

## Conclusion

### Reading Biology

Any treatment of science focusing on its discourse is likely to be seen at first, especially by nonscientists, as an attack on its cultural and political authority as the underwriter of reliable knowledge of nature. Some welcome this attack as a way of turning the tables on the pretensions of now-dominant disciplines, whereas others see it as a dangerous opening that allows other kinds of religious or political belief to take the place of objective science. For instance, several readers' comments on my chapter 6 suggested that because I would not see the rhetoric of *Sociobiology* as essentially different from that of its critics, I was ceding ground to right-wing, sexist, or racist views that have taken sociobiological texts as their scientific authority. These comments, coming as they do from those who share many of my assumptions, demand a response. In the first chapter I argued on methodological grounds for a focus on scientific texts—they can tell us things about science and about texts that we would not learn by other approaches. Here I respond to a different question, a tactical question: can this approach to science be a basis for a good political strategy for the social sciences and for the public, or does it undermine effective social action? I shall also give some idea how I would expect this tactic to work in practice by outlining some of the strategies I would like to see nonbiologists employ in reading biology.

I do not see the analysis of scientific texts as a project of debunking science. Those who have seen these studies as critical are, I think, comparing the view I present to a particular and very restricted realist and empiricist view of science that leaves no room for texts at all. For those holding the realist view, any mention of rhetoric would be debunking: grant proposals, they would argue, are awarded solely on an abstract and quantifiable measure of scientific merit, and they would point to the agency's numerical scores and funding cutoff, and the quantitative approaches of some science policy, as supporting their view. For these readers, my argument in chapter 2, that grant proposals involve a rhetoric of self-presentation and of placing oneself

in relation to the field, and that the writing of them involves and reflects a complex process of negotiation with a particular audience—the agency's panel—could be seen as an attack on scientific objectivity. If one believes that scientific articles merely carry information, and that they are either judged on the truth and significance of that information, or misjudged on the basis of personal or political bias, then my argument in chapter 3—that the form of scientific claims is negotiated in the processes of writing and refereeing—could be seen as an attack on the reliability of scientific information. If one believes that science is an unmediated encounter with nature that leaves no room for controversy, or that controversies are resolved by the discovery of facts and the application of logic, my argument in chapter 4 that controversy involves the construction of stories and the ironic reinterpretation of these stories would seem to reduce scientific argument to trivial word games.

The implications of my last two chapters, on popularizations, are somewhat different, for they deal with the texts on which non-scientists depend for their view of science. In the most restricted realist and empiricist view, scientific facts are objective statements about the world and remain everywhere exactly the same whether they are stated in the *Proceedings of the National Academy of Sciences* or in *Scientific American* or in *Time*. In a public controversy like that over sociobiology, this view of science would suggest that the public need only find out the facts, or, in a case where there is conflict, find out which of the competing disciplines adheres most closely to the methods of science prescribed by philosophers. But if, as I have suggested, different audiences get different narratives, and different narratives carry different views of the work of science, those who take a realist and empiricist view of science can only ask which narrative is correct. If one starts with the assumption that science discovers objective facts solely through empirical methods, then my attention to texts in each study here will seem to be either an attack or a distraction.

The view of science that allows one to be surprised by my findings about scientific texts is indeed a limited one, and it is not the view of science held by most scientists, at least by those I have encountered. In contrast to groups of nonscientists to whom I've spoken, scientists have neither been surprised nor felt threatened by my comments on scientific rhetoric. On the very rare occasions when the subjects of these studies have asked for a change or omission, it was always because I had left room for the implication that they or someone else was guilty of fabrication, incompetence, or bad management, or where they could be seen as criticizing or mocking other scientists.

Such implications could be seen as unscientific behavior, but my discussion of their rhetoric never seems to have been taken as an attack on them as scientists. To take one example, many people have seen the first part of chapter 6, where I discuss the construction of E. O. Wilson's *Sociobiology*, as a criticism of the book. But Wilson himself does not seem to have seen it in this way.

Although I do not see this book as a debunking project, I do see the analysis of scientific texts as a way of promoting change. Specifically, I would like nonscientists to read more science, to read it more critically, to read it with an awareness of the social processes that produce it, and to question the authority with which science is sometimes presented in cultural and political contexts. These might seem uncontroversial and humane goals. But analyses like mine have provoked fierce criticisms from other analysts concerned with science and society, especially from those on the left with whom I would, in general, like to agree. Some on the left have seen the attention to academic science as irrelevant, the focus on texts as trivializing, and the commitment to relativism as dangerously idealist. For Marxists, this is a tactical issue; we will not be able to determine which view is correct, but we can see how the views affect the actions of people.

One tactical question that might be raised is whether one needs to study biology at all. Most critical discourse analysis has focused on such obviously political material as newspaper reports, or political speeches and talk about political issues.<sup>1</sup> But Steven Rose, Stephen Jay Gould, J. B. S. Haldane, and others have pointed out the political importance of biology. And I would argue that a treatment of biology needs to deal with the practices of the discipline in detail; the important points cannot be isolated as "concepts" or "themes" accessible to those outside the discipline. So an analysis of biology articles has, potentially, as much political interest as an analysis of the Pentagon Papers or the Watergate tapes.

Potter and Wetherell raise another tactical question about textual study:

People sometimes assume discourse analysis denies the existence of a world "out there." "Why this concentration on language," they ask, "when people out there are giving birth, making money, and being murdered by oppressive regimes? Why don't you study these

1. Diane Macdonell, *Theories of Discourse: An Introduction* (Oxford: Basil Blackwell, 1986); Gill Seidel, "Political Discourse Analysis." *Handbook of Discourse Analysis*, Vol. 4, ed. Teun van Dijk (London: Academic Press, 1985), pp. 43-60.

real processes and not just language which is a second-hand superficial medium?"

Their answer to this question (which admittedly is put in a rather overstated form) is the argument that there is no dichotomy "between 'real' events and linguistic representations of those events."<sup>2</sup> Potter's and Wetherell's own work, and that of some other discourse analysts focuses on the discourse of racism, and is hardly likely to be dismissed as irrelevant to the world "out there." The texts I study are not obviously political, but they are central to the processes of constructing facts, methods, and authority, in a field that is central to our view of ourselves and of society. While radicals within biology have tried to separate good biology from bad, I am arguing that nonbiologists need to know about the textual processes by which any biology is constructed.<sup>3</sup>

Some analysts of biology would object on political grounds to the relativism of my approach. Hilary and Steven Rose identify the Strong Programme in the sociology of science (the basis for my analysis) with other, more obviously reactionary threats to the "radical science" Rose and Rose had welcomed in several influential publications earlier in the 1970s.

It is this philosophical relativism which has moved from being a critique of other knowledges to an auto-critique of one's knowledge and on towards an escalating reflexivity. It is a hyper-reflexivity spoken of as the "disembodied dialectic" which, both within the sociology of scientific knowledge and within the radical movement, threatens to consume not only ideology but science itself. The certainties of the Althusserian disinction between scientific knowledge and ideology are to be obliterated, dissolved into their social determinations and a belief in the equality of discourses.

Apart from the rather peculiar jargon attributed to relativists, this could be taken as a fair summary of the relativist project and the response to Althusser. But by the end of the paragraph, the position they are criticizing leads, seemingly inevitably, to trivia. "To be cool, to be aware that we are playing in nothing more than a series of more

2. Potter and Wetherell, *Discourse and Social Psychology*, pp. 180, 181. Steve Woolgar presents an argument in more detail in "Discourse and Praxis."

3. On racist discourse, see the Spring 1988 issue of *Text*, edited by Teun van Dijk. On good and bad biology, see, for example, Rose et al., *Not in Our Genes*.

or less elaborate games, constitutes the new authenticity. The politics of subjectivism replace the pursuit of the rational society." In this view, there is no position between the acceptance of the authority of science on one hand and intellectual dandyism on the other. Any questioning of the special position of scientific knowledge "disarms radical scientists."

For instance, when the ideologues of scientific racism, such as Jensen and Eysenck "work on knowledge," is what they produce new knowledge, and if not, what is it? If it is fetishized consciousness, as RSJ [the *Radical Science Journal*] argues, there are no rational grounds for opposing it and the opposition to scientific racism must be seen exclusively in personal and moral terms. If we adopt the position of what is called 'the strong programme' in the sociology of knowledge we must presumably regard all these cultural products as new knowledges.<sup>4</sup>

This sounds as though the RSJ and the Strong Programme would tend to support scientific racism. But that conclusion comes because the passage makes an odd hybrid of the Strong Programme view of knowledge as beliefs about reality and another view in which objective knowledge of reality is the only basis for action. The combination of a skepticism about scientific knowledge and a belief in the absolute authority of some given body of knowledge would indeed be disastrous—the creationists in the United States are an example. But the Strong Programme is not asking what is really the case, it is asking why people believe what they believe.<sup>5</sup> The practical effect of the acceptance of their argument would not be an acceptance of racism, but a skepticism about all claims to scientific authority.

The political strategy of discourse analysis is based on the assumption that this skepticism about scientific authority is a good thing. For instance, some of the current local and national political issues that involve biological expertise include the government directives to farmers after Chernobyl, the possible statistical evidence for increased leukemia around our local nuclear power plant, the use of recombinant DNA technology in agriculture and pharmaceuticals, and the methods

4. Hilary Rose and Steven Rose, "Radical Science and Its Enemies," in *The Socialist Register*, 1979, ed. R. Miliband and John Saville (London: Merlin, 1979), pp. 317–35; quotations pp. 324, 326, respectively (footnotes omitted).

5. A Marxist response to Rose and Rose along these lines is the article by Joe Crocker, "Sociobiology: The Capitalist Synthesis."

used to quantify "quality of life" in the British National Health Service management.<sup>6</sup> In any of these cases, a critical approach to expertise is politically useful. An analysis that would translate the scientific issues into more recognizable political terms such as those of class struggle, state apparatuses, and forces and relations of production might have some use in the case of, say, Chernobyl, but what would be most useful to, say, Cumbrian sheep farmers, would be an understanding of the function and limits of expertise, and of its relation to their own practical expertise in such matters as grazing, weather, and sheep behavior. Such questions arise, not just with obviously controversial issues such as radiation risks, but in nearly every scientific and technological story in the news.<sup>7</sup> Of course, skepticism alone is not a political program, and Rose and Rose are right to point out the dangers in this direction. But it is a reasonable part of a political program. And it is particularly important when we realize that the political program is itself a part of discourse, that it needs to construct narratives and reinterpret the narratives of others if it is to be persuasive. One consequence I would like to see come out of this book, and out of many of the studies in the sociology of scientific knowledge, is a change in reading habits that would make us more active, critical readers of the scientific discourse that enters into our lives.

6. On Cumbrian sheep farmers' responses to experts, see Brian Wynne, Peter Williams, and Jean Williams, "Cumbrian Hill-Farmers' Views of Scientific Advice," *Evidence to the . . . Select Committee on Agriculture: The Chernobyl Disaster . . .* (Lancaster: Center for Science Studies and Science Policy, University of Lancaster, 1988); see also Brian Wynne, "Establishing the Rule of Laws: Constructing Expert Authority," in *Expert Evidence: Interpreting Science in the Law* (London: Routledge & Kegan Paul, 1988), and "Unruly Technology: Practical Rules, Impractical Discourses and Public Understanding," *Social Studies of Science* 18 (1988): 147-67; and, on health economics, Malcolm Ashmore, Michael Mulkay, and Trevor Pinch, *Health and Efficiency* (Milton Keynes: Open University Press, 1989).

7. The following news report (July 28, 1987, "You and Yours," BBC Radio 4) illustrates a problem readers may have with any study trying to show the social construction of scientific knowledge. A court had ruled that a defendant could be convicted of drunk driving, even if his alcohol level was below the legal limit when tested four hours after the accident, because a "scientific" method of "back calculation" could show that his alcohol level had been excessive earlier when he was driving. But a representative of the Police Surgeons Association had expressed doubts about the use of this method in a judicial context, saying there was disagreement within his organization about whether any individual case might have a radically different rate of absorption from the average of many cases that was used as a basis for extrapolation. So, the reporter immediately responded, the method is not *really* scientific. This is the kind of Catch-22 that keeps us from ever seeing the social in the scientific. At first, the fact that the method is "scientific"—without any qualification about what this might mean—puts it beyond any legal or political decision-making. But then, as social factors become apparent, the method is no longer seen as scientific.

I shall try to illustrate some strategies for critical reading with two texts as examples. One is a passage from a book on childrearing that claims that its conclusions are based on scientific research, *Girls and Boys: The Limits of Non-Sexist Childrearing*, by Sara Stein. It may seem too easy to be critical about such a book, but it can stand for a number of other, more sophisticated uses of scientific authority in popular texts. The passage is from a chapter titled, "Ten Million Years of Sexism."

[U]ncovering the origins of sexism will shed light on the puzzle we still haven't pieced together. Park bench impressions and laboratory measurements concur; there are sex differences. But what is the point of them? Or are they, as feminists claim, beside the point?

If our peculiarly human forms of sexism were invented, we would be surprised to find them in a close relative of man. But if they evolved, we should not be surprised at all. Our closest living relative is the chimpanzee. And chimpanzees are sexist.

Female chimps stay close to home. Males spend their day far afield. Females cluster with one or two best friends, their daily routine of gathering staple foods interrupted only to nurse a baby, break up a squabble, or scold a straying toddler. They love to fish for termites by poking sticks into the mound and nibbling off the soft, plump insect that cling to it (p. 31).

My other text is also about the social habits of primates; it is a table reproduced in Wilson's *Sociobiology* from an article Crook and Gartlan published in *Nature* in 1966 (see p. 254). Its validity as a set of categories is not my concern here. Nor is it my purpose, in choosing Stein's book from all the many popular texts that draw on sociobiology, to criticize either the anthropomorphism or the pseudoevolutionary argument of such popularizations.<sup>8</sup> What interests me here is how nonbiologists might approach such popular and specialized texts. Reviewing the studies in this book, I think of five strategies I would apply to both texts.

8. Similar arguments are analyzed in Rose et al., *Not in Our Genes*; in Dialectics of Biology Group (S. Rose, general editor), *Against Biological Determinism* (London: Allison & Busby, 1982), and in Lynda Burke and Jonathan Silvertown, eds., *More Than the Parts: Biology and Politics* (London: Pluto Press, 1984). For that matter, Wilson's chapter on primates in *Sociobiology* gives enough explanation of the difficulties of reasoning from primates to man to make one sceptical about this particular argument, even though Stein presents sociobiology as the authority for her argument.

Table 1. ADAPTIVE GRADES OF PRIMATES (SEE TEXT)

Species, ecological and behavioural characteristics	Grade I	Grade II	Grade III	Grade IV	Grade V
Species	<i>Microcebus</i> sp. <i>Chierogaleus</i> sp. <i>Phaner</i> sp. <i>Daubentonia</i> sp. <i>Lepilemur</i> <i>Galago</i> <i>Aotus trivirgatus</i> Forest	<i>Haplemur griseus</i> <i>Indri</i> <i>Propithecus</i> sp. <i>Avahi</i> <i>Lemur</i> sp. <i>Callicebus moloch</i> <i>Hylobates</i> sp. Forest	<i>Lemur macaca</i> <i>Alouatta palliata</i> <i>Saimiri sciureus</i> <i>Colobus</i> sp. <i>Cercopithecus ascanius</i> <i>Gorilla</i>	<i>Macaca mulatta</i> , etc. <i>Presbytis entellus</i> <i>Cercopithecus aethiops</i> <i>Papio cynocephalus</i> <i>Pan satyrus</i>	<i>Erythrocebus patas</i> <i>Papio hamadryas</i> <i>Theropithecus gelada</i>
Habitat	Forest	Forest	Forest-Forest fringe	Forest fringe, tree savannah	Grassland or arid savannah
Diet	Mostly insects	Fruit or leaves	Fruit or fruit and leaves. Stems, etc.	Vegetarian-omnivore Occasionally carnivorous in <i>Papio</i> and <i>Pan</i>	Vegetarian-omnivore <i>P. hamadryas</i> occasionally also carnivorous
Diurnal activity	Nocturnal	Crepuscular or diurnal	Diurnal	Diurnal	Diurnal
Size of groups	Usually solitary	Very small groups	Small to occasionally large parties	Medium to large groups. <i>Pan</i> groups inconstant in size	Medium to large groups, variable size in <i>T. gelada</i> and probably <i>P. hamadryas</i>
Reproductive units	Pairs where known	Small family parties based on single male	Multi-male groups	Multi-male groups	One-male groups
Male motility between groups	—	Probably slight	Yes—where known	Yes in <i>M. fuscata</i> and <i>C. aethiops</i> , otherwise not observed	Not observed
Sex dimorphism and social role differentiation	Slight	Slight	Slight—Size and behavioural dimorphism marked in <i>Gorilla</i> . Colour contrasts in <i>Lemur</i>	Marked dimorphism and role differentiation in <i>Papio</i> and <i>Macaca</i>	Marked dimorphism. Social role differentiation
Population dispersion	Limited information suggests territories	Territories with display, marking, etc.	Territories known in <i>Alouatta</i> , <i>Lemur</i> . Home ranges in <i>Gorilla</i> with some group avoidance probable	Territories with display in <i>C. aethiops</i> . Home ranges with avoidance or group combat in others. Extensive group mixing in <i>Pan</i>	Home ranges in <i>E. patas</i> , <i>P. hamadryas</i> and <i>T. gelada</i> show much congregation in feeding and sleeping. <i>T. gelada</i> in poor feeding conditions shows group dispersal

Crook and Gartlan's table of the Adaptive Grades of Primates from "The Evolution of Primate Societies," by J. H. Crook and J. S. Gartlan, *Nature*, Vol. 200, p. 1200. Reprinted by permission. Copyright © 1966, Macmillan Magazines, Ltd.



**1. Look for the rhetorical.** The rhetorical element is clear enough in Sara Stein's book; as the title suggests, she wants to convince parents that the gender roles currently defined in our society are natural and inevitable. She steps back from public debate on such matters to the authority of science, and, in a series of moves, comes to the conclusion that whatever traits are shared by humans and chimpanzees are unchangeable in man. Then she gives as the scientific facts a highly anthropomorphized account of chimpanzee behavior. As the blurb on the back of the book says, it is "a rare and reassuring blend of park-bench common sense and wide-ranging specialist research." Although the blend may be reassuring, it is hardly rare; it is a staple of popular scientific comment on human behavior.

In this book I have argued that we do not escape the rhetorical by going back and back to more and more specialized scientific works—to Wilson, and then to researchers like Crook and Gartlan, and even to such a seemingly arhetorical text as a table. For this writing, too, is rhetorical. Species are arranged so as to contrast certain features of their behavior and suggest relations of social organization to ecological facts. This table is not a way of presenting new data, as in, say, a table in an analytical chemistry article; instead it arranges information known from recent studies so as to suggest a new view of the evolution of primate behavior. So this table is not just a representation of what is known but an attempt to make other primatologists accept a claim about this knowledge. And this claim is likely to be contentious; for instance, when Wilson cites it he goes on to give an alternative table that he finds more persuasive. Each element of the table is also a potential matter for controversy. We should recall that Crews and his critics could produce tables of lizard behavior that suggested different interpretations of the relevance of the behavior to reproduction. Bloch, too, rearranged known data in a table that made a new and controversial claim. So such presentations of information are rhetorical, and never more so than when they seem simply to reproduce objective knowledge.

**2. Reconstruct the social context.** We have to remind ourselves of the social context of any scientific text because the form of scientific texts conceals the social—science covers its own tracks. So, for instance, Stein's appeal to scientific knowledge does not allow for the possibility of disagreements among primatologists, or for consideration of the traditional practices of ethology, or for questions about why the research was undertaken. But these omis-

sions are not just matters of simplification or vulgarization of more complete scientific originals. Crook and Gartlan's table does not, by itself, suggest that there could be any controversy about such a scheme. It cites its sources but does not say how various studies were made comparable for the purposes of this exercise in comparison, and does not show how this information was gleaned from a number of other texts. These issues would come out only if there were a controversy about the claim suggested by the table. Similarly, a reader of the latest texts in the *Cnemidophorus* controversy (chapter 4) would think that people in the field surely agree, for in each article the opening summary of the literature and the closing remarks present a firm and growing consensus. And a reader of Bloch's or Crews's published articles (chapter 3), after all their changes, has no way of knowing that the author originally wanted to say something quite different. The very form of the articles and the manner of their publications work against any indication of the social context.

**3. Look for related texts.** One way of seeing this social context is by restoring each of the fragments I have quoted to the context of other texts. In the case of the popularization one would start by looking up Jane Goodall and other primatologists, looking up other books by Stein, looking up other uses of sociobiology to define gender. D. R. Crocker has done this adeptly for just the sort of research and popularization of primate behavior we see here, to show the differences in the anthropomorphization in popular and specialized texts. The critiques of sociobiology make other sorts of political connections. Bruno Latour and Shirley C. Strum have linked such accounts of the origins of society to an even broader survey of philosophical and historical texts.<sup>9</sup>

But Crook and Gartlan's text also emerges from other texts. The authors give us some help here; they say that the table draws on recent work in the field, but also, and more surprisingly, on a model developed in ornithology and on evidence from paleontology. At the end of the article we see that this set of categories can be used to evaluate the hypotheses of other primatologists. And when we see the table in Wilson's book, we see how it is incorporated—or not—into later research. A critical reading of the table would lead us into these various subdisciplines and their texts. The making of such a

9. D. R. Crocker, "Anthropomorphism: Bad Practice, Honest Prejudice"; Latour and Strum, "Human Social Origins."

table affiliates Crook with some approaches to the study of primates in evolutionary terms, rather than with others. This sort of reconstruction and critical reading begins with comparison. Although the reader of Crews's *PNAS* article would not see the possibility of alternative views of the same set of actions by the lizards, the reader of the whole *Cnemidophorus* file, with all the articles, would begin to see different methods, different research goals, different philosophies of science. The nonscientific reader who reads a biology article might do well to start by figuring out the rhetorical point of each of the citations, as I have tried to do with Crews's and Bloch's citations in the articles studied in chapter 2.

**4. Look for the source of authority.** The authority of the passage from Stein, and of the whole book, is based on the happy discovery that "Park bench impressions and laboratory measurements concur: there are sex differences." She presents two kinds of authority, the authority of objective science and the authority of subjective common sense. And the great thing, she says, is that the two agree. But that is not surprising, since her heavily anthropomorphized descriptions of chimpanzees impose conventional social language on them, to find that they are just like humans. (The same sort of turn is done in a *Business Week* article that notes that sociobiology confirms Adam Smith's economics, "A Genetic Defense of the Free Market." But this is hardly surprising, because, as Wilson says, sociobiology takes its model of optimization from economics in the first place.) Science, for Stein, must be true because it confirms what we already know. This kind of authority contrasts with the other indicator of scientific authority in Stein's book, the references to individual scientists as experts, with the names of their institutions. Sometimes it seems the scientists are right because they confirm common sense, sometimes because they are uniquely gifted in abstruse knowledge. We saw the same sort of tension in the *Time* report of the *Cnemidophorus* research in Crews's lab.

The authority of Crook and Gartlan's table depends more on a different sort of authority, a different sort of consensus. It is persuasive because it marshals the studies of many different researchers and makes them all work to one end, to one claim. And the claim in it becomes more fact-like as more and more researchers believe it, use it, base other statements and other work on it. This cumulativeness of scientific texts is what gave Crews and Bloch trouble with the articles in chapter 3. At first Bloch's claim didn't seem to relate to anything,

didn't seem to have interesting work for other scientists in it. Crews's claim, on the other hand, threatened the work of other researchers; if he was right, they had to change their approach, and look at an aspect of behavior in relation to hormones that they hadn't considered before. The tight linking required in scientific discourse between one text and others is part of what makes such texts so forbidding to nonscientists who pull out one block in the middle of this pyramid of claims.

**5. Look for any links between scientific language and everyday uses of language.** Though scientific texts come out of an unusual social structure, and thus are different in some details from texts in other discourses, they are not doing something fundamentally different from other texts. We may stress too much the inaccessibility to nonspecialists of scientific writing. Science uses our language, and despite all attempts to purify it, it is still loaded with social and political implications. These implications are clear enough in Stein's passage. The whole passage, the whole chapter, depends on using the language of sexual stereotypes for descriptions of animal behavior. But as Crocker and others have argued, it is very difficult to eliminate anthropomorphism from behavioral studies. Similarly, the language of biology enters other discourses—such as that of childrearing. Our goal as critical readers should not be to purify the language either of everyday descriptions of behavior or of ethology, but to trace the movements back and forth between the discourses.<sup>10</sup>

If we are to track these textual transformations as critical readers, it is crucial that nonscientists not treat scientific texts as some sort of foreign language. If some of the passages I have analyzed in this book seem forbidding in their vocabulary and methods, this should not conceal the fact that they work just like other texts in English. We know, when we read newspaper articles on how to bring up one's baby, or when we read a letter from a solicitor, or even a poem, that we are stepping into areas of controversy, of rhetoric, of social conflicts. We do not read them as simply communicating data. I have argued in this book that the same is true of scientific texts; they must be put back in the social context from which they arise. As Frederic Jameson says, introducing his collection of readings of novels, "Inter-

10. Teri Walker, "Whose Discourse?" in *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge*, ed. Steve Woolgar (London and Beverly Hills: Sage, 1988), pp. 55–79.

pretation is not an isolated act, but takes place within a Homeric battlefield, on which a host of interpretive options are either openly or implicitly in conflict."<sup>11</sup> The scientific articles that may seem to the nonscientists to be fixed and conventional formats, filled with the appropriate facts and jargon, need to be seen as the battlegrounds on which the terms of knowledge are being defined. That we do not see the armies of the other interpretative options—the losing views of phenomena—is only because in this battle, the losing army is immediately buried. We see only the shining armor of the facts that remain.

11. Fredric Jameson, *The Political Unconscious: Narrative as a Socially Symbolic Act* (Ithaca: Cornell University Press, 1981), p. 13.