Talking History: Reflections on Discourse Analysis

is unsystematic and infrequently subjected to close examination, and it usually remains almost entirely hidden from the analyst and his audience. When we talked informally to historians about this, they often replied by referring to their own craft skills. One implication of this paper is that it may be necessary for historians to attend more explicitly to the way in which their craft is exercised in compiling histories from the interpretative work of participants.

We believe that this suggestion will be found to be most convincing by those readers who are led to scrutinize their own current and not yet fully interpreted historical or sociological material in the light of our discussion and who are thereby made more sensitive to the highly variable interpretative work performed by their subjects. The pages above, then, are offered not as proving a case, but as identifying a series of issues worthy of consideration by historians as well as by sociologists of science.

In summarizing the implications for sociology of material similar to that presented here, we have argued that sociologists should pay much more attention to the nature of scientists' discourse instead of trying, with but little success, to use that discourse as the empirical basis for their own supposedly definitive versions of what is going on in science. The conclusions of this paper suggest that a historian's account will remain merely one among many plausible accounts. We are not suggesting, of course, that historians should cease constructing their own historical accounts; but rather, that they should take up the supplementary goal of describing and documenting the various repertoires and interpretative devices used by participants and, perhaps, of trying to explain how different repertoires and devices come to be adopted in different social settings and in different historical periods.

Talking History: Reflections on Discourse Analysis

By Steven Shapin*

> His writing is not about something;
> *it is that something itself.*
> —SAMUEL BECKETT on James Joyce

Gilbert and Mulkay declare above that historians can go about their usual business, but "that they should take up the supplementary goal of describing and documenting the various repertoires and interpretative devices used by participants and, perhaps, of trying to explain how different repertoires and devices come to be adopted in different social settings and in different historical periods." This is a salutary recommendation, although it would have been of

* Science Studies Unit, Edinburgh University, Edinburgh EH8 9JT, Scotland.

greater assistance had they exemplified their preferred practice using materials historians encounter more routinely than interview transcripts.[1] Scientists do talk in different ways in different contexts: in published work, laboratory notebooks, after-dinner speeches, and in interviews by sociologists. Talk is not just an account of behavior and belief; it is itself behavior that varies according to audience and purpose. It would be a great mistake for historians to give a privileged status to talk in any one setting. No type of talk, taken by itself, offers the historian direct access to historical reality. Gilbert and Mulkay are to be thanked for reminding us of this fact.

This, then, is Gilbert and Mulkay's "inclusive" program of discourse analysis: in addition to our usual historical projects we should describe and explain the situated variability of scientists' talk. If this were all that was implied, there would be nothing with which to quarrel. Unfortunately, this is not all. Gilbert and Mulkay have produced a series of other papers over the past few years in which they recommend something more radical and restrictive. It is entirely legitimate to discuss the "restrictive" program here, for the present paper cites without comment or correction the work advocating that program of research. Far from saying that historians should *also* examine talk, Gilbert and Mulkay's restrictive program says that we must *only* examine talk. Their "supplementary goal" of describing and explaining scientists' variable talk indicates one direction of work; in their restrictive program the goal of describing and explaining action and belief is said to be chimerical.[2]

Gilbert and Mulkay's restrictive program of discourse analysis raises issues of fundamental importance to historians and sociologists of science. It contains four elements: (1) an account of the existing practice of historians and (other) sociologists; (2) a diagnosis of irremediable flaws in that practice; (3) a statement that discourse analysis does not suffer from these defects; and (4) a claim to originality in the treatment of scientists' talk. I will very briefly indicate that all these elements contain mistakes.

1. In Gilbert and Mulkay's opinion all other "analysts" (historians and sociologists) seek to " 'tell it like it is' " in science; to say "*this is the way things actually happen*[*ed*]"; to offer " 'definitive accounts of scientists' actions and beliefs' "; to produce the "one best version" of scientific episodes.[3] One won-

[1] Others have argued that certain pictures of the nature of science cannot be drawn without "participant observation"; see, e.g., H. M. Collins, "Understanding Science," *Fundamenta Scientiae*, 1981, *2*:367–380; but even Collins concedes that certain examples of historical practice can approach the understanding afforded to the participant observer; see p. 380.

[2] Many people count inconsistency as a criticism, invalidating all the statements involved. That is not my position. Inconsistency can always be repaired in principle; I am asking that it be repaired in practice, so that historians can know what it is they are being offered.

[3] These, and similar, locutions (emphases and quotation marks preserved) appear in almost all of Gilbert and Mulkay's discourse analysis papers; see, e.g., Michael Mulkay and G. Nigel Gilbert, "What is the Ultimate Question? Some Remarks in Defence of the Analysis of Scientific Discourse," *Social Studies of Science*, 1982, *12*:309–319, on pp. 310–311; Mulkay and Gilbert, "Accounting for Error: How Scientists Construct Their Social World when They Account for Correct and Incorrect Belief," *Sociology*, 1982, *16*:165–183, esp. p. 181; Gilbert and Mulkay, "Contexts of Scientific Discourse: Social Accounting in Experimental Papers," in *The Social Process of Scientific Investigation*, ed. Karin D. Knorr, Roger Krohn, and Richard Whitley (Sociology of the Sciences Yearbook 4, 1980) (Dordrecht: Reidel, 1981), pp. 269–294, esp. pp. 269–270; Gilbert and Mulkay, "Warranting Scientific Belief," *Soc. Stud. Sci.*, 1982, *12*:383–408, on p. 383; Mulkay, "Action and Belief or Scientific Discourse? A Possible Way of Ending Intellectual Vassalage in Social Studies of Science," *Philosophy of the Social Sciences*, 1981, *11*:163–172, on pp. 163–164; Mulkay and Gilbert, "Sci-

ders whether *anyone* believes that only "one best version" of matters can be offered: I can point to an animal and say "pig," and I can point again and say "pink hairy beast" or "abomination." I do not think any of these versions is "the best." So far as Gilbert and Mulkay's view of *historians'* work is concerned, it is either trivially true or fundamentally false. We do not label our accounts as imaginative fictions because we hope they are not. Our goal is indeed telling it *wie es eigentlich gewesen.* If this is what Gilbert and Mulkay mean by seeking to offer "definitive accounts," we should be proud to plead guilty. However, having this goal does not commit us to being simple-minded about how it is realized or about the nature of the accounts that are its end products. Historians routinely accept that their accounts are theoretical and interpretative in character, that their adequacy or inadequacy depends upon historians' purposes, and that, consequently, they may say as much about us as about the historical past. E. H. Carr has surely supplanted Leopold von Ranke in historians' favor.[4] Gilbert and Mulkay portray the historian as a methodological naïf, but they offer no evidence to support this view.

2. What is supposed to be wrong with seeking to provide "definitive" accounts? According to Gilbert and Mulkay, the problem arises from the real variability of scientists' talk. What the historian or (other) sociologist does is to select and validate one form of talk and to say "*this is the way things actually . . . happened.*" As Mulkay says, "The analyst's underlying methodological procedure here is to accept statements at face value if they occur often enough."[5] This would indeed be a devastating indictment of existing practice if it accurately described what historians do. But it does not. It is routine, if not universal, for historians to note variability in scientists' accounts, to compare one sort of utterance with another and with the overall pattern of scientists' verbal and nonverbal behavior.[6]

Gilbert and Mulkay are, however, right about one thing: historians *do* exhibit some of their subjects' statements as especially revealing. For example, the historian may have a *theory* about a scientist's aims in which exhibiting the scientist saying "this was my aim . . ." counts as evidence. But Gilbert and Mulkay are wrong if they think that this is a case of taking the scientist's words "at face value." It is not, because it is the result of interpretative work, providing a framework and justifying the special status accorded to the words. Moreover, the cited utterances are not selected because of their "frequency," but because

entists' Theory Talk," *Canadian Journal of Sociology,* 1983, *8*:179–197, on pp. 181, 195; and Gilbert and Mulkay, "Experiments are the Key," *Isis,* 75:105–125, on pp. 107, 123, 124.

4 Edward Hallett Carr, *What is History?* (New York: Vintage Books, 1961), esp. Ch. 1.

5 Mulkay, "Action and Belief or Scientific Discourse?" pp. 164, 168; cf. Gilbert and Mulkay, "Warranting Scientific Belief," p. 384.

6 Here is a short list of such historical work: P. B. Wood, "Methodology and Apologetics: Thomas Sprat's *History of the Royal Society,*" *The British Journal for the History of Science,* 1980, *13*:1–26; Martin J. S. Rudwick, "Charles Darwin in London: The Integration of Public and Private Science," *Isis,* 1982, *73*:186–206; Dorinda Outram, "The Language of Natural Power: The 'Eloges' of Georges Cuvier and the Public Language of Nineteenth Century Science," *History of Science,* 1978, *16*:153–178; Owen Hannaway, *The Chemists and the Word: The Didactic Origins of Chemistry* (Baltimore: Johns Hopkins Univ. Press, 1975); Nicholas Fisher, "Avogadro, the Chemists, and Historians of Chemistry," *Hist. Sci.,* 1982, *20*:77–102, 212–231; J. R. R. Christie and J. V. Golinski, "The Spreading of the Word: New Directions in the Historiography of Chemistry 1600–1800," *Hist. Sci.,* 1982, *20*:235–266; James R. Jacob, *Henry Stubbe, Radical Protestantism and the Early Enlightenment* (Cambridge: Cambridge Univ. Press, 1983).

of their explanatory significance. The historian might well regard the most frequently encountered utterances as an unreliable account of a scientist's aims.

3. Gilbert and Mulkay's strictures do not apply to any one type of historical practice: to "externalism" versus "internalism," to the sociology of knowledge versus the history of ideas.[7] They apply to all descriptive and explanatory enterprises whatsoever, to everyday speech, and, presumably, to the observation reports of the natural sciences. "It is simply impossible to produce definitive versions of scientists' actions and beliefs"; all that existing approaches to understanding have yielded is "an analytical impasse" and an "Analytical Tower of Babel." What "*wise* man," Gilbert and Mulkay ask, "does not retreat from" this "impasse"?[8]

Our release from this "impasse" is, according to Gilbert and Mulkay, their restrictive program of discourse analysis: not as a "supplementary goal" (as they say in the present paper), not as just another thing historians might do, but as a total replacement of all existing approaches to understanding science: "My formulation," Mulkay says,

> does not present scientific discourse as just another topic to be covered in this area. The analysis of discourse is being presented as an alternative to the more traditional concern with describing and explaining action and belief. . . . [T]he traditional objective of describing and explaining what really happened has been abandoned and replaced with an attempt to describe the recurrent forms of discourse.[9]

The inconsistencies between restrictive and inclusive programs seem irreparable. In apparently more recently written papers, however, Mulkay and his collaborators present yet another claim: the analysis of discourse is to have "methodological" or "analytical" *priority* over traditional exercises; one has to do discourse analysis "at least initially."[10] This position only confuses matters, for Mulkay *et al.* do not offer any way of getting from their restrictive program back to description and explanation of science. Moreover, there is no logical way of doing this except by the abandonment of the restrictive program.

Gilbert and Mulkay's restrictive program seeks to replace "why-questions" with "how-questions." "Instead of asking: What is really going on in science?

[7] For reasons possibly having to do with the local topography of the sociology of science in Britain, discourse analysts have preferred to attack "externalist" or "interest explanation" empirical studies. Still, the fully general nature of their criticisms of description and explanation is stipulated; see, e.g., Steve Woolgar, "Interests and Explanation in the Social Study of Science," *Soc. Stud. Sci.*, 1981, *11*:365–394; and Steven Yearley, "The Relationship between Epistemological and Sociological Cognitive Interests: Some Ambiguities Underlying the Use of Interest Theory in the Study of Scientific Knowledge," *Studies in History and Philosophy of Science*, 1982, *13*:353–388. Both of these condemn the work of Donald MacKenzie, especially his "Statistical Theory and Social Interests: A Case Study," *Soc. Stud. Sci.*, 1978, *8*:35–83. For replies to Woolgar, see Barry Barnes, "On the 'Hows' and 'Whys' of Cultural Change," *Soc. Stud. Sci.*, 1981, *11*:481–498; and Donald MacKenzie, "Interests, Positivism and History," *ibid.*, 498–504; and, for a response to Yearley, see MacKenzie, "Reply to Steven Yearley," *Stud. Hist. Phil. Sci.*, in press.

[8] Mulkay, "Action and Belief or Scientific Discourse?" p. 169; Mulkay and Gilbert "What is the Ultimate Question?," p. 310; cf. Mulkay and Gilbert, "Scientists' Theory Talk," pp. 181–182.

[9] Mulkay, "Action and Belief or Scientific Discourse?" pp. 163, 170.

[10] Mulkay and Gilbert, "Joking Apart: Some Recommendations Concerning the Analysis of Scientific Culture," *Soc. Stud. Sci.*, 1982, *12*:585–613, esp. pp. 588–589, 610; Michael Mulkay, Jonathan Potter, and Steven Yearley, "Why an Analysis of Scientific Discourse is Needed," in *Science Observed: Perspectives on the Social Study of Science*, ed. Karin Knorr-Cetina and Michael Mulkay (London: Sage, 1983), pp. 171–203, on pp. 195–196, 200.

we should try asking: How do scientists construct their versions of what is going on in science?" This "more modest and more answerable" question "must be given analytical priority." We cannot know action and belief *through* talk; all we can do is to "reflect upon" the "patterned character" of talk itself. They say that the discourse analyst "is no longer required to go beyond the data."[11] This is the crucial point: discourse analysis is offered as a form of theory- and interpretation-free historical and sociological practice.

Of course, in reality Gilbert and Mulkay are just like the historians and sociologists they condemn: they too "go beyond the data." Gilbert and Mulkay's work is constitutively interpretative. As Collins has already noted, scientists' talk is "no more or less transparent than any other data."[12] Take any quotation from Gilbert and Mulkay's present paper. What is it that makes it visible as an instance of "empiricist" or of "contingent" accounting? Try the experiment of reading their quotation 3, which Gilbert and Mulkay offer as "empiricist" talk, as riddled with "contingent" locutions. This reading is perfectly possible: the "contingencies" might involve what scientists know about the mentioned biochemists, what significance is accorded to their "working on their own," being "largely ignored," and so forth. I do not argue that Gilbert and Mulkay's interpretations are wrong, only that they *are* interpretations and not pure data. Discourse analysis is not, in this respect, qualitatively different from the sorts of practice Gilbert and Mulkay's restrictive program criticizes. Their recommended way out of our alleged "analytical impasse" cannot be travelled.

4. Finally, we have the claim that discourse analysis is a radically new initiative in the study of science. Gilbert and Mulkay say that their approach is "unusual," that is "presages a major conceptual change," that it is an alternative "to almost all prior work."[13] Priority is not important. What is important is the availability of empirical studies from which historians might benefit. So far as Gilbert and Mulkay's restrictive program is concerned, one might well concede their claims to novelty and priority, although, as I have argued, their practice belies their recommendations not to "go beyond the data." So far as the more inclusive and liberal program of describing and explaining variable talk is concerned, Gilbert and Mulkay's claims to originality are hardly warranted. There are a number of such studies in the empirical literature, including a fine example by Mulkay himself. In that paper Mulkay analyzed scientists' talk about *norms,* explaining its variation in different contexts in terms of scientists' interests and audiences. This paper, presumably now repudiated by its author, represents a model of how attention to scientists' talk can be fruitfully integrated with the traditional historians' goals of describing and explaining action and belief.[14]

[11] Mulkay and Gilbert, "What is the Ultimate Question?" pp. 310, 314–315 (cf. Mulkay, Potter, and Yearley, "Why an Analysis of Scientific Discourse is Needed," p. 200.)

[12] H. M. Collins, "An Empirical Relativist Programme in the Sociology of Scientific Knowledge," in *Science Observed,* ed. Knorr-Cetina and Mulkay, pp. 85–113, on p. 102. Readers are referred to Collins' perceptive remarks on this subject (esp. pp. 101–104).

[13] Mulkay and Gilbert, "Accounting for Error," p. 182; Mulkay "Action and Belief or Scientific Discourse?" p. 163; see also Mulkay and Gilbert, "What is the Ultimate Question?" pp. 310, 312; Mulkay and Gilbert, "Scientists' Theory Talk," p. 194.

[14] M. J. Mulkay, "Norms and Ideology in Science," *Social Science Information,* 1976, *15*:637–656. This paper is summarized in Mulkay's *Science and the Sociology of Knowledge* (London: George Allen & Unwin, 1979), pp. 112–113, along with a series of empirical explanatory studies of the type he now regards as failures (Ch. 4). For exemplary treatments of scientists' talk in different

The historiographic issues involved in the analysis of talk are not trivial. Scientists' talk constitutes a major portion of historians' evidence, and a more reflective attitude to our handling of that evidence can only be a good thing. I have shown that, in practice, Gilbert and Mulkay offer historians not one but two programs for treating talk. Their restrictive program, if accepted, involves a radical narrowing of historians' goals and procedures. The more inclusive program can be, and has already been, assimilated into our normal range of descriptive and explanatory projects. The attractions of the inclusive program are already evident in historians' practice; the weaknesses of the restrictive program are ones of principle. It is therefore scarcely surprising that they have not been counterbalanced by any significant concrete achievements.

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forums, see H. M. Collins and T. J. Pinch, "The Construction of the Paranormal: Nothing Unscientific is Happening," in *On the Margins of Science: The Social Construction of Rejected Knowledge*, ed. Roy Wallis (Sociological Review Monograph, No. 27) (Keele, Staffs.: Keele University, 1979), pp. 237–270; idem, *Frames of Meaning: The Social Construction of Extraordinary Science* (London: Routledge & Kegan Paul, 1982), esp. pp. 109–110, 121–125; also Steve Woolgar, "Writing an Intellectual History of Scientific Development: The Use of Discovery Accounts," *Soc. Stud. Sci.*, 1976, *6*:395–422; Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (London: Sage, 1979), pp. 75ff.; and, for an analysis of scientists' rhetorical techniques for imputing bias, Steven Shapin, "The Politics of Observation: Cerebral Anatomy and Social Interests in the Edinburgh Phrenology Disputes," in *On the Margins of Science*, ed. Wallis, pp. 139–178. See also works cited in note 6.