme have broken with the Mertonian tradition is the assumption that the content of natural knowledge can be separated from the social processes that produce it. Merton shows how institutions function to further the accumulation of knowledge about the natural world. The researchers in the Strong Programme would argue that the knowledge itself can be seen as social; they distinguish their field from that of the Mertonians by calling it the sociology of scientific *knowledge*. One way these sociologists bring science into the realm of sociology of knowledge is by arguing that explanations of beliefs should be symmetrical; that is, one should not distinguish between "correct" and "incorrect" beliefs in making explanations. One should use the same modes of explanation for belief in witchcraft or phrenology as for belief in electromagnetic waves or neuroendocrinology. The particular explanations behind these beliefs may, of course, be different, but one can't say, in this approach, that the nineteenth-century public believed in phrenology for cultural rea-

13. Bazerman's studies have been useful to such applied linguists as John Swales and Tony Dudley-Evans, who are trying to put linguistic research on scientific texts, and the teaching of English for Special Purposes, back in a social context. (For Swales, see n.12; Dudley-Evans has edited "Genre Analysis and ESP," *ELR Journal* 1 [1987], available from the English Language Research Unit, University of Birmingham.) Bazerman sees his work as leading to practical knowledge for writing teachers, and as he has produced these specialized sociological studies he has also produced an influential textbook for university writing courses, *The Informed Writer* (Boston: Houghton Mifflin, 1981; 1985; 1989).

sons, whereas we believe in neuroendocrinology because it is true. Many of these researchers critical of Merton trace their inspiration to Thomas Kuhn. But there have been a number of other strands entwined with this approach: the sociology of knowledge from Karl Mannheim, ethnographic methods from anthropologists studying nonwestern cultures, and conversation analysis from Harvey Sacks and others. Rather than trace all these strands I shall contrast two studies with Merton's.<sup>14</sup> Here I am not so much concerned with the specific findings of these studies as with their general method. They represent two ways of keeping scientific knowledge from seeming natural and self-evident: by focusing on controversies and contexts, and by taking apart the processes by which facts are constructed.

Steven Shapin's "Pump and Circumstance: Robert Boyle's Literary Technology" changes our view of a scientific fact by looking at its production in a political, social, and textual context. Shapin argues that Boyle and his colleagues, attempting to promote the experimental approach to natural science in a seventeenth-century England in which this approach was still controversial, designed their program to produce "indisputable matters of fact." They were produced through a material technology, such as the expensive and delicate air pump, a literary technology, in the form of reports that would make it seem as if the reader had witnessed a demonstration, and a social technology that governed debate such that matters of fact would be decisive.

So far Shapin seems similar to Merton, in his focus on the seventeenth century as crucial for modern science, his interest in the social validation of knowledge, and his interest in the rise of publication along with the rise of science. The key difference is that Shapin argues that we must look to the social and political struggles of the seventeenth century, not only for the origins of social institutions like the Royal Society, but also for the origins of matters of fact. He describes, not a unitary, functional science that progresses smoothly to the science of the present, but a variety of fundamentally different sciences competing with each other, varieties we see now through the assumptions of modern textbook science.

The foundations of knowledge were not matters merely for philosophers' reflections; they had to be constructed and the propriety of their foundational status had to be argued. The difficulties that many historians evidently have in recognizing this work of construc-

14. See Steve Woolgar, *Science: The Very Idea* (London: Tavistock, 1988) for a brief summary and critique of various strands.

tion arise from the very success of that work. To a very large extent, we live in the conventional world of knowledge production that Boyle and his colleagues amongst the experimental philosophers laboured to make safe, self-evident, and solid. (P. 482)

In Shapin's study (and in his larger-scale work with Simon Schaffer, *Leviathan and the Air-Pump*) the alternative science is that of Hobbes, who objected to the experimental method and held out for a more logical basis of scientific thought; in other studies, the other side of the controversy might be proponents of "charm" in high-energy physics, the geological catastrophists, or phrenologists, or J. Barkla, a twentieth-century physicist whose discovery of a "J phenomenon" was not accepted by the rest of the scientific community.<sup>15</sup> To follow a study like Shapin's, we have to suspend our certainty that Boyle won because he was, after all, right about the properties of air.

Each of Merton's norms of the ethos of science is shifted by Shapin, from the terms of a functioning system to terms of a contest between opposed forces. Merton's Universalism corresponds to Boyle's attempts to find a language in which his science—based on "matters of fact"—would gain support by being presented as a neutral ground between bitterly divided sects. Where Merton talks about Communism, in terms of channels for the sharing and evaluation of given information, Shapin talks about texts, focusing on the medium and its powers. So, for instance, he looks at Boyle's "prolixity" as a technique for creating a sense of realism, a sense that the reader is a "virtual witness." Merton considers Disinterestedness as a norm of the scientific community, whereas the parallel category in Shapin's analysis, "modesty," is another rhetorical device. For Merton, "Organized Skepticism" can be explained as another functional norm, whereas Shapin treats the apparent skepticism of Boyle's separation of the language of fact and the language of interpretation as a strategic device for the defense of his epistemology.

From Shapin's viewpoint, the norms of science tell us about its

15. Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Edinburgh: Edinburgh University Press, 1984); Steven Yearley, "Representing Geology: Textual Structures in the Pedagogical Presentation of Science," in *Expository Science: Forms and Functions of Popularization*, ed. Terry Shinn and Richard Whitley (Dordrecht: D. Reidel, 1985), pp. 79–101; Steven Shapin, "The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes," and Brian Wynne, "Between Orthodoxy and Oblivion: The Normalization of Deviance in Science," both in *On the Margins of Science: The Social Construction of Rejected Knowledge*, *Sociological Review Monograph* 27, ed. Roy Wallis (Keele: University of Keele, 1979).

rhetoric, not about its ethos; to accept them as functional is to promote an ahistorical view of science and to accept uncritically science's view of itself. Merton's and Shapin's approaches may seem complementary, one starting at the level of the whole—institutions—and looking at the parts—the individuals—the other starting from the parts and working up to the whole. But for Shapin, the acceptance of science's view of itself keeps one from seeing how it works on any level—one not only has to untangle complexity, one has to penetrate disguises. "Just as the three technologies operate to create the illusion that matters of fact are not man-made, so the institutionalized and conventional status of the scientific discourse that Boyle helped to produce makes the illusion that scientists' speech about the natural world is simply a reflection of that reality" (p. 510). So the relation of Shapin to the texts he studies is radically different from that of Merton; Shapin wants to see more than seems to be there, to see through the text to another set of meanings. For Shapin, as for many analysts of culture, the text conceals its origins.

Even traditional historians who would not accept the "Strong Programme" have long been concerned with the political and social background of science. It is harder to see this background in contemporary science, where the world of scientific facts may seem divorced from the world of political and social struggle. However many meticulous historical case studies make the connection, it will always be possible to say that science is different now. The sociologist Harry Collins comments on this apparent lack of evidence for the social construction of contemporary science. "It may be that scientific institutions have become more autonomous, so that the social network between science and the wider society is now sparse. I think it far more likely that it is a matter of not being able to 'see the wood for the trees' in very recent scientific history" (p. 153). An influential article by Collins, "The Seven Sexes," shows how a study of a current controversy while it is still unresolved can lead to issues relevant to textual study (even though, as we shall see, Collins excludes written texts from his study).<sup>16</sup>

Collins describes the problem of what Shapin calls the "self-evidence" of scientific fact using the figure of a ship in a bottle:

It is as though epistemologists were concerned with the characteristics of ships (knowledge) in bottles (validity) while living in a world where all ships are already in bottles with the glue dried and the

16. Harry Collins, "The Seven Sexes: A Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics," *Sociology* 9 (1975): 205–24.

strings cut. A ship within a bottle is a natural object in this world, and because there is no way to reverse the process, it is not easy to accept that the ship was ever just a bundle of sticks. (P. 94)

Although Collins agrees with Shapin that the analyst needs to cut through these assumptions, they have quite different methods, in these studies, of taking apart the ship in the bottle. Shapin and other historians are able to connect scientific controversies to their social context by studying controversies sufficiently far in the past for their social context to assume definite outlines for us. The terms of the historical background—class, political struggle, educational and religious institutions—are dealt with in familiar social historical terms, and in that sense can be taken for granted. Collins, on the other hand, sees an advantage in focusing on contemporary controversies. Even though tracing of social interests in conventional sociological terms is not as easy in such cases, the analyst has the advantage that the process of making a self-evident fact is not yet complete. “It is actually possible to locate this process in scientific laboratories, in letters, conferences, and conversations. It is possible then to perform a kind of automatic phenomenological bracketing for ideas and facts, by looking at them while they are being formed, before they have become ‘set’ as part of anyone’s (scientific) world” (p. 95).

Collins privileges the conversations, letters, and interview comments of scientists—what he calls the contingent forum—over the published articles—part of what he calls the constitutive forum—because the informal texts can show what goes on before discourse is fitted into the formalities of research articles. His style of interviewing is an important part of this strategy. Sociologists usually find out about the social side of science by asking about it directly; their interviews or questionnaires are designed to get reliable data about institutional and interpersonal issues. But when one focuses attention on it in this way one gets certain familiar and stereotypical answers. Collins approaches the social obliquely; his interviews “were built around detailed *technical* discussion of the experiment and scientists’ interactions rather than straightforward sociometric questioning” (p. 98). So his approach is exactly the opposite of that of Mertonian interviews—he finds the social through the technical instead of extrapolating from the social to the function of norms and social structures in the technical realm.

Collins’ study has implications for textual analysts even though it avoids written texts. He compiles lists of interview comments to show the varying sources of attitudes toward an experiment and the use of “other than formal methods of argument and persuasion” (p. 106).

These lists would lead us to look in the rhetoric of (informal, contingent) scientific discourse for the full range of rhetorical appeals used in other discourses, and not just for the logic and evidence on which scientific arguments are supposed to be based. Collins is interested in these rhetorical devices in relation to the problem of replication, arguing that what scientists are engaged in is “negotiations about the meaning of a competent experiment in the field” (p. 107). He sees a circular rhetoric in any field of science that is making fundamental discoveries: only a competent experiment will show the true nature of the phenomenon, but the competence of the experiment is judged on whether it shows the true nature of the phenomenon. Thus the argument could go on forever, but as Collins shows in a subsequent article, in practical terms closure is brought by social means.<sup>17</sup> Somebody doesn’t get their articles cited, or their grant renewed, or their discovery in the textbooks.

### ***Scientific Texts: Discourse Analysis and Its Critics***

Shapin’s and Collins’ approaches seek to take us behind the text; in some ways this approach is much like literary study when it focuses on manuscripts, letters, and biographical background as showing the process underlying the published work. I need, for the questions I am asking, the example of someone who looks at the surface of the text. A key work in this shift of focus is Bruno Latour and Steve Woolgar’s ethnographic study of biochemical research, *Laboratory Life*. Latour and Woolgar see the whole work of science in terms of the production of inscriptions: machines tracing graphs, researchers making notes, articles lying on a desk. Latour has continued this approach in his work on Pasteur and has outlined it for a general audience in *Science in Action*.<sup>18</sup>

17. Harry Collins, “Son of the Seven Sexes: The Social Destruction of a Physical Phenomenon,” *Social Studies of Science* 11 (1981): 33–62.

18. Bruno Latour, “Give Me a Laboratory and I Will Raise the World,” in *Science Observed: Perspectives on the Social Study of Science*, eds. Karin D. Knorr-Cetina and Michael Mulkay (London: Sage, 1983); *Les Microbes: Guerre et Paix* (Paris: Éditions A. Métailie, 1984).

A nonsociologist interested in sociological approaches to scientific texts might start with Bruno Latour, *Science in Action*; Gilbert and Mulkay, *Opening Pandora’s Box*; Charles Bazerman, *Shaping Written Knowledge* (Madison: University of Wisconsin Press, 1988); Bruno Latour and Steve Woolgar, *Laboratory Life* (Beverly Hills and London: Sage, 1979); Michael Lynch, *Art and Artifact in Laboratory Science*; and K. Knorr-Cetina, *The Manufacture of Knowledge* (Oxford: Pergamon, 1981).

The break with both Shapin and Collins implied in this approach through texts can be seen in Woolgar's article, "Discovery: Logic and Sequence in a Scientific Text."<sup>19</sup> Woolgar's "central assumption" is the "isomorphism between presentational context and scientific concepts" (p. 239). That is, a text on a phenomenon takes on the same shape as that phenomenon, or rather, the phenomenon takes shape through the text. So the text is neither the empty tube that carries the scientific facts (as it is for Merton), nor is it the formal surface that conceals the real business of science (as it is for Collins); it is the crucial document that allows Woolgar to recover "the structure of the conceptual model which is made use of in recognizing that [a discovery] is what it [the phenomenon] is" (p. 251). The search for the real processes behind the apparent processes of science will yield just another text, not a truer one. Woolgar, like some literary critics, insists that one cannot get to something beyond representation. But such an insistence may be more surprising to Woolgar's sociological readers than it would be to many literary critics. When he says in a note, "I have no interest in the 'accuracy' of the data," he is tossing out most of what sociologists do, but he is keeping the privileges of a literary critic. What he seems to mean is that he is not interested in whether the speaker's account of discovery accords with some hypothetical objective view of events, but is interested, rather, in how the events are defined, and are constantly redefined in further texts, as a discovery.

Woolgar makes it clear in his heavily ironic introduction that this approach contrasts both with that of "the rationalists," like Merton, and "the Strong Programmers," like Shapin. Woolgar notes that both sides, in their discussions of the nature of proof and evidence, refer to documents for their evidence: "To the extent that we are constrained in our use of available language resources, we will inevitably reproduce the language of realism" (p. 242). It is that rhetoric Woolgar seeks to analyze. He argues that scientists themselves, in controversies, use both rationalist and Strong Programme arguments. He steps back from the sociological controversy about scientific knowledge to study the textual strategies used in all controversies to create a sense of reality. "The analysts' task is not to resolve such disputes, but rather to develop an appreciation of their form and currency" (p. 243).

Woolgar presents his study as an extension of Collins's, but it can

19. Steve Woolgar, "Discovery: Logic and Sequence in a Scientific Text," in *The Social Process of Scientific Investigation*, *Sociology of the Sciences*, Volume 4, ed. K. Knorr, R. Krohn, and R. Whitley (Dordrecht: D. Reidel, 1980).

also be read as a reversal of the direction of interpretation. Collins speaks of negotiating the character of the phenomenon, whereas Woolgar notes that "the out-there-ness of a phenomenon is accomplished in establishing its properties" (p. 245). This seemingly innocent turn means that instead of using texts to show the socially contingent nature of scientific phenomena, as Collins does, Woolgar assumes the social contingency of research, including his own, and uses this assumption as a starting point in analysis of texts. Woolgar uses the term "documents" in its special ethnomethodological sense, meaning all sorts of outward and visible signs from which the interpreter must begin.<sup>20</sup> But the document he analyzes here is a document in the familiar sense of the word as well; he says he has "arbitrarily chosen" to write about writing, when he could just as well find practical reasoning in "the actions, conversations, seminar discussions, conference presentations, inscriptions, recordings, and writings of scientific work" (p. 245). But as we shall see in contrasting him with another ethnomethodological approach, his choice of published writing as the scene of such practical reasoning is significant; he challenges the implied hierarchy in which writing, and especially a published text, is secondary to informal speech and practical actions. His choice of a text, the Nobel address of an astrophysicist, would seem to be both secondary and unrepresentative; secondary because it comes long after the events it describes and their announcement, and unrepresentative because not many astrophysicists have to produce Nobel addresses. But his point is that the events have to be redefined as a discovery in each new text, so that a late text does work just as the first publication did. And if the specific occasion is an unusual one, it is not unusual for a scientist to have an occasion to present a scientific claim in terms of a narrative of his or her career.

Woolgar focuses on four regular patterns in the text that he relates to accomplishing the "out-there-ness" of the discovery. "Preliminary instructions," such as the title, the identification of the author, and the occasion, assert that the text is *about* something, and direct us to consider it in only a certain context. "Externalizing devices," including the quasi-passive voice and the invocation of community membership, seem to deemphasize the author's role in the discovery, so that the discovery is seen in terms of a path of coincidences. "Pathing devices" are ways of "portraying work as the latest in a long line of

20. The classic source for ethnomethodology is Harold Garfinkel, *Studies in Ethnomethodology* (Englewood Cliffs, N.J.: Prentice-Hall, 1967); see John Heritage, *Garfinkel and Ethnomethodology* (Cambridge: Polity Press, 1984), for an introduction.

development" (p. 256), shaping history retrospectively. And "sequencing devices" are forms that "act as a cutting out process, whereby other potential paths and potentially relevant events are backgrounded" (p. 258). Woolgar's conclusion is that such narrative devices that create a sense of "the nextness" of events produce the effect of logic in scientific texts, so as to make it difficult to imagine alternate interpretations. Woolgar, like a good literary critic, gives striking examples from the text to illustrate each of these devices. But for all his close reading, his devices are a linguistic grab bag, hard to define in terms of signals in the text. Instead, one has to start with the sociological problem, and then look for the bits of text that relate to it. This does not undermine his approach as sociology, but it makes it difficult to use in other disciplines, such as linguistics, which require that generalizations be tied to formal features in the text.

At the end of his essay, Woolgar suggests that "the perspective here might be profitably developed and extended to an examination of a much wider range of scientists' accounting practices" (p. 263). This was done in a series of articles by Nigel Gilbert and Michael Mulkay, who, like Collins and Woolgar, based their conclusions on a detailed study of one research program, in this case a biochemical controversy concerning the mechanism for transfers through cell membranes. Like Woolgar, they were interested in the ways scientists define their world in their accounts of it. Like Woolgar, they looked at patterns of the accounts themselves, instead of using these accounts directly as evidence for the correctness of one or another assertion about the social structure of science.<sup>21</sup>

Gilbert and Mulkay's major device is the categorization of scientists' discourse into an "empiricist repertoire" stressing impersonality and experimental results, and a "contingent repertoire," which acknowledges social factors. These are like the positions of Mertonians and of Strong Programmers as Woolgar lays them out at the beginning of his article. Gilbert and Mulkay argue, as Woolgar does, that scientists can shift strategically between these two repertoires. Thus the repertoires coexist in the same texts, instead of being found in separate places like Collins' contingent and constitutive forums. Gilbert and Mulkay show such shifts by contrasts of various texts. It is the comparisons, rather than any specific linguistic features, that make the devices apparent. For instance, in one chapter which I shall cite frequently, "Accounting for Error," they show how scientists

21. The essays are collected in Nigel Gilbert and Michael Mulkay, *Opening Pandora's Box*.

regularly use the empiricist repertoire to describe their own successes, while using the contingent repertoire to explain why competing researchers are wrong. In another chapter, they describe a rhetorical device that allows for contingency in research while still preserving the empiricism of science, the "Truth Will Out Device," holding that facts will triumph over social factors in the long run. They also analyze attributions of consensus, illustrations, and even jokes using these categories.

Though my own approach follows that of Gilbert and Mulkay in many ways, I now find their analysis into two repertoires a cumbersome analytical tool. It does not help in studying the generation or reception of the text. The problem that bothers me is not the problem that bothers more empiricist sociologists of science like Harry Collins, the turning of irony upon irony that characterizes this deconstructive approach.<sup>22</sup> Rather, my main problem is that the two categories seem to owe their existence to a polemic against the idea that anything lies beyond the text. Even if one is persuaded by this polemic, it does not necessarily take one much further in textual analysis. As Woolgar shows clearly, he and Gilbert and Mulkay turn a sociological controversy into an analytical tool. This leads them to striking insights but means they are tied to the terms of that controversy. Like Woolgar's contrast between "rationalist" and "Strong Programmer," the two repertoires seem to lead only to the selection of some features that parallel these two lines of interpretation. And Gilbert and Mulkay's interpretations seem to be limited to showing that there is interpretive variability; beyond making this sociological point, they say little about the processes of writing and reading.

There are two lines of criticism of discourse analysis in the sociology

22. For further discussions of the differences of approach between Discourse Analysis as practiced by Gilbert and Mulkay and their colleagues and the Empirical Program of Relativism as practiced by Harry Collins, see Michael Mulkay, Jonathan Potter, and Steven Yearley, "Why An Analysis of Discourse Is Needed," in Knorr-Cetina and Mulkay, eds., *Science Observed*. Collins' response, "An Empirical Relativist Programme in the Sociology of Scientific Knowledge," is in the same volume. Mulkay plays wittily on the debate in *The Word and the World* (London: Allen and Unwin, 1985), and Malcolm Ashmore critiques it in his playful essay, "The Life and Opinions of a Replication Claim: Reflexivity and Symmetry in the Sociology of Scientific Knowledge," in Steve Woolgar, ed., *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge* (Beverly Hills and London: Sage, 1988), pp. 125–54. Another critique of Gilbert's and Mulkay's approach to science studies is the review by Peter Halfpenny of *Opening Pandora's Box* and *The Word and the World*, "Talking of Writing, Writing of Writing: Some Reflections on Gilbert and Mulkay's Discourse Analysis," *Social Studies of Science* 18 (1988): 169–82.

of scientific knowledge that I will keep in mind without really answering: one comes from the discourse analysts themselves, and another from ethnomethodology. Discourse analysts take an ironic stance toward scientific texts, but they also take an ironic stance toward texts in the sociology of science, toward all the devices of interpreters establishing an extratextual reality on which to base their arguments. And this irony applies to the sociological analyst's own text, including the present one; Woolgar has continued to investigate the implications of turning one's methodological tools back on oneself (see *Science: The Very Idea*), coming finally to a critique of the notion of the self. In these investigations he uses the ethnomethodological term "reflexivity," but literary critics unfamiliar with sociological jargon will still recognize the move. The consistent development of the discourse analyst's ironic stance is in Mulkay's work *The Word and the World*, in which he plays with his own text's attempts to establish authority by writing parodies and dialogues. For instance, "The Scientist Talks Back" is a one-act play that draws on the writings of Zuckerman, Collins, and Karin Knorr-Cetina, along with comments of scientists made in interviews, to present a fictional dialogue about replication in science and in sociology.<sup>23</sup>

Just as sociology of science has its deconstructivists like Woolgar and Mulkay, it has its phenomenologists—the ethnomethodologists. Michael Lynch, who was trained in ethnomethodology, has produced a series of studies that question all the lines of work that I have surveyed so far. One article, "Technical Work and Critical Inquiry" (1982), summarizes his critique and provides a model of an alternative approach.<sup>24</sup> Essentially, Lynch wants to study the way scientists themselves make sense of their world in their daily work. So he dismisses at the outset those sociological studies like Merton's that do not involve "the technical work of science." But he sees a flaw in the approaches that try to show the social contingency of scientific knowledge, arguing that they just impose sociological categories on scientific work. Like Woolgar, he says that the Mertonians and the Strong Programmers ignore the social analysis the scientists themselves do in their daily work. They assume some place to stand ironically outside science to see through its pretensions. But what validates the methods of sociology?

23. Woolgar collects some essays developing, exemplifying, and criticizing reflexivity and new forms in *Knowledge and Reflexivity*; see especially the contributions by Malcolm Ashmore, Anna Wynne, and Bruno Latour.

24. Michael Lynch, "Technical Work and Critical Inquiry: Investigations in a Scientific Laboratory," *Social Studies of Science* 12 (1982): 499–533.

Lynch proceeds through a critique of the various devices used to reveal social contingency, presenting all these devices as ways of getting around the fact that sociologists are not themselves doing technical work. "Critical historiography," like Shapin's, substitutes "disengaged overview" for "technical access." "In this way the 'particulars' of the technician's work are disregarded, except in so far as they supply the documentary materials for an historian's operation of showing how the reputedly objective concerns of the technician are the legacy of a contentious (and in some ways capricious) social history" (p. 505). When the historian's perspective is substituted for that of the technician, the self-evident quality of scientific objects is broken open, but in the process everything gets lost except what can be used to show social contingency.

Lynch's treatment of the sort of rhetorical analysis I am doing in this book is brief but damning.

On setting up these operations, the identification of science within the traditional distinctions between logic and rhetoric, truth and fiction, and fact and construction is *inverted* for methodological purposes. . . . Rhetorical analysis of the ostensibly 'non-rhetorical' relies upon an argument which places 'objectivity' in quotes (or brackets) and subsumes [it] to the omnirelevance of rhetoric. (p. 505)

Again, the critical analyst is accused of privileging his own categories, here simply the ironic reversal of just those categories put forward by scientists. Lynch asks analysts of the rhetoric of science, "Rhetoric as opposed to what?" (p. 527n). The claim that scientists are using rhetoric is only interesting as an ironic debunking of the assumption that their discourse is especially "objective." Once one grants that this objectivity is something they create in their work, the claim that everything is rhetoric has little meaning.

Finally Lynch criticizes the device of "the stranger" used by Latour and Woolgar, and by many studies (like mine) that must view science from the outside to find a chink in the armor of its apparent self-evidence. Lynch questions the sort of reflexive turn that reduces all of science to inscriptions and then sees scientists as doing the same sort of literary interpretive work as the anthropological observer. This claim for the sufficiency of nonscientific methods, he says, is disproved by the Latour and Woolgar "stranger's" in incompetence as a lab technician. There is, then, some crucial technical skill that the sociological observer does not have.

In Lynch's view, all these analysts of science are guilty of trying to impose their own rather simple sociological categories on the complex processes of scientific work. Instead of imposing social science methods on natural science inquiry, they should become aware of the processes through which scientists create their world. "Scientists speak and act in each others' competent presence in ways that exhibit an untold-of richness and specificity to questions on the constructive horizons of specific objects under investigation" (p. 511). But in order to get at this richness, one must try to become competent at what the scientists do, and one must be at the same place and the same time as the scientific work being done, in the presence of the objects under discussion; in ethnomethodological terms, the inquiry must be situated. That is, one must enter into the world of the lab, and recognize one's own limitations as a nonscientist.

To point to this disciplinary limitation on what can count as a competent observational vantage is not to propose closure on the problem of multiple interpretations, but instead to insist that any interpretation, evaluation, or argument must, first of all, contend with what scientific practitioners produce and recognize as a competent interpretation, evaluation, or argument in *their* local setting of inquiry. (p. 54)

Lynch gives an example of how scientists negotiate the character of an object.<sup>25</sup> In this case, a lab director and a technician are discussing an electron micrograph montage and determining whether it was done incorrectly and whether it is still usable. For textual analysts, the key point is that this discussion only makes sense when considered in the presence of the montage. The processes going on cannot be reconstructed later, just from written texts. In Lynch's view, such a situated analysis avoids the imposition of sociological frameworks but still shows the socially negotiated nature of reality. "The work, then, not only consists of a 'progress' through a socio-historical terrain; its *progression* from within involves an articulation, for all practical purposes, of what that socio-historical terrain consists of as immediately pertinent details" (p. 519).

The implication is that a study that starts with a ready map of the social landscape—in terms of class interests, or theories of paradigm shifts, or progressive research programs, or the unmasking of

25. The example is discussed within a more detailed theoretical framework in *Art and Artifact*.

rhetoric—does not allow the work to articulate its own map. Like Merton and Woolgar, Lynch sees science as social, but he sees the relevant items of the social framework as emerging in the situated work of scientists, rather than in the work of sociologists or historians fitting science into their categories. He sees ethnomethodological “documents” as the basis of analysis, but criticizes approaches that depend entirely on written and published accounts, which cannot yield analyzable material on scientific practices in action.

Lynch also differs from Woolgar in the relation he tells the interpreter to take toward the text. In the view of the ethnomethodologists, the interpreter must drop the ironic stance and enter into the technical work of science undefended to see how natural objects are interpreted. This critique of irony is the most valuable part of Lynch’s approach for me, for I find that, far from unmasking the ideological processes of science, I have created descriptions that don’t surprise the biologists I am studying. Surely if I were unmasking some concealed pattern, they would be annoyed, or would at least disagree with what I showed. While I would have preferred to reveal what they could never have seen, I have to admit that they are quite capable of themselves undertaking the sort of analysis that I do.

I have not taken the ethnomethodological program as the basis for this study for three reasons. First, I can’t. The ethnomethodologists, in accordance with the severe demands of their methodology, set very high standards for initiation into their fraternity, so that Lynch, for instance, must try to be both a phenomenologist and an electron microscopist. To an outsider, at least, these standards seem not only high, but unattainable. Second, I’m not sure that even if I could take this approach, I would find anything interesting. Lynch’s studies so far suggest what might be done, but the findings themselves are not so interesting as the methodological preamble. Finally, I remain suspicious of ethnomethodologists for the same reason that many literary critics are suspicious of phenomenological criticism; it seems to posit an underlying level of reality in processes, a level that seems neither reachable nor necessary. In this case, one could ask whether Lynch can escape imposing social science methods like all the other approaches he criticizes. Lynch, I must say, argues strongly and consistently against any accusation that he claims to find a deeper level of reality. Still, one sometimes thinks when reading ethnomethodologists of the Emperor’s new clothes, or of a new version in which the Emperor claims to walk in front of his people completely naked, but in fact is wearing rich old robes.

I have arranged this sequence of studies so that it leads back to my

own approach. Like Bazerman, I study scientific texts as part of a social system. I am, like the Strong Programmers, involved in a project of explaining the scientific in terms of the social. Like Collins, I see this as requiring a relativist epistemology, a disorienting shift of perspective, and detailed study of technical work. Like Woolgar, Gilbert, and Mulkey, I see texts as a way of investigating these social negotiations, and I would study, not just the content of texts, but their form and processes of production. But like Lynch, I want to avoid the ironic stance of these other approaches. The project should also require some attempt to relate the conflicts involved in science to larger conflicts in society, whether the relation is one of simple congruence or is more complicated. In fact, I make these connections only occasionally; I shall return to these relations in the Conclusion.

### ***Materials and Methods***

Each of the studies in this book uses the form of some scientific texts to reveal the social processes of the construction of scientific knowledge. Though the questions I am answering and the theoretical basis for my approach are drawn from sociology of scientific knowledge, I find that my methods remain those of someone trained in literary criticism and rhetoric. Like many traditional literary historians, I am particularly interested in revisions, and in sources and analogues, as starting points for analysis. In various studies I draw on such traditional topics of literary and rhetorical study as ethos and pathos, narrative, irony, persona and characterization, subjects and tenses of sentences, and the use of quotation and echoic speech. In effect, I take each text apart, so that we can see alternative choices at various points, and then put it back together again in a way that stresses a coherent pattern in these choices, and implies a process underlying them.

The studies that follow analyze writing in various genres by five American and British biologists whose research touches in one way or another on evolutionary questions. I chose to focus on evolutionary biology simply because that was what my first two subjects had in common. But biology is a useful discipline for a study of this kind. First, it is not physics, and although many sociological and philosophical studies take physics as the exemplar of science, I think we may learn from the rather different methods of biology. Evolutionary biology is often nonexperimental, so it raises rather different questions about observation, interpretation, and persuasion than those treated in most studies of physics. The processes these researchers want to explain are entirely inaccessible by direct means (one researcher

points to cosmology as a parallel example), so there can be no naive sense for them of immediate confrontation with nature. Narrative is the point of the discipline, and the need for interpretation to make the mass of data coherent is acknowledged by the scientists themselves. I will refer in the chapter on popularizations to Ernst Mayr's remark in *The Growth of Biological Thought* that advances in biology involve conceptual shifts rather than discoveries; the same facts come to be seen from a radically different perspective. This makes rhetorical analysis of the work of evolutionary biologists particularly interesting, since the finding can be said to emerge in the text and in discussion, as events are assembled into a narrative, rather than, say, having the finding seem to emerge, its meaning self-evident, from the detector of an accelerator. Finally, evolutionary biology has long been an area of public interest and controversy, so its larger ideological context is of interest. For instance, the popularization of evolutionary ideas in sociobiology has an effect on a public debate, on social and political issues, in a way that popularization of the equally interesting issues of quantum physics does not.

In the course of this study I have talked to a number of biologists about their writing, but I have chosen to discuss only a few in this book. One thing they all have in common is that they work on some boundary between fields; this creates a tension that brings out some of the social networks involved in scientific publication. I shall introduce them all here to give a *Dramatis Personae* for the studies that follow:

*David Bloch* is an example of an experienced researcher trying to enter a new area of research relatively late in his career; his writing showed all the skills of an experienced member of the discipline, but he lacked, at least in the beginning, the network of contacts that even a newly graduated Ph.D. would have. His original field was cell biology; he studied at Wisconsin and Columbia and taught at UCLA before coming to the Botany Department of the University of Texas in 1961. Until 1980, his published articles were on cell biology, and through the 1970s his lab was supported by grants for flow cytometric studies. He also did a relatively large amount of undergraduate teaching. The origins of the "big idea" that led him to change his field of research are complex. He traced his interest in evolutionary questions to his reading of Schrodinger's *What is Life?* in graduate school, to his having to present basic concepts in introductory biology courses, to a period of free writing forced upon him by a back injury that kept him out of his lab ("I was prone to write," he said), and to a graduate seminar that allowed him to follow up some of the questions raised

during this free period. His ideas on the evolution of nucleic acids emerged before he had a chance to read the current literature, instead of arising in response to that literature; this may explain why it was so hard for him to fit these ideas back into the current questions in the field. As we shall see, he wrote a number of drafts of an article, and made a number of proposals for new funding, while "moonlighting," as he put it, with his lab funded for cell biology work. His articles began to attract some interest in major journals and a news report in *Science* in 1985, after he began a collaboration with a physicist—surely a rather unexpected linking of disciplines—that enabled them to present more elaborate statistical analyses of the data. He got some funding from a private foundation for his new line of work, and prepared several further articles. Dr. Bloch died of cancer in October 1986, after a year of treatment during which he continued to do a great deal of writing and research.

*David Crews* specializes in the reproductive physiology of reptiles, so he sees himself as working between the herpetologists who study snakes and lizards from a natural history standpoint, and the neuroendocrinologists who compare various hormonal control systems. He began his graduate training in the Institute of Animal Behavior at Rutgers, after studying to be a social worker. It was there he began his physiological study of reptiles, working under the comparative psychologist Daniel Lehrman (whose name will come up later in the controversy over sociobiology). His Ph.D. was in psychobiology, an interdisciplinary area of research combining physiology, comparative psychology, and ethology. He did postdoctoral studies at Berkeley and taught for seven years at Harvard before coming in 1981 to the University of Texas, where he is now a Professor of Zoology and Psychology. As we shall see, this need to look in two directions for his audience complicates his publication of some of his articles. He directs a laboratory with several postdoctoral students and a number of graduate and undergraduate students. He has several research grants, which is useful for my purposes because he must devote much of his writing to getting his funding renewed or finding new sources. He is author or coauthor of about ten or fifteen papers a year. Some of these are popularizations and reviews aimed at presenting his area of work to a larger audience; he remarks that his applications for funding have made him aware of the need to be able to explain and justify the goals of his research simply.

*Lawrence Gilbert* specializes in the population biology of tropical forest insects and plants, a line of research that cuts across several of the older fields of zoology and botany. After receiving his undergraduate

degree at the University of Texas in 1966, he had a Fulbright scholarship to Oxford, so that he has some links with research traditions in England, which in this area are not identical to those in the United States. He began publishing articles while he was a graduate student at Stanford in the late 1960s. He is now a Professor of Zoology at the University of Texas, where he also directs the Brackenridge Field Laboratory, a reserve in Austin. The research on which I will focus concerns the behavior of *Heliconius* butterflies and the morphology of the passion vines on which they lay their eggs; he has studied the relations of these butterflies and vines both in the jungles of Costa Rica and in a laboratory setting. As a population biologist, he is interested not just in observing individuals or describing a species, but in showing how and why populations vary in their natural setting. His work presents a particularly interesting case in popularization—a colorful subject matter combined with a highly abstract theoretical problem.

*Geoffrey Parker's* career also straddles a boundary. Like Dr. Bloch, he was moving into another field, teaching himself its methods and finding other researchers who shared his interests. His training, at the University of Bristol, was in entomology—he did field studies of the mating habits of dung flies. But these studies led him to broader problems of applying mathematical models, drawn from game theory, to the evolution of many sorts of behavior, such as fighting strategy, the competition of sperm, and the competition of parents and offspring. He commented to me: “The development of the new field involving game theoretical analysis of evolutionary problems is generally accepted as starting with [John] Maynard Smith and Price in 1973, though several people had made contributions in this direction beforehand (e.g., in investigating evolutionary problems of a frequency-dependent, game-like nature). I started modelling dung-fly mating and other more general problems before the formal ESS [Evolutionarily Stable Strategy] approach was available, and had to ‘re-jig’ in terms of the new formalism.” Dr. Parker’s work has been highly influential in this developing field, and his articles are frequently cited in the sociobiological literature. He had one year of sabbatical working with the King’s College Sociobiology Research Group in Cambridge, but has otherwise spent his career working at the University of Liverpool, where he is now Reader in Zoology.

The study of E. O. Wilson’s *Sociobiology* took a different form from the others, because I interviewed neither Wilson nor any of his critics and drew the interpretations of the text only from published comments. (I did get his comments, and those of some critics, after I had written the chapter.) Fortunately, there is a good biographical account

by Ullica Segerstrale, "Colleagues in Conflict," that clarifies some of the more baffling strands in his career.<sup>26</sup> Segerstrale stresses the shaping influences of the Society of Fellows at Harvard, where Wilson was inspired by the attempts of the entomologist W. M. Wheeler "to integrate the social and natural sciences on the basis of equilibrium theory." Along with this "scientific agenda," Segerstrale attributes to Wilson a "cognitive approach" linking scientific and moral notions, a "personal moral agenda" deriving from his upbringing as a Southern Baptist in Alabama and his "reconversion" to evolutionary thought (p. 56–57). His attempt to produce a grand synthesis in *Sociobiology* was so unusual that another sociobiologist, Robin Dunbar, suggested I study it, considering his rhetoric in its disciplinary context.

Though a British zoologist recommended the study of *Sociobiology* to me, Wilson's line of evolutionary thought differs in important ways from that of Parker and other British sociobiologists, just as it differs in important ways from the line in which Crews and other American comparative psychologists place themselves, and from the population biology of Gilbert. By lumping all these writers together as "evolutionary biologists" I do not mean to suggest that the term describes one unified program or self-defined discipline. What these researchers do share are the problems of bringing together several disciplinary perspectives, and certain basic assumptions about what sorts of evidence and forms are persuasive.

My selection of subjects and my selection of textual features could both be criticized as idiosyncratic by researchers who want more generalized knowledge about society or about texts. First there are the criticisms of traditional social scientists who identify knowledge with quantitative methods. An administrator who commented on an early research proposal, based on the work in my first two chapters, said, "What do you expect me to make of a study with an  $n$  of 2?" Perhaps my indifference to this problem is the result of my literary training. Like some of the other analysts of texts work I have described, I have based my findings on a very small sample, nothing like the thousands Merton considered necessary as a basis for conclusions. But with such case studies, what number would be large enough? These four biologists (five counting Wilson) are different enough from each other to make for some interesting comparisons. They all write a lot and are successful in their specialized fields. But they are not chosen to be representative. Rather, I turned to each of them when I first heard

26. U. Segerstrale, "Colleagues in Conflict: An 'In Vivo' Analysis of the Sociobiology Controversy," *Biology and Philosophy* 1 (1986): 53–87.

about some situation or problem he had as a writer. And they all agreed to work with me, so they must all have had some unusual interest in finding out about the ways in which scientists write. I will have to take refuge in the argument David Crews makes for his studies of atypical species of reptiles—it is the unusual cases that are likely to lead us to form hypotheses that will challenge accepted ideas. These hypotheses can then be applied to the more typical species.

The texts chosen are also, in some ways, atypical. For instance, most articles were not rejected five times, like those I study, and most proposals are not revised over such long periods, and most popular articles are not so extensively rewritten by the editors. But I shall try to show that in these rare, open conflicts one sees social processes that are also at work in other texts, but which are less easily seen when all goes smoothly. When texts seem to emerge unproblematically from the research work, the ways in which they emerge, the choices that have to be made, are not apparent, even to the writers and readers themselves.

I have chosen to study texts from several different genres—experimental reports, review articles, proposals, popularizations, and one massive and unclassifiable book. Most recent studies have focused on the experimental article (most importantly Bazerman's *Shaping Written Knowledge*). It makes sense if one is trying to demonstrate the importance of rhetoric in scientific writing to start with what is apparently the most scientific and least rhetorical form. It should not surprise anyone to hear that grant proposals are rhetorical, or that popularizations require careful consideration of audience, but some readers, particularly nonscientists, might think that research reports just communicate facts. Still, I think that the rhetorical nature of such articles should be well established by the studies I cite, and I can go on to other genres. I am more concerned with the relations between texts in several genres, with the production of knowledge as a social process that includes (in Ludwik Fleck's terminology) both the esoteric audience, the core group who will read the original report, and the exoteric, the broader community concerned with science who will read reviews, popularizations, proposals, textbooks, and news reports.

There is another line of criticism of my methodology that brings out an important feature of my approach. Linguists may find that when I analyze passages I ignore all that really interests them in the nominalizations, passive or active constructions, hedges, cohesive devices, and shifts of verb tenses. For each feature I interpret there are of course many possibly interesting features about which I say nothing

at all. What brings a feature to my attention is the difference between a text and its revised version, or between a text and a comment on it, or between two texts for different audiences. My assumption is that such comparisons will bring out features that matter to the participants, rather than just features that are expected by the analyst. I do not provide a system that covers all the data, with unambiguous features in the data relating to each of my categories.<sup>27</sup> But I have set out, not to provide a general description of scientists' discourse, but to find textual evidence for the social nature of that discourse. If my approach is useful to linguistic discourse analysis, it is not in providing any formal system, but in suggesting how certain features might figure in social negotiation.

On such a methodological basis my claims must be tentative. I would hesitate to generalize any of my more specific claims about the links between linguistic features and social processes to scientific discourse as a whole; there are too many ways in which other cases may differ. On the other hand, I have become cautious about dismissing any of my observations as peculiar to one case. This is because again and again the scientists who have read my studies have said that what I say is true enough, but that it is true only of Americans, or of the British, or of the new young researchers, or of well-established, eminent researchers, or of evolutionary biology, which is taken as less scientific than physiology or molecular biology, or of biology in general, which is taken as less scientific than physics, or of those researchers who work with this genus of lizards. These comments may all be true, but I have now come across them in so many forms that I wonder if this process of categorization is a way of protecting the core of science from the suggestion of social contingency. Is real science, the unmediated encounter of man and nature, always going to be somewhere else, in another discipline, another age, another country? One advantage of starting with cases rather than with norms is that it is not my job to look for this somewhere else, for the typical science and the typical scientific text. I can begin with the material at hand.

27. For such criteria, see John Sinclair and Malcolm Coulthard, *Towards an Analysis of Discourse: The English Used by Teachers and Pupils* (London: Oxford University Press, 1974).