

BOOK REVIEW

Bruno Latour and Steve Woolgar, Laboratory Life: The Social Construction of Scientific Facts. Sage Library of Social Research, v. 80. Beverly Hills: Sage Publications, 1979, 272 pages. \$7.95

Bruno Latour, a French philosopher, and Steve Woolgar, a British sociologist, present in Laboratory Life an account of how scientific statements are developed in individual laboratories and accepted as fact by scientists beyond that particular laboratory. Working from the data of Latour's two years' observation of daily activity in the Salk Institute for Biological Studies, the two authors explore how laboratory activities are transformed into published statements through what they call a process of "literary inscription," and then how those statements gain "credit" to emerge as accepted fact out of the many contending statements proposed in the literature of the field. The authors also rely on historical case material concerning the isolation and identification of the chemical structure of Thyrotropin Releasing Factor (TRF) during the 1960's; through this case study they show how TRF only takes on "solidity"--recognizable and accepted identity as a substance--in consequence of statements about the substance being accepted as fact.

The questions Latour and Woolgar pursue hold broad interest; their data are suggestive; many of their observations are perceptive; and some of the analysis is promising. But commitment to a strong epistemological position leads them to construct an unnecessarily comprehensive theoretical scheme, a line of argument that obscures important questions, and an attempt to make the data and analyses say more than they do. Despite frequent denials, Latour and Woolgar speak from a radically subjectivist position which, although not necessary for most of their substantive observations, leads them in their framing argument to make a basic error of conflating fact with the statement of fact. As will be discussed below, their desire to show that fact is only a construct of language and exists only in the scientists' adherence to the statement leads the authors to force the argument at several key points.

Latour and Woolgar begin with what they call an anthropological approach, as they describe the strange activities of the tribe residing during working hours in the Salk Institute. But after they identify the aim of the lab's activity and social structure as the production of written documents, their attention, argument, and methodology turns to language. The data are largely objects of language; the analyses are primarily linguistic; and much of the theory derives from the French blend of philosophy and literary analysis, currently localized in the field of semiotics.

The opening anthropological approach calls attention to several important, but taken for granted, features of laboratory structure. The laboratory is divided into two almost equal areas: in one part are apparatus and people in white coats working with the apparatus; in the other part are books, other written materials, the instruments of language (e.g., typewriters and telephones), and people in office dress working with words. Within the apparatus section, physical materials are labelled, measured, and otherwise transformed into signs; for example, extracts are taken from designated rats, labelled, and then placed in a machine which produces a set of figures. These figures are fed into a computer that produces another set of figures, which is converted into a graph. The graph is then delivered to the office section for further transformation into an article. In this process of literary inscription only the most recent symbolic product is attended to at each stage; all the earlier activities, inscriptions, and transformations seem to be forgotten.

This interpretation of laboratory structure and events establishes the research problems of the remainder of the book. Analysis of the variety of statements produced by the laboratory leads to an examination of how statements of contending laboratories are juxtaposed in the literature until a single claim emerges as a solid fact. In the TRF case study articles are shown to be shaped by the discussion to that point and to shape the possibilities for future statement. The questions the articles attempt to answer, the criteria of judgement for new evidence, and even the nature of the substance being looked at change through time as the literature shapes itself in the emergent field. In this competitively--or in the authors' more precise word, "agonistically"--structured field, scientists make statements strategically to advance their own claims, to undermine, or "deconstruct," the oppositions' claims, and even to eliminate or disqualify certain opponents from the field altogether.

A closer look at the microprocesses of laboratory talk follows, for in that talk decisions are made that determine what goes into the article to be published. An analysis of conversation reveals that the talk anticipates the reaction of an audience within the agonistically structured field. Collaborators discuss what they can get away with stating, given the current facts, criteria and competition in their field. Finally Latour and Woolgar set forth an economic model of how scientists rise to positions where they may indeed produce statements that will be taken seriously, literary credibility being the prime unit of exchange.

The substantive program of the book seems to me a fruitful one. As a teacher and student of writing I have become aware that linguistic (or symbolic) choices transform content, that the process of statement creation influences the statement, that anticipation of audience reaction is a key feature of writing, that prior statements help establish the occasion for the new statement, and that a document once written becomes a feature of the landscape. Although conventions of scientific discourse may attempt to limit some of the factors at play in writing, scientific writing cannot be free of the mechanisms of all language and language production. Thus writing, both as a process and a product, should be treated as problematic, and the archive of science should not be viewed as a transparent conveyor of non-symbolic fact. Understanding the what and how of scientific writing could significantly influence what we understand science to be.

And a revised understanding of science is the aim of Latour and Woolgar. They prepare their vision of science in the polemical opening chapter, and they draw it together in the comprehensive scheme of the last chapter. In order to evaluate that frame, however, we need to become clear on what they accomplish in the middle. There they have done less than they suggest. Claims are not established firmly and precisely, and later claims do not clearly derive from earlier claims. Although the authors offer a wealth of evidence, that evidence is not coherently enough drawn together to support the broad and radical conclusions drawn from it.

An intuitive indicator that the argument is not under complete control is the almost constant reference backward and forward to other parts of the book, as though the authors were still mulling over the meaning of their evidence and claims, trying out different lights under which to view their material. They frequently try to draw their argument together in passing summaries, but each time the sum of the argument comes out differently (see pages 40-41, 105, 151, 187, 217, 235 and 236-240). Such casting about for meaning and shape is a necessary exploratory procedure and reveals many interesting insights, but it is not the firm basis for strong conclusions.

The kinds of imprecision that prevent the argument from gaining clear shape are evident in the two analyses of written statements at the end of the second chapter. First the authors categorize the publications of the laboratory according to what they claim to be genre, but actually the categories come from audience analysis: lay audiences, scientists outside the field, specialists not current with latest findings, and insiders (pp. 72-73). Although relationships may exist between genre and audience, they are far from the same; more importantly, analysis of each gives rise to a different set of questions. Genre refers to the conventional forms of language and gives rise to questions concerning the role form takes in sorting out and shaping content. Analysis of genre goes directly to the heart of the issue of what is knowable given our forms of expression--an epistemological question that should be important to Latour and Woolgar, given their larger argument. Analysis of audience leads to the interesting, but lesser, question of how the needs and knowledge of readers influence the presentation and appearance of content, leading to observations of how knowledge is distorted by the pragmatics of communicating with less core audiences. This last they explore lightly.<sup>1</sup>

After three pages they drop their first analysis to consider what they call "statement types." They present a five point scale of how solid or conditional a statement seems to be. This analysis is based on the shrewd observation that the most persuasive statements are those that do not seem to persuade, but which rather rise above the momentary context of statement to appear as the transparent conveyor of fact. "A text or statement can thus be read as 'containing' or 'being about a fact' when readers are sufficiently convinced that there is no debate about it and the processes of literary inscription are forgotten" (p. 76). Calling attention to the process of inscription, such as mentioning the particularities of a lab or the context of debate in which the statements is made, tend to undermine the statement's claim to general truth. Thus Latour and Woolgar define the objective of laboratory activity as "to persuade colleagues that they should drop all modalities used in relation to a particular assertion and that they should accept and borrow this assertion as an established matter of fact" (p. 81). They then give an account of how some laboratory activities serve this end.

The argument and examples of transformation of statement types are enlightening, but again imprecision makes it appear that more has been accomplished than actually has been. First these types are presented only as the results of an impressionistic examination; no systematic investigation is given to indicate that the typology actually distinguishes texts from each other. Further it is not even clear from the discussion whether these categories are meant to reflect a consistent linguistic practice, or they are meant as ways scientists consider other people's work, perhaps reflected in talk, or they are only a plausible heuristic to help explain some events. Second, once the categories are presented they are used only loosely in the following chapters. The TRF case study, which follows immediately after, although presenting many insights of many kinds, is not a rigorous demonstration of the transformation of statement types as the primary mechanism of scientific argument. The idea of the scheme does remain loosely as an element of discussion, but the five distinct categories vanish. Third, calling the categories "statement types" gives the impression that they offer the major distinction to be made among statements; that is, it appears as though the categories get to the root of the issue of statement form. But the categories actually concern only the relatively minor feature of how heavily couched propositions are. More thoroughgoing and consequential issues concern the form and role of the propositions themselves. Thus I could more readily imagine "statement types" applying to the variety of syntactic relations (defining logical relations) in the many sentences of a scientific report (as Gopnik has studied<sup>2</sup>), to the variety

of speech acts consummated by the statements (in the tradition of Austin and Searle, now only first being applied to written texts<sup>3</sup>), or to the variety of roles individual propositions play within the argument of the entire paper. Fourth, no distinction is made here between the many statements made within a single article and the overall statement of the article, if such a univocal statement of an entire text could be said to have definitive existence. This conflation of two levels of statement may be justifiable, but it is certainly problematic enough to require an extensive rationale, missing in this book. In short, Latour and Woolgar do not seem aware of the complexities of the kind of language analysis they wish to pursue: they use pre-contextual, pre-social categories to support large contextual and social conclusions.

Close contact with the evidence of the TRF case study does lead the authors to some striking and admirable observations of how scientific statements are shaped by the conditions of their making, but the neatness of the case study is marred by the fundamental problem of the book. Fact is conflated with statement of fact. To Latour and Woolgar, fact exists only in the minds of scientists who accept particular statements; the authors argue that TRF did not exist prior to its isolation and chemical identification, nor does it exist outside the circle of those familiar with and accepting of its "discovery." I can easily accept that as an item in human consciousness, in the symbolic terms graspable by human consciousness, TRF is constructed, a new item of consciousness; but to ignore that there is a substance out there that precedes our attempt to describe it, to suggest that what we isolate and label are only products of human consciousness, seems a rather naive kind of anthropocentrism. Indeed later Latour and Woolgar present a fancy version of the tree falling unheard in the forest (p. 183), although the authors do feel uncomfortable enough with the argument to qualify its use (see footnote 23, p. 186).

Such a radical position precludes the investigation of one of the more interesting questions about scientific discourse: to what extent and in what way do the conventions and criteria of scientific discourse encourage a grasping towards a reality that exists prior to the words? Or more broadly, in what ways do scientists create a symbolic analogue for nature that moves toward an increasingly good fit between consciousness and that which exists outside consciousness. It would be hard to imagine a science without a commitment to a nature that exists independently of its study, a nature to excite and constrain the imagination, despite science being a human (and therefore a social and historical) activity. Although the human imagination is fertile, would it have sought out, identified, and described TRF unless certain biological and chemical mechanisms suggested the existence of something like TRF? Ludwik Fleck's suggestion of active and passive elements of knowledge offers a much more promising model for the investigation of the commerce between human consciousness and nature, a commerce reflected in scientific texts.<sup>4</sup>

Once having taken the position that science is driven only by the dynamics of its construction with no substantial constraint by things outside consciousness, Latour and Woolgar are moved to show that scientific reality, constructed in daily conversation in the laboratory, is the same as everyday reality, constructed in everyday conversation. After only the sketchiest argument with Garfinkel's distinction between everyday reasoning and scientific reasoning (p. 153), they rest their case on the claim that talk in the laboratory is, like everyday talk, heterogenous. If heterogeneity is to mean anything consequential, it must suggest that the minds and words of all the participants in a conversation are not headed in the same direction at the same time. Heterogeneity could be indicated by differing purposes of different participants, differing interpretations by the participants of the progress of the conversation, shifting planes of

discussion, or a variety of non-harmonious speech acts. But here all Latour and Woolgar show is that laboratory conversation has different referents. At different times scientists talk about four different kinds of things: known facts, activities, theoretical matters, and other researchers (pp. 160-163). The observations that talk in the laboratory seems to revolve around several poles and that these poles are referred to tactically in the course of the communal process of constructing an article are not trivial (although not unexpected, either). But this is far from proving heterogeneity in conversations; by this standard the sentence John recited the second law of thermodynamics. would be heterogenous in itself, because it refers to a person, an activity, and a known fact. The authors have hardly shown that a multiplicity of referents undermines the rationality of the laboratory enterprise. Again the authors have done less than they think they have.

In the final chapter Latour and Woolgar draw together their vision of a science which creates its own reality and of scientists who rise within science. Theirs is a world of making investments, measuring costs, and setting prices, where the currency is all man-made, where the only law of the marketplace is getting away with whatever others will let you get away with. The argument is seductive in the same way a novel is seductive: it begins with a recognizable experience, then gets you so involved in its vision of experience that you no longer notice that experience has been left far behind. Although Latour and Woolgar consider their enterprise part of science, they would not be troubled by this comparison, for they use the language describing fiction to describe both their own work and the work of the laboratory they observe. Their self-consciousness about fictiveness even adds to their abandon, as when they create a fictionalized observer who is allowed to forget things we ordinarily know. Such a tactic may be useful for creating fresh eyes with which to examine the taken for granted things usually overlooked, but at some point the fiction needs to be ended, and the new observations should be set in the context of what we knew originally. But here the fiction is never recalled. Things forgotten are never remembered, and we are lead to the far ends of the fictional observer's speculation--a technique common in the novels of Vladimir Nabokov, who plays with his characters' solipsistic tendencies and who knows the novel is a constructed game to be perpetrated on the reader. Latour and Woolgar make a principle of forgetting in their emphasis on the material left behind at each stage of the process of literary inscription. They do not bother to pay attention to exactly what is carried on to the next stage; they are not interested in defining what traces of reality might remain in the final statement nor what forms the traces might take.

The limited accomplishments of this book do indicate that much can be gained by treating the language and texts of science as problematic, and some of the book's excesses may be forgiven on the basis of the headiness of that discovery. But as the book's shortcomings indicate, great care needs to be taken in applying linguistic and literary analyses to scientific discourse. Most language studies have developed in response to texts and utterances that have little responsibility to reality; novels, poems, literary essays, and even the talk necessary to get through the social world each have their separate commitments, but none requires a representation of nature that goes beyond verisimilitude. Scientific discourse does in some ways resemble other forms of discourse--and those resemblances should be explored--but a commitment to ever more precise descriptions of ever more closely watched realities distinguishes scientific discourse from other forms of discourse. Since language is constructed, in analysing it we must attend to those commitments around which it is constructed. A science of the language of science needs to take seriously the idea that language can create an analogue, no matter how imperfect and mediated, for real things outside the speaker's

imagination. Until stronger evidence than this book comes in, it is plausible to believe that a non-fiction is possible

Notes

I would like to thank Fred Baumann and Larry Stern for their comments and criticisms. I of course take responsibility for all errors and opinions.

<sup>1</sup>For a more satisfactory discussion of the effect of various audiences on the shaping of scientific knowledge, see Ludwik Fleck, Genesis and Development of a Scientific Fact, trans. Fred Bradley and Thaddeus J. Trenn, ed. Thaddeus J. Trenn and Robert K. Merton (Chicago: The University of Chicago Press, 1979), pp. 112-125.

<sup>2</sup>Myra Gopnik, Linguistic Structure in Scientific Texts (Amsterdam: Mouton, 1972).

<sup>3</sup>J.L. Austin, How To Do Things With Words (Cambridge, Mass.: Harvard University Press, 1962); and John R. Searle, Speech Acts: An Essay in the Philosophy of Language (Cambridge, England: Cambridge University Press, 1969).

<sup>4</sup>Ludwik Fleck, op. cit.

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Because I knew that Laboratory Life reflected several years of field work, I ordered it eagerly as soon as I saw it announced. What I received and read, however, was not a research report. It was also not an ethnography, although it claimed to present an "anthropology of science." Laboratory Life is instead a grasping toward a statement of the phenomenological approach to the social study of science. I do not think that this book will be the programmatic statement of the approach: it is too undisciplined to serve that function. But I think that it may turn out to be an exemplar, a conglomeration of theory, method, and observation which will form a rich source of insight, in a fuzzy way, for much future work.

As I understand the phenomenological approach, it claims that a scientist's work is in essence a process of symbolic interaction with nature and with other scientists, the purpose of which is to make claims about the former. The organizing question of Latour and Woolgar's work is "How do scientists come to label certain of their claims about nature as facts?" Their specific concern is with how this labelling happened in the Salk Institute in several cases. In the course of their discussion, the authors make it clear that most of the claims about nature advanced and discussed by the scientists they observed were not treated as facts, but only as potential facts. For observations to become facts, a process of consensus-formation had to take place within and outside the laboratory. The consensus thus formed through interaction was also dissolved in the same way in certain instances. The authors