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15

Discourse, Rhetoric, Reflexivity

Seven Days in the Library

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. . . I'll start it with my Plan for the Week just to let you know right away what's going on.

Monday: I will find the Canonical Footnote and discover Discourse Analysis.
Tuesday: I shall look at Rhetorical Studies.
Wednesday: The day for Historical Studies and Science Education.
Thursday: When I intend to deal with Other Texts: Visual, Mathematical, Material.
Friday: The occasion for looking at Gender Studies.
Saturday: Will find me immersed in Studies of Social Science.
Sunday: A rest, surely? But no, I will be getting into Reviews, Reflexive Work, and Decisions.

But before we can start I must explain why I have written this "review" in the form of a "diary." (Why the accredited authors of this piece are three men, while I am not, must wait until the end to be fully explained.) I have four very specific reasons for foregrounding "form."

First, studies of discourse and rhetoric have broken down easy distinctions between form and content as well as showing the historical contingency and rhetorical orientation of the literary genres used in technoscience. The form of science writing has thus been made problematic. It is not just a matter of how *it* is put; the *it* is mixed up with the *putting*. This can, of course, simply be *said*, if not said simply. How much better, then, to *show* the mutual constitution of form and content in a form that, through its unconventional character, makes it visible.

Second, reviews, as I conclude on Sunday, are a particularly interesting and underanalyzed genre. As far as I can see, the only way to write a review, while at the same time attempting an analytic treatment of "the review," is to use a form that is "self-commenting." One prominent strategy I use is to point up certain noticeable absences in normal review discourse by foregrounding their presence in my text. For example, in this diary I situate myself in space and time, in networks of relationships, as a person with specific interests and a particular history.

Moreover, the task of reviewing, as we all know but seldom tell, is riddled with accident, serendipity, and loss. The rules of selection, the criteria for inclusion and exclusion, are only made explicit, if at all, post hoc; pre hoc, they are ad hoc. The appearance of coherence and order that is the achievement of the text is built on extremely shifting sands. My review, through its metaphor—if you insist on being Borgesian about it—of the working Library, with all its practicalities, frustrations, and miracles, attempts to display not just an achieved order but the processes contributing to that improbable result. I believe that work stressing the *achievement* of order is an important corrective to "final versions" that deny their own production conditions; I am with ethnomethodology *and* SSK on this.

Third, a point about my first person role as narrator in/of the text. This device is a method of reader identification. The progress of the narrator during her Seven Days in the Library mirrors the progress of the ideal reader of the text (and of the *Handbook* as a whole). Starting the week (text, book) as an interested but ignorant outsider, she ends it as a newly sophisticated paraparticipant (even to the point of being able to write this inappropriately placed explanation). Thus the writer and reader merge in a single textual figure representing the point of the project.

Last, an argument from tradition: In research in scientific discourse and rhetoric, one of the current orthodoxies is the use of "new" literary forms (Myers, 1992). People have written dialogues, plays, encyclopedias, lectures, fragments, and parodies; and now we have a diary. (And now I've found I'm not the first—I came across Rényi, 1984, on a table in the math section.)

And one more thing—gender: Gender has been a notorious "blind spot" in studies of technoscience, as we will see on Friday. Here it is, up front. For me, the form/content being inseparable, I *need* to be gendered, which means, doesn't it, *this* gender, to make the review work appropriately. I have just finished Jeanette Winterston's *Written on the Body* (1992) and am acutely sensitive to the strange literary effects that can be produced by characters without gender; in most science writing and writing on science, it is the presence of characters *with* gender that makes for strangeness. Prepare, then, to be "stranged"—and may you enjoy the experience.

The Epigraph

And now for the epigraph, which I hope you will find as appropriate as is appropriate (Katriel & Sanders, 1989). A *vademecum*, by the way, is a handbook. A classicist friend objects to my conflating *handbook* with *vademecum*. Apparently the *OED* defines *vademecum* as "a book or manual suitable for carrying about with one for ready reference." That sounds like a handbook to me, though perhaps not one this heavy.

> The vademecum is not simply the result of either a compilation or a collection of various journal contributions. The former is impossible because such papers often contradict each other. The latter does not yield a closed system, which is the goal of vademecum science. A vademecum is built up from individual contributions through selection and orderly arrangement like a mosaic from many colored stones. The plan according to which selection and arrangement are made will then provide the guidelines for further research. It governs the decision on what counts as a basic concept, what methods should be accepted, which research directions appear most promising, which scientists should be selected for prominent positions and which consigned to oblivion. Such a plan originates through esoteric communication of thought—during discussion among the experts, through mutual agreement and mutual misunderstanding, through mutual concessions and mutual incitement to obstinacy. (Fleck, 1935, p. 119)

MONDAY: THE CANONICAL FOOTNOTE AND DISCOURSE ANALYSIS

I never want to see another gerbil sand bathing again. I think I may give up on my dissertation; what really interests me now is not the behavior of rodents but the behavior of the psychologists and zoologists studying them. Specifically, what does this dissertation contribute to knowledge? What does a study like this do for me or anyone else? Why does it have to be written in that strange style and form?

Jamie said I should be thinking of switching to philosophy—there's lots of stuff on epistemology, she said. Well, I tried looking at that last week and only lasted a couple of days. I just couldn't see the point of most of it. Of interest though, from a "rhetorical perspective" (I suppose), a lot of the more recent philosophy of science (such as Hacking, 1983, and Galison, 1987) seemed to be engaged in heavy "intertextual negotiation" (does this kind of language make *any* sense?) with relativistic work in science studies. And to judge from the concessions made in such work, the philosophers are in retreat. Though traditional claims for realism and method are still made, they appear increasingly weak and residual. I enjoyed some of Kuhn (1962/1970)—very suggestive and obviously highly influential in science studies—and Feyerabend (1975) was fun though frequently infuriating. But really, I don't want to know what some philosopher thinks, I want to know what people have found out about scientific arguments and writing and talk. So I am now thinking I might switch to science studies, and I've promised myself, this time, a whole week in the library to see if I've got a new research topic.

This morning was awful. After queuing to get on a terminal, the first searches I tried were on scientific TALK and WRITING. Very little joy there—just a load of "how-to" books addressed to novices. So I browsed the stacks for a while to sophisticate myself and then tried again with RHETORIC and DISCOURSE, and hit the jackpot! For a while there I had gotten quite excited. There was a profusion of works with titles like *The Rhetoric of ___ or Discourse and ___* about almost every science and academic discipline. The same topic seemed also to be addressed in terms of texts and literary production as well as, yes, talk, writing—and even wrighting, whatever that is. There were titles that were conjugated from a set of esoteric and oh-so-trendy terms like *epistemology, representation, reflexivity, postmodernity.* There were journals called *Philosophy and Rhetoric* and *Discourse and Society.* It was just too much. Of this wealth of writing (about writing), what was Important and Central and what unnecessary and peripheral? Anyway, I got myself a random selection of these texts and began to read. It didn't take me long to realize that I was no longer enjoying myself. In fact, a feeling of depression set in. And a tell-tale headache began (why are libraries so *airless*?). Much of what I was plowing through was so . . . *pointless*, somehow. Full of airy discussion of the possibility and likely significance of a rhetorical and/or discourse analytical study of science yet devoid of any actual *analysis.* "If things don't improve," I thought, "it's back to the gerbils."

I decided to keep my sanity by looking for some *empirical* studies of scientific discourse and some review articles to help me find them. So I made myself a list:

1. Studies that actually analyze some text (instead of talking about "discourse" or "rhetoric" in general)
2. Studies that make generalizations extending beyond the text analyzed (instead of just giving a clever reading of *The Origin of Species*)
3. Studies that have something to say about science (instead of assuming an understanding of science at the outset)

Those guidelines, I thought, should clear out a lot of the undergrowth so that I could see the fauna.

I felt a lot better at lunchtime, sitting out on the square in the sun eating my sandwiches and skimming through a sheaf of photocopies of science studies reviews and introductory chapters. Though many of these were far from complimentary about what was usually referred to as "discourse analysis," they were helpful in one very important respect. They all appeared to carry a version of what I have come to call "The Canonical Footnote," which represents mainstream science studies' standard gesture toward *all* studies of "scientific language." Looking back, I am unsure now if the note as I write it here ever existed in precisely this form—though as I (now) understand it, such "intertextual uncertainty" can only enhance its canonical status (I think!). (Of course, I [have] also made a list of the full bibliographical details of these texts.)

[26]See, for instance, Medawar (1964); Gusfield (1976); Woolgar (1976, 1980); Latour and Woolgar (1979); Knorr Cetina (1981); Yearley (1981); Law and Williams (1982); Mulkay, Potter, and Yearley (1983); Gilbert & Mulkay (1984); Latour (1985/1986/1990, 1987); Lynch (1985a); Mulkay (1985); Shapin and Schaffer (1985); Potter and Wetherell (1987); Bazerman (1988); Ashmore (1989); Myers (1990a).[1]

Anyway, the CF was definitely the best find of the day so far and proved to be very fertile. I spent the rest of the afternoon collecting all the available CF texts. Unfortunately, Potter and Wetherell was on loan, Ashmore was on order, and they didn't have Myers, but I managed to find most of the others. Leafing through them I noticed that many had pictures (scientific diagrams and photographs mostly, though Latour's book, 1987, had a much greater variety). Most of them seemed to analyze conversations transcribed in comic detail. Several (Latour and Woolgar, Knorr Cetina, Law and Williams, and Lynch) were apparently *laboratory studies,* a term I learned from the science studies reviews. Of interest, the life sciences were by far the most popular site for study and only Shapin and Schaffer and part of Bazerman dared to study physics. Incidentally, there also seems to be a flourishing industry in analyses of medical discourse, to judge from Soyland (1991).

The impression that most of these texts deal only with the "softer" end of science was confirmed by a glance at Gusfield's piece. Though called "The Literary Rhetoric of *Science,*" it was about some form of social policy research on drunk drivers! Still, maybe I shouldn't be too snobbish—gerbil watching is hardly atomic physics. Gusfield's paper was most peculiar—it is set out almost as a play with three Acts, a Prologue, and an Epilogue. However, it was not half so weird as Mulkay's book, which not only includes a "real play" with characters and everything but also has a whole series of other "alternative textual forms," which I really failed to see the point of. I'm clearly not up to this sort of thing yet and I'm far from sure I want to be. Another rather odd text is Latour's, which is full of funny diagrams and jokes.

Finally, I settled in a quiet corner of the library bar with a coffee and Nigel Gilbert and Michael Mulkay's *Opening Pandora's Box* (1984) for a more detailed read. Their main idea, as I understand it, is that scientists' discourse is structured by the use of two basic "accounting repertoires"—the "empiricist" and the "contingent." In a chapter called "Accounting for Error," they analyze various extracts from their interviews with biochemists involved in a controversy and find that the scientists' use of each repertoire is overwhelmingly asymmetric with respect to their assessments of each other's correctness. In particular, Gilbert and Mulkay claim the empiricist repertoire, in which the facts speak for themselves, is used to account for correctness (such as each scientist's *own* position) while the contingent repertoire, in which social and personal factors play a part, is used in accounts of (others') error.

> Each speaker who formulates his own position in empiricist terms, when accounting for error, sets up the following interpretative problem: "If the natural world speaks so clearly through the respondent in question, how is it that some other scientists come to present that world inaccurately?" . . . [T]he introduction of the contingent repertoire resolves the speaker's dilemma by showing that the speech of those in error . . . is easily understood in view of "what we all know about" the typical limitations of scientists as fallible human beings. . . . . Because contingent factors are mentioned only in the case of false belief, because they are directly contrasted with the purely experimental basis of the speaker's views and because their power to generate and maintain false belief is taken as self-evident, the contingency of scientists' actions and beliefs is made to appear anomalous and as a necessary source of, as well as an explanation of, theoretical error. (Gilbert & Mulkay, 1984, pp. 69-70)

This analysis made some sense to me: I had always been rather puzzled about how my colleagues could so easily explain away competitive results while holding fast to the "empiricist version" of their own. What puzzled me now was why Mulkay should have gone on to write plays—but that would have to wait for another day. The light was fading, the library was closing, and discourse analysis had had its allotted time. Tomorrow was rhetoric. Tonight was . . . well, none of your business, really.

TUESDAY: RHETORICAL STUDIES

Perhaps I should get up later. I have to report that the morning was awful—again! The phrase *rhetoric of science* is so suggestive and I was really looking forward to finding out what it might mean. So I started with a recent review article by Randy Harris (1991) with that title and then hunted up some of his references. Harris spends a lot of time categorizing the various approaches, rather like rhetorical treatises do, but he's funnier than a treatise. The big problem, though, is that he had the kind of bibliography that almost sent me running back to the gerbils. Most of his references were by people in the Communication or Composition programs of big North American universities. There were lots of grand theoretical statements of the "Rhetoric as Epistemic" sort (R. L. Scott, 1976; Weimer, 1977), which I skipped. Then there were studies for which rhetoric means various features of style and studies for which rhetoric means arguments. There was almost a whole shelf of studies of scientific style (C. L. Barber, 1962; Gopnik, 1972; Huddleston, 1971). But they didn't seem to be interested in what scientists do; they just tabulated the verb types or sentences as if they'd been handed some literary work. So I looked around for something that had more to do with science.

There are lots and lots of people finding that scientists use metaphor or other figures of speech (D. McCloskey, 1985) or that there is some other way that the framework of classical rhetoric can be applied to them. The rhetorical studies seem to begin by asserting that there is something striking about the very idea of a rhetoric of science, and assume their readers will think of science as unrhetorical and unliterary (J. A. Campbell, 1987; Gross, 1990; Halloran, 1986; Myers, 1985). Most of these choose canonical figures like Newton, Darwin, or Einstein, rather than the kinds of psychologists and zoologists I know and love. I found the *Quarterly Journal of Speech*, which seems to have rhetoric of science articles frequently, and turned to a paper by John Lyne and Henry Howe (1986) on Stephen Jay Gould and Niles Eldredge. But Lyne and Howe hardly refer to the texts at all, offering their own rather loaded paraphrase instead. And what's worse, in their account rhetoric is contrasted with scientific truth—the good guys don't need rhetoric. Hmm, well . . .

Later, and as something of an antidote, I went back to the science studies section on Level 4 and found an issue of *Science, Technology, & Human Values* with a section on rhetoric (Bazerman, 1989; Fahnestock, 1989; C. Miller, 1989; Waddell, 1989; Woolgar, 1989b). These articles focus on specific, current controversies. Jeanne Fahnestock, who looks at a controversy over when people first crossed the Bering Straits to the New World, has a rather more appealing definition of the rhetorical approach that emphasizes forms of argument and textual detail:

> An analysis of texts from a rhetorical perspective asks what tactics and topics of argumentation are used, how the arguments are arranged sequentially as a series of effects, and how they are actually expressed, their precise wording, their qualifications (or lack thereof), their indirection, their use of figures (tropes and schemas). The rhetorician is primarily interested in explaining textual features as an arguer's creative response to the constraints of a particular situation. (Fahnestock, 1989, p. 27)

This seemed a tall order! I looked for an example and found she talks about something called *copia*, "the technique of enumeration or listing, creating a series that suggests a large number of things, too many for the writer or speaker to specify" (Fahnestock, 1989, p. 37). This is the sort of commentary Fahnestock gives on a passage from one of the articles in the controversy that uses a long list of sites as part of its argument.

> The first three items in the series are fuller, specifying the artifact as well as the location. From then on only sites are named. The impression created is one of increasing quickness as though the arguer were speedily recalling instances from a vast store. The speed creates the impression that the author could name many more sites if either he or the audience had the time. That is precisely the impression *copia* is supposed to create. (Fahnestock, 1989, p. 38)

At first I thought this was just an exercise in dusting off a classical term and giving some modern examples (and as Woolgar, 1989b, might argue, Fahnestock's enumeration and listing of examples of *copia* must themselves be examples of *copia*). But Fahnestock goes further, comparing specialist publications and popularizations, and relating the choices of devices to the dynamics of the particular controversy.

But in the end I was left wondering what the status of this system was supposed to be. What does it mean that there are examples of *copia* in the work of physical anthropologists (or economists, or whatever)? Surely not that these scientists must have been taught classical rhetoric as Shakespeare or Milton were. Was it that the classical notions of persuasive approaches define universals, or perhaps define a particular cultural line of what is persuasive? Or perhaps this kind of work merely shows that, given a long tradition of lists, terms, and broadly defined features, a trained analyst can find them anywhere. For Fahnestock, "rhetoric" provides the terms, and thus a way of organizing the complex data of comparisons. But as I drifted out of the main entrance and headed for the bus stop, I was left thinking, "So what?" However, tomorrow is another day, as they say.

WEDNESDAY: HISTORICAL STUDIES AND SCIENCE EDUCATION

Last night something else started to bother me about these rhetoricians; they don't try to deal with the ways texts are treated by the scientists in context. All they have is this ahistorical, technical system for analyzing them. They didn't tell me much about science that I didn't already know. So this morning I headed straight for the history of science books—except that they were not with the science studies collection on Level 4. It turned out they were in the general science and engineering section on Level 1. So I'd traversed the entire library before I started. After getting my breath back, I tried looking through the last few years of indexes in *Isis* and found a large amount of history of science consists of very detailed accounts of how a text was read in its time and of its influence in later times (Bazerman, 1991; Cantor, 1987; Dear, 1985, 1987; Golinski, 1987; Le Grand, 1986; Schaffer, 1986, 1989; Yeo, 1986; R. Young, 1986). I was lucky to find a new collection (Dear, 1991) that led me back to some cases from the seventeenth, eighteenth, and nineteenth centuries. Despite the fact that (I admit it) I hadn't heard of a lot of the famous scientists discussed, and despite the rather oppressive weight of the footnotes, I enjoyed it. After the first shock, I found the articles dealt with more general issues than their titles seemed to indicate: ideas of genre (Thomas Broman), visual representation (Lissa Roberts), and narrative (Peter Dear and Frederick Holmes). I was surprised these current issues went so far back, and there was a satisfyingly solid feel to the cases.

I found a sort of a review in Markus (1987), but it said it was about *hermeneutics*. Lots of these articles aren't about *rhetoric* or *discourse*; they say they are about things like *documents, method, context*, or *tradition*. But they do have detailed readings of texts. These historians allow none of the grand generalizations of the rhetoricians about ahistorical categories, about likely responses, about ideas that were in the cultural soup at the time. Everything is pinned down to some letter or marginal annotation, a manuscript somewhere, in a way that is alarming to people working far from a major library. (I don't want to be insulting; this place is fine, no really, but . . .) Lots of these history of science articles point to the need to go beyond published sources to notebooks, letters, drafts (Bazerman, 1988; Herbert, 1991; Holmes, 1987; Myers, 1990a, chap. 3; Rudwick, 1985). Others blithely infer authors' intentions from the texts (Block, 1985; Sapp, 1986) without using evidence for earlier stages. The method in most of these studies is to focus on change: from notebook to publication, or from draft to draft, or from publication to publication. For instance, Frederick Holmes (1987) looks in detail at Lavoisier's notebooks. Living scientists only occasionally get this attention (Myers, 1985). Perhaps historians' self-identities would be compromised were they to deal too often with the living.

One of those terrifyingly detailed articles had a tempting subtitle: "Saving Newton's Text: Documents, Readers, and the Ways of the World" (Palter, 1987). A quick skim showed Palter was interested in the dating and interpretation of a text by Newton I'd never heard of. He is a true historian of science in his concern with the documentary details, like whether Newton had Galileo in his library, or had access to it, or knew Italian (fn. p. 392)—quite a change after the grand generalizations of the literary-rhetorical types. But he is struck by the problem of how historians and philosophers disagree in their interpretations. That leads him to some reflections on how interpretation is possible at all, with no access to the intended meaning, to the readings of the time, or to the worldview, except through texts (what he calls "pantextuality"). (I saw there was the same sort of division of the problem in Marcus, 1987: "The inscribed author," "The intended reader," and "The work in the context of its tradition.") Palter points out the circularity of dating the Newton manuscript as early because of its immaturity, and then finding support for its immaturity in its early date. He ends up by explaining the differing interpretations of historians and philosophers of science, in terms of their distinctive disciplinary orientations and styles. The task in his view is to place this text in relation to other texts, other conceptions of space and time, even later ones. What I don't get is how he then decides the other interpretations are wrong, which is an approach that seems to ally him with philosophers, after all.

About midafternoon, I decided to give myself a break. I had been wondering whether there had been any relevant work done on science education. I suppose my interest in this area was based on an analogy with infancy: If you can help explain adult capacities through studying infant development, you should be able to cast some light on scientific practice through an understanding of scientists' socialization in science education. On the basis of my brief search, however, there seemed to be very little. It seems education, as that Delamont (1987) piece in *Social Studies of Science* put it, is indeed a notable "blind spot" in current work in science studies. Most of the stuff I found seemed either not to take much notice of the discourse (Collins & Shapin, 1983; Millar, 1989) or to be more concerned with education generally than science in particular (Mehan, 1979). There was a new book for science teachers that focused attention on language (Sutton, 1992). Not exactly trailblazing, but good examples. A book by Valerie Walkerdine (1988) looked at mathematics teaching and made some nice points about gender issues. Apparently if you use the "three bears story" to teach size relations to a mixed class of kids, you come unstuck because of hidden assumptions about gender and size. An ethnography by Derek Edwards and Neil Mercer (1987) focused on the way teachers deal with the contradictory requirements of "child-centered pedagogy" and of experiments having "right" outcomes. You have to subtly lead the kids to the right answer but always give the impression that they found it themselves, which is also the conclusion of a "nondiscourse" study by Atkinson and Delamont (1977). The authors don't make the point, but I wonder at the problems that this generates for any of these kids going on to do real science when there will be no one subtly pushing them in the right direction. I know that my (ex?) Ph.D. work is worryingly hard to square with the stories I learned about science at school. And the worst thing is how the aura of certainty surrounding those old stories is perpetuated in the textbooks I still have to read. While some theorists of science recognize this disparity, most (Brush, 1974; Kuhn, 1962/1970) rather insultingly consider that it is necessary for our morale to be told comforting stories.

In the end, libraries are so oppressive. Full of words, words, words. Zillions of them! I need a hot bath and a cup of tea.

THURSDAY: OTHER TEXTS—VISUAL, MATHEMATICAL, MATERIAL

It was as much as I could do to drag myself in this morning. "If there's one good thing about gerbils," I thought, "it's that they don't write and don't talk." Word sickness, you could call it. A bad case of. So today I'm avoiding them—as much as possible, that is. I have settled myself near the art history stacks; the atmosphere suits my mood.

And really, this isn't as silly or self-defeating as it sounds. After all, most of what I was doing with the gerbils involved persuasion through numbers or graphs. There were some chapters or comments on illustrations by authors in the Canonical Footnote (Gilbert & Mulkay, 1984, chap. 7; Latour, 1985/1986/1990; Lynch, 1985a). But there were lots of other studies, scattered all over the library, from history, visual arts, and cultural studies (Fox & Lawrence, 1988; Ivins, 1973; Jacobi, 1985; Rudwick, 1976; Silverstone, 1985) and some collections (Fyfe & Law, 1988; Latour & de Noblet, 1985; Lynch & Woolgar, 1990). These read quite differently than the studies of written texts, even those by the same authors. Academics seem to know what to do with writing—find metaphors in it, or find an argument, or look at the pronouns. There seems to be no general agreement about how to approach visual images; for example, some use art history (Latour, 1985/1986/1990) and some use semiotics (Bastide, 1985/1990; Myers, 1990b). Or maybe the problem is that people don't see any need to *read* visual texts; they are just there.

I found another set of stuff that had been shelved under semiotics; puzzlingly, most of this work also went under the name of "actor-network theory" or the "sociology of translation" (Callon, Law, & Rip, 1986; Latour, 1987; Law, 1986c, 1991c). I got quite excited. It seemed to promise a much more *complete* framework of analysis than the straight rhetorical analyses I had been looking at. It claims to deal with power, politics, agency, knowledge, the association of people and things, and no doubt everything else there is. However, when I showed the stuff to Karla (who has nearly finished her doctorate on Baudrillard, the State, and modern architecture), she said it wasn't what she thought of as semiotics, and then lost me with a lot of talk about paradigmatic oppositions, free-floating signifiers, and the relation between poststructuralism and postmodernism. Rather reluctantly, I concluded that actor-network theory should not be in this review.

And, to tell the truth, I was getting distracted from the books by the realization—was this trivial or significant?—that peoples' clothes here on Level 3 (Art and Humanities) were so much more interesting than on Level 1 (Science and Engineering). As I walked up the stairs to Level 4 (Social Sciences), I began to wonder if I looked the part. How *did* science studies researchers dress? And, just as important, would any of them find this an interesting question?

I finally settled down at my favorite table in the science studies section with a piece by Michael Lynch (1990), because I liked the choice of pictures, lots of related examples. He talks about how images like photographs get made into images like graphs, talking about *selection* and *mathematicization*. First, he looks at pairs of photographs and diagrams, which were like those I had in my own textbooks, analyzing what happens in the transformation. Then he looks at how quantitative data are translated from visual data; I particularly appreciated the example from a field study of lizards. He says these processes are not just a matter of reducing complexity; they *add* visual features as well. He says, "Specimen materials are "shaped" in terms of the geometric parameters of the graph, so that mathematical analysis and natural phenomena do not so much *correspond* as do they *merge* indistinguishably on the ground of the literary representation" (Lynch, 1990, p. 181). Now what does that mean? First, I think he's saying we see the gerbils in terms of the graphs we are finally going to make of them. Sand bathing becomes a countable behavior, not something they do "in real time." But also, when we make an image, it is hard then to separate that image from the object it represents, maybe like the way I think of hormone levels.

Now that Lynch has made me aware of mathematicization, I see there are other, mainly historical, studies that deal with this issue (Dear, 1987; Kuhn, 1977b; Restivo, 1990; Shapin, 1988c). Most of these, though, seemed interested in mathematicization in the abstract. In contrast, a paper by Potter, Wetherell, and Chitty (1991) tried to show how different sorts of mathematicization could be put to different rhetorical purposes. For example, they draw a rather nice analogy between the way a table of cancer death statistics is constructed and the way a market trader constructs an item as a bargain. I particularly liked the idea that numbers, which are often treated as hard, solid things in contrast to airy rhetoric, can themselves be viewed as *the* most effective form of rhetoric.

Also, I am starting to note a theme that crops up repeatedly in many of the studies I have been looking at. Researchers use variations between stretches of discourse as a guide to what the discourse is used to do. I can also see that very soon I am going to actually do some of this sort of analysis so I can really understand how it works. I get the impression that there are some important craft skills at work here that do not come over very clearly in the published papers.

If images and numbers can be read as "text," why can't everything else? No reason at all, it seems. Latour (1987) writes about laboratory apparatus as an "inscription device"; Woolgar (1985, 1991b) advocates a "sociology of machines"; and someone called Jim Johnson (1988) has studied the semiotics of a door-closer! I also saw some titles of articles and chapters that dealt with space, especially with the space of laboratories. Latour and Woolgar (1979) start with a tour of a biochemistry lab, and Traweek (1988) has a chapter that describes all the buildings of a high-energy physics lab, including the offices and cafeteria. There was a piece on the design of British university science buildings (Forgan, 1989) and, on the current periodicals shelves, which I looked at before I left, there was a whole issue of *Science in Context* (Ophir, Shapin, & Schaffer, 1991), with yet another piece by the ubiquitous Michael Lynch (1991)! But I'm getting confused. If authors and readers are texts, and gerbils are texts, and labs are texts, what isn't a text? A meal and a bottle of wine, perhaps? No, *don't* tell me about Mary Douglas (1975).

Oh yes, I picked up a splendid-looking book called *Incorporations* (Crary & Kwinter, 1992) from the new acquisitions on the way out. It is full of photos printed on different-color paper and contains lots of short pieces on machines, architecture, and science from such as Félix Guattari, John O'Neill, and the novelist J. G. Ballard. Jamie dismissed it as a postmodernist coffee-table book. It made me uncomfortable, though. Too many distinctions blurred, perhaps, just as I was starting to get them clear. And although it is not *about* the rhetoric of science, it succeeds in commenting obliquely on such matters, through, I am beginning to think, the way it foregrounds form even, perhaps especially, in its self-conscious stylishness.

FRIDAY: GENDER STUDIES

This morning was—pretty good, really, if rather puzzling. Though I've been slogging away for 5 days and coming across studies of scientific discourse from lots of different disciplines, I've seen remarkably little in women's studies. So today, that's what I've been concentrating on. Right off I found a good review in *Theory and Society* (Jansen, 1990) of books by people like Sandra Harding and Evelyn Fox Keller, and it said that feminist critics of science had made use of the same sociologists of scientific knowledge (mainly Bloor and Barnes of the Edinburgh/Strong Programme tendency) as these discourse people. On the other hand, Delamont (1987) argues that sociology of scientific knowledge ignored feminism, which on my reading is quite correct. So what is the relationship? Or is this an irrelevance for discourse studies?

Though there are a number of books on science and gender (Keller, 1985), on women's careers (or lack of them; Kelly, 1987), on famous women scientists (Keller, 1982), on the biases of various disciplines, especially biology (Brighton Women and Science Group, 1980), on the way sciences construct women (MacCormack & Strathern, 1980), and on the masculine orientation of scientific epistemology (Harding, 1986; Harding & Hintikka, 1983), what I don't find are detailed readings of texts. Is this because they are often looking at a broader level? On the other hand, the people combing texts for signs of social construction don't seem to be much interested in gender. They raise epistemological questions but aren't interested in alternative epistemologies. What's the problem? Surely this area should not be impervious to feminism. So I spent the rest of the morning skimming tables of contents in collections on scientific discourse and looking through the feminist studies for a long quotation.

It turns out that feminist studies do raise questions about texts but don't usually foreground the method of discourse analysis. For instance, Jordanova's *Sexual Visions* (1989) has close readings of Michelet and of wax dummies. She points out the dangers of superimposing contrasts of male and female on contrasts of culture and nature, with a sharp conclusion. There's another collection of detailed studies of particular issues by Schiebinger, *The Mind Has No Sex?* (1989), that has a nice chapter on allegorical representations of science as a woman, especially in the frontispieces of seventeenth- and eighteenth-century books; it neatly complements Jordanova's chapter on the image of unveiling and dissections. She too sees a complex interaction of various contrasts revolving around masculinity and femininity, for instance, in a brief comment on scientific and literary style in the eighteenth century. Traweek's essay "Border Crossings" (1992) raises the issue of texts by giving her own text a narrative form that dramatizes what she calls her marginality, her difference; there is no analysis of texts but there is an implied critique of existing scholarly forms. Star's (1991b) essay "On Being Allergic to Onions" tries to reorient the actor-network studies I read about yesterday, to see these networks from the margins rather than from the perspective of a masterful strategist. Haraway's huge and fascinating *Primate Visions* (1989) deals with some alternative forms of texts explored by primatologists like Zihlman and Hrdy. It's interesting that all these authors deal as much with nonverbal as with verbal texts. Maybe this is just the way people work in cultural studies in the late 1980s. But maybe pictures play off against texts in a way that is useful to them. Maybe the reason I haven't found more close readings of scientific writing in feminist studies is that there is something about the assumptions underlying this kind of "literary" work that is inconsistent with recent feminist approaches; if close reading is the attempted provision of a One Best Reading, it is not going to appeal to those who wish to legitimate a diversity of readings.

A collection edited by Jacobus, Keller, and Shuttleworth (1990) has lots of articles with some textual analysis, though again none on the methods used. In it, Emily Martin (1990) has a piece about kinds of language used to describe menstruation. She analyzes metaphors in textbooks, and as one might expect she finds a male view in and as the scientific view. But this does not lead her to a juxtaposition of false male science with true female experience. She reports a study in which she asked various women two questions: "What is your own understanding of menstruation?" and "How would you explain menstruation to a young girl who didn't know about it?" She contrasts one kind of language women use to talk about menstruation—"internal organs, structures, and functions," with another she calls "the phenomenology of menstruation"—what it feels and looks like to the woman experiencing it. The working-class women interviewed "share an absolute reluctance to give the medical view of menstruation." It's not just that there's a scientific mode and a popular mode but that there are significant and analyzable ways of slipping into or out of the scientific mode of talking about one's own body.

With a tip from Susan, who was also spending time in the library, I finally found a direct link between feminism and discourse analysis in the sociology of scientific knowledge. It was in what I thought was a surprising place, Celia Kitzinger's *The Social Construction of Lesbianism* (1988). She draws on Gilbert and Mulkay (1984) in a critique of the rhetoric of earlier psychological work on gays. Now, I thought, that should be like shooting fish in a barrel, ironicizing the scientism of all that stuff on homosexuality as an illness. But Kitzinger does something more complicated and much more interesting; she also analyzes the line of research on gayness as "lifestyle" that I was naively expecting her to favor and thus to exempt from scrutiny. She even includes quotations from her own work as she shows how the two sides invoke scientific authority. In her next chapter she confronts the issue of political commitment and concludes that it is still possible (as her own work suggests) to work for a clear political aim while still insisting on analyzing *all* relevant discourses, however they may be evaluated.

The idea of competing discourses or registers or styles seems consistent with the largely separate line of work in sociology of scientific knowledge, such as Gilbert and Mulkay's (1984) "repertoires" (so why are they so often separate?). I'd come across similar sorts of juxtapositions in reading earlier in the week, like Walker (1988) on therapy talk and Brodkey (1987) on attempts to collaborate on a paper in feminist literary criticism. There was a recent piece by Mulkay (1989) that tried to link his own work to feminist critiques of science, largely through the valorization of multiplicity. So maybe what I am seeing is similar methods—bringing out the multiplicity of discourses—put to different ends—the critique of scientific authority and of patriarchal knowledge. Maybe that's my problem. They use similar methods sometimes, but feminist studies of science and social studies of scientific knowledge seem better defined by their separate missions than their common methods. Question: How Bad a Thing can this be, if diversity is what they both value?

On the bus home I read Donna Haraway's "Manifesto for Cyborgs" (1985), which Susan had lent me; I must remember to make a copy of it tomorrow. Now, this could not be fitted into any one of my day's plans; nor did it fit easily with what I am coming to understand as the usual forms of theorizing or analysis in studies of technoscience. It mixed political polemic, feminism, sociology of science, and cultural studies—and quite uniquely. It could have been a mess—it *should* have been a mess—but it really wasn't. I was rather inspired by it. And again, the text was not so much *about* rhetoric as it was a very conscious, some may say "arch," display of a different rhetoric—indeed, a rhetoric of difference.

SATURDAY: STUDIES OF SOCIAL SCIENCE

I was late in this morning because I had to get to the shops as I'd run out of . . . Sorry. Start again. On Monday, which feels a long time ago, I was appallingly dismissive of the Canonical Footnoters' tendency to analyze only the softer end of science. It has struck me forcibly since that this attitude is ridiculous—and not only because I am now trying to become a much softer scientist than I used to be. The real point, of course, is that the division of the sciences into "hard" and "soft" is itself a historically analyzable phenomenon and a rather obviously gendered rhetorical device. So to make amends, I am going to focus on social science analyses today. My favorite table had been taken so I carried my new heap of books to a quiet corner of the top floor that had a soothing view of the still frosty countryside just off campus.

One thing I saw immediately is that it is much more difficult to tell who this work on social science is for. The natural/biological science stuff is typically published in journals like *Isis* or *Social Studies of Science*, and you don't get the feeling that particle physicists or plant biologists are meant to read it. However, the social science work is often published in places where the readers are going to be the people the studies are about and often it does not address the theoretical concerns of science and technology studies at all. Take psychology, for example. There is some work dealing with themes like repertoires and categorizations, which reminded me of sociology of science discourse analysis (McKinlay & Potter, 1987; Potter, 1984, 1988; Potter & Mulkay, 1985). Yet there are also plenty of papers—like Harré (1981, with a neat section here called "The Rhetoric of Social Psychological Theory Considered as Talk"), Lubek (1976), Billig (1991), and many in the volume edited by Parker and Shotter (1990)—which seem to be mostly concerned with effecting change in the discipline itself.

A lot of the most interesting stuff seemed to work on both levels, talking to some group of social scientists as well as to sociologists and rhetoricians of scientific knowledge. Bazerman's (1988, chap. 9) piece on the American Psychological Association style manual tried to show how the assumptions of behaviorism were embodied in the prescribed textual form of the discipline. I liked Steve Woolgar and Dorothy Pawluch's (1985) argument about the way researchers into social problems rig the outcome of their discussions by a process of "ontological gerrymandering."

> The successful social problems explanation depends on making problematic the truth status of certain states of affairs selected for analysis and explanation, while backgrounding or minimizing the possibility that the same problems apply to assumptions upon which the analysis depends. By means of ontological gerrymandering, proponents of definitional explanation place a boundary between assumptions which are to be understood as (ostensibly) problematic and those which are not. This "boundary work" creates and sustains the differential susceptibility of phenomena to ontological uncertainty. (Woolgar & Pawluch, 1985, p. 216)

I could see that this kind of analysis could be more generally applied to a lot of areas of scientific work.

I found a rhetoric of economics (D. McCloskey, 1985), a poetic for anthropology (Clifford & Marcus, 1986), and one of each for sociology (R. H. Brown, 1977; Edmondson, 1984). D. McCloskey's work is interesting as it appears to be part of a large and booming project known as "rhetoric of inquiry" (Nelson & Megill, 1986), which seems to have covered every known discipline in the humanities/human sciences area and now boasts at least three large and diverse collections (Nelson, Megill, & McCloskey, 1987; Simons, 1989, 1990). It is anthropologists, though, who seem to have undertaken the most comprehensive and radical look at the way their own discourse works, both through rereadings of the classics (Marcus & Cushman, 1982; Stocking, 1983) and rewritings of ethnographies (Tyler, 1987; S. Webster, 1982).

However, I was particularly taken by a book by Paul Atkinson (1990). This looks at sociological ethnography as a textual form using particular conventions and devices to produce a "reality effect." It draws on some of the stuff I encountered earlier in the week like literary theory, ethnomethodological ideas, and sociology of science. For example, Atkinson closely compares the introductory paragraphs of a short story by Hemingway (actually, he gets this from Fowler, 1977) and a well-known ethnography. He shows how both use very similar devices to get the reader into the story and suggests that these devices "serve to warrant the subsequent sociological discourse by establishing its *vraisemblance* [naturalness or genuineness]. It furnishes the 'guarantee' of an eyewitness report, couched in terms of the dispassionate observer, using the conventional style of the realist writer of fiction, or documentary reporter" (Atkinson, 1990, p. 70).

I wondered if some of the descriptions in my diary were able to generate this effect. Probably not! Atkinson did not seem to have any particular story to tell about the general role of this reality construction in ethnography, except that it makes it convincing and we should pay more attention to it. In contrast, a piece by Peter Stringer (in Potter, Stringer, & Wetherell, 1984) tried to show how in a social science setting the rhetoric of the researcher can become mixed up with the rhetoric of the people who are being researched. Rather neatly, it shows the way President Kennedy's key advisers' excuses and justifications for the Bay of Pigs fiasco become incorporated in the "scientific" account of this group's "group processes" given by a social psychologist. I noted that Stringer was insistent that his own text should not be treated as immune from such problems.

The next thing of his I read made me see exactly how insistent he was. This was a paper called "You Decide What Your Title Is to Be and [Read] Write to That Title" (1985). Tucked away in a book on a rather obscure psychological theory, it is an . . . I am rather at a loss for the word here . . . an *exploration* of various textual forms in social science: the review, the title, the textbook, informal talk, exam papers. For example, it shows how the version of a particular theory in textbooks is a product of the particular organizational scheme used, and that textbooks "demonstrate despite themselves the inevitability of the re-writing that reading evokes" (Stringer, 1985, p. 221). Through a set of textual "fragments," it questions standard ways of understanding the relation between authors and readers, and between truths and fictions. Its final challenge to the reader is to be more active in your own readings—hence the title. I now recognize this as a "reflexive piece," a "new literary form." Ashmore, Mulkay, and Pinch's (1989) book on health economics is another good social science example. On Monday this all seemed to be idle navel-gazing and too-clever-by-half tricks—as most critics insist (Baber, 1992; Doran, 1989)—but after nearly a week in the library I am starting to see the point. I have also realized that I have visited every level and probably every set of stacks in this library.

SUNDAY: REVIEWS, REFLEXIVE WORK, AND DECISIONS

Last night I had a long talk with Jamie—I had hardly seen her since I started all this. She said that the way I'd been going at it all week—first one thing and then another, and another—was crazy. And that I was looking worn out. She said that if I *insisted* on coming back here tomorrow (today), I should spend the day thinking more clearly about what I'm going to *do* with all this stuff. She's right, of course. At the moment, my project looks set to be a discourse analytical cum rhetorical and historical study of natural and social scientific texts of all possible kinds that is greatly concerned with gender and slightly concerned with science education! This clearly won't work. I think I'm getting depressed again.

In an effort to cheer myself up, I spent the morning reading about reflexivity. I had been thinking about this on and off ever since Monday when I came across that funny book by Mulkay (1985). And looking at all those social science analyses yesterday—and Haraway's cyborgs, Traweek's border crossings, and Star's onions on Friday—brought it to a head. All week, the most interesting things I have read are those that foreground their authors' own involvement in the text. And it seems that the closer one's *topic* is to one's *method*, the more important it becomes to devise some way of coming to terms with the implications of that similarity. In this argument, writing about writing *has* to be a self-consciously circular process and its practitioners must learn to live with the (rhetorical) consequences—such as my own initial negative reaction. The earliest argument on these lines I have found in science studies is in a piece by Latour (1981), though he certainly doesn't advocate writing plays. (Which is odd, come to think of it, given his earlier, 1980, experimental piece written as "a sociologist's nightmare.")

One of the most entertaining pieces of reflexive writing I read was by Trevor Pinch and Trevor Pinch (1988), which masquerades as a critique of reflexivity and "new literary forms" (or "unconventional texts," as the Pinches put it) and yet that does so in the form of a playful and intensely self-referential dialogue. After a discussion of two versions of reflexivity as formulated by Woolgar (1983, 1988c), this is how the two versions of the author (fail to) present Woolgar's third version of reflexivity (admittedly, things do get a bit complicated):

> *By the way, I take it that we do not have to mention here the third form . . . the "benign introspection" version? I know that neither you nor I would ever take that sort of psychological talk seriously.*
>
> Quite—no need to mention that here.
>
> *Good. Back to self-reference.* (Pinch & Pinch, 1988, p. 183)

What this kind of work does, it appears to me, is to put in question some of the most basic and taken-for-granted desiderata of scholarly or academic or scientific writing—such as the distinction between the serious and the nonserious, the important and the trivial. Perhaps, after all, this kind of thing did have something to offer. Perhaps, I too (no less) could learn to play. (But would I be allowed to? Would an outsider be accepted? After all, this kind of stuff did have a rather closed and cliquey feel to it. Where does the *community* end up in all this "hermeneutic hyperconsciousness"—Beer & Martins, 1990a, p. 172?)

After a quick lunch at the pub around the corner, I realized I would have to start at the beginning again so I took a new look at the collection of reviews I had amassed during the week. I began to notice that these texts were highly argumentative even (especially?) when they presented themselves as "innocent" and above the battle. For instance, a general survey of linguistically turned history of science by J. V. Golinski (1990a) rejects the work of Gilbert and Mulkay (1984) by contrasting it with that of another Canonical Footnote member (Shapin & Schaffer, 1985), which is made to stand for the acceptable face of rhetorical analysis: "[Shapin and Schaffer] have used techniques of the rhetorical analysis of scientific discourse, but have not shirked the historian's duty to place that discourse in its historical context" (Golinski, 1990a, p. 120). I love that "but" (which is elaborated by Shapin, 1984, in his own critique of discourse analysis, that is, Gilbert and Mulkay). There's a similar sort of move evident in political critiques of the sociology of scientific knowledge. For instance, Aronowitz (1988b) reviews the field fairly sympathetically but then withdraws, saying that the primacy of discourse is itself historically conditioned. If the use of this kind of move serves to ally Aronowitz with Golinski, their alliance does not last for long—and they fall

out, significantly, on the issue of what their common discipline of history is all about. For Golinski, "history" is what historians do; for Aronowitz, it is what the working class does.

One source—or better, resource—for review rhetoric appears, then, to be disciplinary membership. Though as befits the concept of "resources," accounts and attributions of disciplinarity appear to be highly "occasioned" (I think I'm getting the hang of this). An interesting example of this feature is a pair of reviews (D. McCloskey, 1987; Potter & Wetherell, in press) that come, independently as far as I can tell, to a similar conclusion about a major piece of work in the sociology of scientific knowledge: Collins's (1985) study of the gravity wave controversy.

> Collins . . . writes on rhetoric without knowing it. . . . This is not sociology of knowledge but rhetoric of knowledge. (D. McCloskey, 1987, p. 14 [draft])[2]

> Rhetoric is, arguably, one of [Collins's] major concepts for understanding social life. However, he does not theorise the concept and explore its senses. (Potter & Wetherell, in press: chap. 1, p. 23 [draft]; see note 2)

Both of these examples attempt a reconstruction of Collins's work in alternative disciplinary terms more in line with the reviewers' own concerns. And in both cases, Collins is chided for not already having done this work himself.

In all three examples, we can see that the rhetorical resource of disciplinary membership is used to do what is known as "boundary work" (Gieryn, 1983). But whereas Golinski, as spokesperson for history of science, attempts to exclude inappropriate work, McCloskey and Potter and Wetherell, on behalf of rhetoric of science, use an inclusionary strategy. It is tempting, though doubtless premature, to conclude from these differences in reviewing strategies the existence of concomitant differences in the status of the disciplines concerned—and so on. Of course, this is only a sketch of an outline of a single aspect of what might be involved in a fully fledged analysis of "review rhetoric." I haven't touched at all on other aspects of the construction of authority, let alone the ways that reviewers justify their selections and judgments. Nevertheless, this *could* be just what I've been looking for: an interesting and worthwhile dissertation topic. Perhaps something like "The Rhetorical Structure of Review Discourse."

I'm going to take a break now while I think over what I've done. After all, what do I really know about writing reviews? Surely, if I'm serious about taking on board the reflexive argument, the first thing I would have to do is to write one myself. Now. Hang on. What was it that Jamie mentioned about this new *Handbook of Science and Technology Studies*? Something about a bidding process for all the chapters. But if that's true, then why shouldn't I use my week's diary . . .? No. They'd never accept it. I've got no standing, no credibility, no . . . GOT IT! I'll attribute it to someone—or better, several people—who have. Not anybody whose work I've reviewed, of course. That would be unethical. Yes. Of course. The three absentee (and male!) authors from the Canonical Footnote: in alphabetical order—Ashmore, Myers, and Potter. Perfect! And I'll write it as a new literary form—as a diary of a fictional graduate student's 7 days in the library. Jamie will hate it. "Overcute and pointless," she'll say. I must do some careful work on that. Everything must be justified. So . . .[3]

NOTES

1. See, for instance, Gilbert and Mulkay (1984, p. 194, n. 27); Lynch (1985a, p. 17, n. 2); Schuster and Yeo (1986, p. xxxii, n. 5); Bazerman (1988, p. 156, n. 3); Golinski (1990b, p. 497, n. 11); Ashmore, Myers, and Potter (1995, n. 1).

2. I now find, on going back to the library, that this quote does not appear in the published version of McCloskey's review. Let me explain. My new colleagues—Malcolm, Greg, and Jonathan—were kind enough to let me see the draft of McCloskey's piece (and of Potter and Wetherell's forthcoming book). A pity, though, that they neglected to tell me of the important changes in the published text; perhaps they didn't know themselves? In any case, the evident "illibrarality" of these two texts provides an occasion for two interesting textual manoeuvres: the acknowledgment of debts, as above, and the confession; here, of having resorted, in the interests of realism, to the occasional fiction.

3. We now go back to the beginning—as weeks do, of course.