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CRITIQUES & CONTENTIONS

The Theory of Practice and the Practice of Theory: Sociological Approaches in the History of Science

*By Jan Golinski**

EIGHT YEARS AGO Steven Shapin began his influential paper "History of Science and Its Sociological Reconstructions" with the provocative assertion, "One can either debate the possibility of the sociology of scientific knowledge or one can do it."[1] What followed was a forceful and (in the view of many readers) successful attempt to counter the view that sociologically inspired research in the history of science was a fundamentally misconceived enterprise. Attacking such views with enthusiasm, Shapin displayed the considerable range of achievements that the field could already claim and discussed a number of areas in which further contributions could be expected. He balanced theoretical analysis of the central historiographical problems with summaries of a large number of relevant empirical studies.

In the years since it was published, Shapin's paper has been accorded frequent citations, a testimony both to its lasting value as a survey and to the fact that practitioners of the approach he recommended have continued to get on with the job. Since 1982 the body of historical work that draws upon insights provided by the sociology of science has grown significantly. During a period when the traditional links between the history and the philosophy of science seem to have been weakening, newly established connections between history and sociology are broadening and strengthening. Shapin's own continuing work has been central to this development, and he has been joined by a number of other influential historians. The importance of the relationship between the two disciplines has also been recognized by sociologists.

In view of the continuing fertility of this relationship, it seems timely to review some of the conceptual resources that the sociologists have provided and the success with which they have been applied to historical studies. What follows is not an attempt to replace Shapin's survey, but an effort to identify some of the

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[1] Steven Shapin, "History of Science and Its Sociological Reconstructions," *History of Science,* 1982, *20*:157–211. Other useful surveys of work in this field include Richard Whitley, "From the Sociology of Scientific Communities to the Study of Scientists' Negotiations and Beyond," *Social Science Information,* 1983, *22*:681–720; and Harriet Zuckerman, "The Sociology of Science," in *Handbook of Sociology,* ed. Neil J. Smelser (Newbury Park, Calif.: Sage, 1988), pp. 511–574.

leading characteristics of the work that has emerged since he wrote. It will be noted that historians have not been passive partners in this dialogue but, rather, have sought to modify and refine sociological perspectives in the course of applying them to empirical research. It seems likely that the relationship will continue to involve a two-way exchange of conceptual vocabulary and mutual discussions of crucial case studies.

I

A development of central importance in cementing the connections between historical studies and sociological research has been the emergence of a common interest in experimental practice. In his review Shapin called for more studies in this area, noting that historians of science had shown surprisingly little interest in how experiments are actually done. Sociologists, on the other hand, had been publishing descriptive accounts of contemporary experimental work that offered resources of considerable potential value to historians. Shapin pointed to the work of H. M. Collins, Andrew Pickering, Trevor Pinch, and others who had pioneered this kind of analysis. In tackling the practices of scientists at work, these researchers could be said to have transcended the limits traditionally regarded as circumscribing sociological investigations; they were dealing with the "content" of science and not just with its institutional "context." By concentrating on the observable features of scientific practice they had exposed the interpretive and contingent nature of experiment, bringing to light the rich variety of elements that enter into the making of scientific facts.

These sociological studies of experimental practice have become more widely known in recent years. Collins, Pickering, and Pinch have all produced well-noticed books and have continued to publish stimulating case studies in a variety of fields of contemporary science.[2] The influence of their analyses can be detected in a number of recent historical works. For example, Steven Shapin and Simon Schaffer's highly regarded book on Robert Boyle and the experimental "form of life" in the seventeenth century relies heavily on Collins's ideas about the fragility of experimental culture and the problems of replication. David Gooding's studies of Michael Faraday and Peter Galison's work on twentieth-century physics show a similar sensitivity to the context-bound nature of experimental endeavor, as do many of the contributions to a recent collection of essays, *The Uses of Experiment*. Summing up this shift away from the isolated study of theory in the history of science, Timothy Lenoir has referred to "a recent movement to reevaluate the relationship between theory and all levels of practice," a symptom, he conjectures, of "a more pragmatic age."[3]

From this more pragmatic perspective, experiment can no longer be regarded

[2] H. M. Collins, *Changing Order: Replication and Induction in Scientific Practice* (London/Beverly Hills, Calif.: Sage, 1985); Andrew Pickering, *Constructing Quarks: A Sociological History of Particle Physics* (Edinburgh: Edinburgh Univ. Press, 1984); and Trevor J. Pinch, *Confronting Nature: The Sociology of Solar Neutrino Detection* (Dordrecht: Reidel, 1986).

[3] Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton, N.J.: Princeton Univ. Press, 1985); David Gooding, " 'In Nature's School': Faraday as an Experimentalist," in *Faraday Rediscovered: Essays on the Life and Work of Michael Faraday, 1791–1867,* ed. Gooding and Frank A. J. L. James (London: Macmillan; New York: Stockton, 1985), pp. 105–135; Gooding, " 'He Who Proves, Discovers': John Herschel, William Pepys and the Faraday Effect," *Notes and Records of the Royal Society of London,* 1985, *39*:229–244; Peter Galison, *How Experiments End* (Chicago: Univ. Chicago Press, 1987); David Gooding,

as an unproblematic link between theory and the realities of the natural world. Considerable interpretive work can be seen to be required, and not just to connect the experimental facts with relevant theories. Even before an experiment can be said to have produced any identifiable "facts," a complex of judgments must be made concerning the nature of the outcome to be expected, the competences of previous experimenters, the reliability of equipment, and so on. These judgments have to be made repeatedly, since no experiment can be regarded as completed after a single trial. A process of continuous tinkering and reckoning, experiment appears in these accounts more like an art or craft than the traditional conception of a science.

Furthermore, as Collins has argued most strongly, experiment is potentially open ended. At no point, in his view, does nature force a particular interpretation upon experimenters. Nor can the choice be made by following rigid rules of "scientific method." The evidence is always both too much to fit within any interpretive scheme, and too little to determine the choice between any number of possible alternative schemes. In principle, critics of a particular experimental claim can always find grounds for challenging some of the supporting assumptions that allowed the interpretation to be made; they can attack the competence of the experimenter, the working of the equipment used, its appropriateness in this particular case, or any number of other circumstances. Controversy can be continued as long as a critic can find the resources to sustain these objections. Nor would an attempt to replicate the original experiment necessarily close the debate, as Collins has shown in a number of fascinating case studies. Sufficient differences between two versions of an experiment could always be found by a critic who wished to deny that a proper replication had been achieved.

Collins's arguments are seen by some philosophers as denying the "rationality" of science and, hence, as implying a skeptical conclusion in which properly grounded natural knowledge would be rendered impossible. More sympathetically, most historians who have taken an interest in the sociological research inspired by Collins see it as licensing a more detailed picture of the factors that enter into the production of scientific knowledge. They see their task as the investigation of the complexity of the situations in which experimental science is practiced, regardless of whether that practice is as rule governed an activity as philosophers would wish. That is not to say that all historians are happy with Collins's account of what the relevant features of the experimental context are. Though all are willing to admit the relevance of interests and assumptions, skills and instrumentation, some see Collins's "relativism" as ignoring material or phenomenal constraints, which limit the possible outcomes of scientific experiments and hence require recognition in historical accounts. This is a complex issue, where epistemological questions mingle with those of historiographical method; and we shall return to it later.

As this work has developed, two complementary tendencies in the orientation of analysis have been noticeable. On the one hand, the focus has been narrowed to the local setting, the laboratory (or, in natural history and the earth sciences, the field trip), where experimental facts first emerge. On the other hand, the ways in which such knowledge is transformed in the course of its communication

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Trevor Pinch, and Simon Schaffer, eds., *The Uses of Experiment: Studies in the Natural Sciences* (Cambridge: Cambridge Univ. Press, 1989); and Timothy Lenoir, "Practice, Reason, Context: The Dialogue between Theory and Experiment," *Science in Context*, 1988, *2*:3–22, on p. 4.

into more public settings (lectures, meetings of scientific societies, published books and journal articles) have also been subjected to investigation. The specialized private space of the laboratory and the public spaces in which science meets its audiences have thus both been opened up for exploration.

Studies of laboratories have drawn inspiration from a variety of philosophical and sociological sources. The pioneering work of Ludwik Fleck, *Genesis and Development of a Scientific Fact* (originally published in German in 1935, and republished in English translation in 1979), has been one stimulus for a new concern among historians with the specific settings in which experimental knowledge is made.[4] Drawing upon his own experience as an immunologist, Fleck's work related the production of experimental facts to the "thought style" (*Denkstil*) characteristic of a particular "thought collective" (*Denkkollectiv*) of researchers. Significantly, his conception of a thought style included practical nonverbal skills of the kind that recent sociologists have also argued are essential to the creation of phenomena in the laboratory. Fleck used the example of the Wassermann test for syphilis to argue that practical experience of working with the techniques characteristic of a certain thought style is a necessary condition for the production of a particular experimental phenomenon.

Arguing along similar lines, the philosopher Ian Hacking has described what he calls the "creation of phenomena" by laboratory techniques:

> To experiment is to create, produce, refine, and stabilize phenomena. If phenomena were plentiful in nature, summer blackberries there just for the picking, it would be remarkable if experiments didn't work. But phenomena are hard to produce in any stable way. That is why I spoke of creating and not merely discovering phenomena. That is a long hard task. . . . Noting and reporting readings of dials—Oxford philosophy's picture of experiment—is nothing.[5]

In this view, experimental science proceeds by constructing phenomena using specialist skills and apparatus in a specific setting, that of the laboratory. The "facts" that it produces, far from being universal, appear initially as a kind of "local knowledge," strongly dependent upon the practices employed in its making. The process of extending scientific knowledge outward from the laboratory into the wider world thus requires the transfer of a "package" comprising the experimental phenomena and the techniques through which they were produced. The philosopher Joseph Rouse has provided a clear and helpful discussion of this subject. He writes: "The local laboratory site turns out to be the place where the empirical character of science is constructed through the experimenter's local, practical know-how. The resulting knowledge is extended outside the laboratory not by generalization to universal laws instantiable elsewhere, but by the adaptation of locally situated practices to new local contexts."[6]

This outlook raises a host of problems deserving of historical research. The

[4] Ludwik Fleck, *Genesis and Development of a Scientific Fact,* ed. Thaddeus J. Trenn and Robert K. Merton, trans. Fred Bradley and Trenn (Chicago: Univ. Chicago Press, 1979); originally published as *Entstehung und Entwicklung einer wissenschaftlichen Tatsache: Einführung in die Lehre vom Denkstil und Denkkollectiv* (Basel: Benno Schwabe, 1935). See also the important collection of documents and studies in Robert Cohen and Thomas Schnelle, eds., *Cognition and Fact: Materials on Ludwik Fleck* (Boston Studies in the Philosophy of Science, 87) (Dordrecht: Reidel, 1986).

[5] Ian Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge Univ. Press, 1983), p. 230.

[6] Joseph Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Ithaca, N.Y.: Cornell Univ. Press, 1987), p. 125.

establishment of laboratory cultures in the various sciences, the inculcation of new skills and practices therein, and their connection with instrumentation and the production of new phenomena all cry out for investigation. Studies that have explored the connections between knowledge-producing practices and their physical settings include those by Timothy Lenoir, Merriley Borell, and Gerald L. Geison on nineteenth-century physiology, J. B. Morrell on nineteenth-century chemistry, and Peter Galison on twentieth-century physics.[7]

The transition of knowledge from private to public spaces also emerges as a crucial problem. Historical research has shown how the relationship between science and its audiences (a traditional focus of interest for the sociology of science) can be reexamined in the light of laboratory studies. A greater awareness of the practices by which knowledge is constructed in the laboratory enables us to see how its transition to the public stage requires reconstruction with rather different kinds of resources. A variety of experimental, representational, and discursive strategies have been shown to be implicated in the creation of a public authority for scientific knowledge. Thus Shapin has examined how Robert Hooke managed the transition of experimental facts from his workshop to the more public space of the meetings of the early Royal Society; and David Gooding has explored a similar process in the work of Michael Faraday at the Royal Institution in the early nineteenth century.[8]

A number of these historical inquiries show the lasting influence of Bruno Latour and Steve Woolgar's *Laboratory Life* (1979).[9] In a lively and highly origi-

[7] Timothy Lenoir, "Models and Instruments in the Development of Electrophysiology, 1845–1912," *Historical Studies in the Physical and Biological Sciences*, 1986, *17*:1–54; Merriley Borell, "Instruments and an Independent Physiology: The Harvard Physiological Laboratory, 1871–1906," in *Physiology in the American Context, 1850–1940*, ed. Gerald L. Geison (Bethesda, Md.: American Physiological Society, 1987), pp. 293–321; Geison, *Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late Victorian Society* (Princeton, N.J.: Princeton Univ. Press, 1978); J. B. Morrell, "The Chemist Breeders: The Research Schools of Liebig and Thomas Thomson," *Ambix*, 1972, *19*:1–46; Galison, *How Experiments End* (cit. n. 3); and Peter Galison, "Bubble Chambers and the Experimental Workplace," in *Observation, Experiment, and Hypothesis in Modern Physical Science*, ed. Peter Achinstein and Owen Hannaway (Cambridge, Mass.: MIT Press, 1985), pp. 309–373. Other relevant studies include Owen Hannaway, "Laboratory Design and the Aim of Science: Andreas Libavius versus Tycho Brahe," *Isis*, 1986, *77*:585–610; Larry Owens, "Pure and Sound Government: Laboratories, Playing Fields, and Gymnasia in the Nineteenth-Century Search for Order," *Isis*, 1985, *76*:182–194; and Arleen M. Tuchman, "From the Lecture to the Laboratory: The Institutionalization of Scientific Medicine at the University of Heidelberg," pp. 65–99, and Robert G. Frank, Jr., "The Telltale Heart: Physiological Instruments, Graphic Methods, and Clinical Hopes, 1854–1914," pp. 211–290, in *The Investigative Enterprise: Experimental Physiology in Nineteenth-Century Medicine*, ed. William Coleman and Frederic L. Holmes (Berkeley/Los Angeles: Univ. California Press, 1988).

[8] Steven Shapin, "The House of Experiment in Seventeenth-Century England," *Isis*, 1988, *79*:373–404; David Gooding, " 'Magnetic Curves' and the Magnetic Field: Experimentation and Representation in the History of a Theory," in *Uses of Experiment*, ed. Gooding, Pinch, and Schaffer (cit. n. 3), pp. 183–223; and Gooding, "In Nature's School" (cit. n. 3). See also the papers collected in Terry Shinn and Richard Whitley, eds., *Expository Science: Forms and Functions of Popularisation* (Sociology of the Sciences Yearbook, 9) (Dordrecht: Reidel, 1985), esp. Whitley, "Knowledge Producers and Knowledge Acquirers: Popularisation as a Relation between Scientific Fields and Their Publics," pp. 3–28; and Steven Shapin, "Science and Its Public," in *Companion to the History of Modern Science*, ed. R. C. Olby *et al.* (London: Routledge, 1990), pp. 990–1007.

[9] Bruno Latour and Steve Woolgar, *Laboratory Life: The Social Construction of Scientific Facts* (London/Beverly Hills, Calif.: Sage, 1979); republished as *Laboratory Life: The Construction of Scientific Facts* (Princeton, N.J.: Princeton Univ. Press, 1986). Other pioneering laboratory studies, comparable in some respects in their approaches, are Karin Knorr-Cetina, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science* (Oxford: Pergamon,

nal book these authors recounted how they entered a biochemical laboratory in San Diego, supposedly without preconceptions as to the nature of scientific activity, and proceeded to observe what happened there. The resulting "anthropological" study describes the physical layout of the laboratory and the functions of the personnel and instruments gathered therein. Latour and Woolgar show how the concentrated resources of the laboratory are used to produce claims about nature and persuasively to communicate them to the outside world. These claims emerge initially in the form of "inscriptions"—the visible traces yielded by various kinds of instruments, which can be represented in published papers to support the plausibility of scientists' statements. The laboratory can thus be seen as a kind of factory for inscriptions.

One striking feature of Latour and Woolgar's analysis, which has continued to be central (albeit in different ways) to both authors' subsequent work, is its portrayal of science as a *rhetorical* enterprise. In their view, scientific discourse persuades its audience by mustering sufficient resources (of instruments and the visible inscriptions they produce) to overwhelm opposition. Science is seen as a constant trial of rhetorical strength, a battle across what Latour and Woolgar call the "agonistic field," against opponents who may in fact be silent but whose potential objections always have to be answered.

The analysis of scientific discourse as a rhetorical construction has been pursued in several studies by sociologists and literary specialists. They have examined various genres of scientific writing with the aim of showing in detail how they are constructed so as to persuade particular audiences.[10] Attention has been focused, for example, on the use of narrative devices to convey accounts of experimental discoveries, and on the techniques by which allusion to the external world is conveyed. The assembling and ordering of references to prior research has also been viewed as a component of a strategy of persuasion. Scientific writers choose their citations with evident deliberation, aiming to situate their own work in relation to a selective view of the past and borrowing the authority of certain past claims to support their own.[11]

Some sociologists have proposed that rhetorical analysis is incompatible with the recognized aims of historical (or, for that matter, sociological) explanation. G. Nigel Gilbert and Michael Mulkay have argued that an awareness of the multiplicity of discursive accounts that scientists give of their own actions should make analysts unwilling to accept the validity of any particular account, such as that given by a sociologist. The sociologist's is just one explanation among many,

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1981); and Michael Lynch, *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory* (London: Routledge & Kegan Paul, 1984).

[10] Introductions to this kind of rhetorical analysis are given in W. Weimar, "Science as a Rhetorical Transaction," *Philosophy and Rhetoric,* 1977, *10*:1–29; and Joseph Gusfield, "The Literary Rhetoric of Science," *American Sociological Review,* 1976, *41*:16–34.

[11] Examples of these studies include Steven Yearley, "Textual Persuasion: The Role of Social Accounting in Scientific Arguments," *Philosophy of the Social Sciences,* 1981, *11*:409–435; J. Law and R. J. Williams, "Putting Facts Together: A Study of Scientific Persuasion," *Social Studies of Science,* 1982, *12*:535–558; Greg Myers, "Texts as Knowledge Claims: The Social Construction of Two Biology Articles," *Soc. Stud. Sci.,* 1985, *15*:593–630; and G. N. Gilbert, "Referencing as Persuasion," *Soc. Stud. Sci.,* 1977, *7*:113–122. More comprehensive accounts of scientific rhetoric are given in G. Nigel Gilbert and Michael Mulkay, *Opening Pandora's Box: A Sociological Analysis of Scientists' Discourse* (Cambridge: Cambridge Univ. Press, 1984); and Charles Bazerman, *Shaping Written Knowledge: The Genre and Activity of the Experimental Article in Science* (Madison/London: Univ. Wisconsin Press, 1988).

and "almost every single account is rendered doubtful by its apparent inconsistency with other, equally plausible, versions of events."[12] To this argument, historians and some sociologists have responded that the variation among possible accounts of scientists' actions merely raises the question of appropriate ways of choosing between them; it does not by itself indicate that the choice is entirely arbitrary.[13]

If this line is taken, there seems to be considerable scope for further development of rhetorical analysis and for integrating it into historical accounts. In this regard, it might be particularly illuminating to complement descriptions of strategies of writing with some discussion of the ways in which the texts in question were read by their audiences. Thus analysis of science as rhetoric would be supported by scrutiny of the hermeneutical practices in which readers of scientific texts are engaged.[14] Another potentially fertile field of research comprises the rhetoric of visual imagery in science. A number of investigators have recently begun to consider the role of diagrams, maps, graphs, photographs, and other types of images in creating and communicating a vision of the natural world. The production of images constitutes another respect in which human labor and technique are invested in the construction of a represented reality.[15]

The view of science as a rhetorical activity, one that mobilizes discursive and visual resources toward the aim of persuasion, reappears in Latour's recent book *Science in Action*.[16] This begins in the realm of rhetoric with a discussion of how scientific texts persuade their readers of the factuality of the claims they contain. Latour then considers how a reader might resist the authority of the texts—for example, by questioning the interpretation of the inscriptions represented there.

[12] Gilbert and Mulkay, *Opening Pandora's Box*, p. 11. Compare the papers in Steve Woolgar, ed., *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge* (London/Beverly Hills, Calif.: Sage, 1988); Woolgar, *Science: The Very Idea* (Chichester, Sussex: Ellis Horwood, 1988), esp. pp. 89–95; and Malcolm Ashmore, *The Reflexive Thesis: Wrighting Sociology of Scientific Knowledge* (Chicago: Univ. Chicago Press, 1989).

[13] See, e.g., Steven Shapin, "Talking History: Reflections on Discourse Analysis," *Isis*, 1984, *75*:125–130.

[14] A good example of the integration of an analysis of scientific rhetoric with a description of its historical context is Steven Shapin, "Pump and Circumstance: Robert Boyle's Literary Technology," *Soc. Stud. Sci.*, 1984, *14*:481–520. For a review of this subject see Geoffrey Cantor, "The Rhetoric of Experiment," in *Uses of Experiment*, ed. Gooding, Pinch, and Schaffer (cit. n. 3), pp. 159–180. On hermeneutics see Gyorgy Markus, "Why Is There No Hermeneutics of Natural Sciences? Some Preliminary Theses," *Sci. Context*, 1987, *1*:5–51.

[15] Work on techniques of imagery in science includes Gooding, "Magnetic Curves" (cit. n. 8); Martin J. S. Rudwick, "The Emergence of a Visual Language for Geological Science, 1760–1840," *Hist. Sci.*, 1976, *14*:149–195; Bruno Latour, "Visualisation and Cognition," *Knowledge and Society: Studies in the Sociology of Culture Past and Present*, 1986, *6*:1–40; Michael Lynch, "Discipline and the Material Form of Scientific Images: An Analysis of Scientific Visibility," *Soc. Stud. Sci.*, 1985, *15*:37–66; Lynch and Steve Woolgar, "Introduction: Sociological Orientations to Representational Practice in Science," pp. 99–116, and John Law and Lynch, "Lists, Field Guides, and the Descriptive Organization of Seeing: Birdwatching as an Exemplary Observational Activity," in *Representation in Scientific Practice*, ed. Lynch and Woolgar (special issue of *Human Studies: A Journal for Philosophy and the Social Sciences*, April–July 1988, *11*[2–3]), pp. 271–303; and Lynch and Samuel Y. Edgerton, "Aesthetics and Digital Image Processing: Representational Craft in Contemporary Astronomy," in *Picturing Power: Visual Depiction and Social Relations*, ed. Gordon Fyfe and John Law (*Sociological Review Monograph*, 35) (London: Routledge, 1988), pp. 184–220.

[16] Bruno Latour, *Science in Action: How to Follow Scientists and Engineers through Society* (Cambridge, Mass.: Harvard Univ. Press, 1987). See also Latour's attempt to put his principles into practice in a monograph on Pasteur: Latour, *The Pasteurization of France*, trans. Alan Sheridan and John Law (Cambridge, Mass.: Harvard Univ. Press, 1988); originally published as *Les microbes: Guerre et paix* (Paris: Editions A. M. Métailié, 1984).

In the face of such resistance, the scientist is shown to return to the laboratory, to the instruments from which the inscriptions were derived. To continue to doubt the scientist's claims would then be to call in doubt the apparatus used, in which the trust of many previous investigators has been reposed. By mobilizing the instruments and their inscriptions in support of his or her assertions, the scientist is thus mobilizing the collective authority of colleagues and predecessors.

The move from texts to the laboratory is one that, Latour suggests, must be made by the sociologist of science. To understand how the authority of science is constructed we must pass from an analysis of scientific discourse to an investigation of the laboratory, and then out again to see how the resources of the laboratory are used in the outside world. We have to see science as different from other forms of persuasion in its reliance upon manipulations of the material world, and thus upon the equipment through which those manipulations come to be practiced in a routine way. Whatever rhetorical forms it might share with other kinds of writing, science is distinguished from them in that it also mobilizes other (nondiscursive) resources.

Recognition of this seems essential if historical research is to reap the benefits of the sociologists' insights, particularly if we are to uncover how the "local knowledge" produced in laboratories makes its way in the world outside. Much of Latour's book is concerned to show how this is accomplished by building "networks"—chains of linked persons and things along which the products of laboratories (the phenomena and practices that are boxed together as "facts" or "machines") can pass smoothly. An infrastructure of standardized procedures and instruments must be constructed to transmit the products of science away from the site of their creation. Only by means of these networks can laboratory phenomena be reproduced elsewhere, as the practices that brought them into existence are extended to support them. Thus the world outside is reshaped to resemble the laboratory, in a "gigantic enterprise to make of the outside a world inside which facts and machines can survive."[17]

In Latour's view, the construction of networks is the key to the diffusion of the products of laboratory science. Actors are "enrolled" in networks by processes of "translation" whereby their interests become identified with passing the fact or machine further down the line. In this sense the fate of scientists' products is in the hands of their subsequent users. Things made in the laboratory have no essential or eternal truth independent of the contexts in which they are later put to work. That said, however, Latour gives no account of why actors might choose to *resist* "enrollment" in a particular network, why they might decide not to give their assent to a scientific claim or not to trust a certain machine. In other words, unlike Collins, who has devoted several studies to precisely this issue, Latour has no account of controversy, no way of explaining why certain scientists might disagree and go on disagreeing about the validity of a particular claim.

As Shapin has pointed out, this feature of Latour's perspective constitutes a

[17] Latour, *Science in Action*, p. 251. Compare Fleck's account of the diffusion of the Wasserman test from the laboratory where it was first discovered. Fleck shows how the technique had to be remodeled to make it less sensitive to variations in conditions in the world outside, concluding that the transmission of scientific knowledge "can by no means be compared with the translocation of a rigid body in Euclidean space. Communication never occurs without a transformation, and indeed always involves a stylized remodeling": Fleck, *Scientific Fact* (cit. n. 4), p. 111.

serious deficiency from the point of view of historians, who are likely to continue to want to *explain* the choices of historical actors.[18] Latour's rhetorical view of scientific discourse focuses exclusively on its persuasive character; his military model of the extension of scientific networks makes the outcomes of their "trials of strength" inexplicable on any independent basis. In Latour's world there are just winners and losers, stronger and weaker networks. And yet Collins's studies provoke the question, How can the rejection of scientific claims, by certain individuals in certain circumstances, be explained? As Shapin has argued, by refusing to identify the interests that actors have apart from the networks in which they might be enrolled, Latour rejects one important resource by which he might have explained their decisions.

Paradoxically, a similar charge of ignoring the necessity to explain actors' choices has been made against Collins. In an extensive review of Collins's book *Changing Order* (1985), Andrew Pickering faults the author for offering only unanalyzable contingency as the reason for scientists' decisions to accept or reject particular claims.[19] Collins is so concerned to stress the flexibility that actors have when making their choices, the freedom they always (in principle) retain to go on questioning the assumptions that support any experimental claim, that he is left without a way to explain the decisions they eventually come to. Collins, in Pickering's view, celebrates contingency rather than looking for ways to account for scientists' choices in particular situations.

There can be no doubt that Collins and Latour, from their intriguingly different perspectives, offer many conceptual resources that can be productively applied to historical research. But both ultimately fail to satisfy historians' demands for ways of explaining the actions of scientists in particular contexts. Recently a number of sociologists and historians seem to have realized that more needs to be done in this respect. Various attempts have been made to develop more plausible accounts of scientific practice that recognize both the flexibility and the constraints operating in any specific situation. In general terms, the problem is that of developing a "theory of practice," which would enable scientific work to be related to the aims and resources of its practitioners and to the structure of constraints within which they find themselves. In the attempts that have been made in this direction, it is notable that theory has reappeared as a relevant feature of the scientific endeavor. Theory, it appears, need not be viewed as existing in a realm of ideas apart from practice; nor need it be reduced to its expression in instruments or social relations. Historians and sociologists have been examining ways in which theoretical activity (the "practice of theory") can be studied in its relationship to experimental work. The remainder of this paper will survey some of the prospects for a more cogent and nuanced view of scientific practice offered by this recent research.

[18] Steven Shapin, "Following Scientists Around," review of Latour, *Science in Action,* in *Soc. Stud. Sci.,* 1988, *18*:533–550. Shapin also notes that Latour's "actants" (entities that include both human and nonhuman actors) transcend the categories with which we usually deal in giving historical explanations. Latour clearly does not want his perspective to be easily assimilated by historians, though he may not be able to stop them from appropriating aspects of it with profit.

[19] Andrew Pickering, "Forms of Life: Science, Contingency and Harry Collins," *British Journal for the History of Science,* 1987, *20*:213–221.

II

In order to organize our discussion of this work, it will be useful to keep in mind a terminology first introduced by Ludwik Fleck, namely that of "active" and "passive" elements in the production of scientific knowledge. Active elements comprise those under the control of scientists: their will, desires, and imagination and the skills and techniques that they select to probe the natural world. The passive elements are those beyond their control that they encounter when they feel resistance in the course of investigation. The aim of scientific work is to bring such passive elements within experience, but they can never be encountered independently of practical intervention in the world. All knowledge production comprises an inseparable compound of active and passive elements; the latter can never be detached from the former. This conception lies behind Fleck's definition of a fact as "a stylized signal of resistance to thinking."[20]

Fleck's epistemology connects his work with that of certain hermeneutical philosophers. In hermeneutical philosophy the world is regarded as a realm that is always encountered through practical activity. Before we can know or even perceive things as such, we experience them as being directed toward some purpose or use. Things are perceived as objects when they run counter to our purposes or emerge as "in the way" of what we want to do.[21] Thus the relationship of active intervention in the world, rather than that of detached contemplation of external objects, is fundamental to the production of knowledge.

Fleck's work suggests that this is particularly true for science. Despite some philosophers' persistence in regarding science as no more complex in principle than the knowledge of tables and chairs, it is apparent from studies of scientific practice that epistemological problems arise with more urgency and regularity in science than in everyday life. It is the complexity of the practices through which the external world is encountered in science (the use of instruments, for example) that makes scientific knowledge much more problematic.

Precisely how is the epistemological situation complicated by the use of instruments in science? Latour argues that instruments are rhetorical resources with which scientists reinforce the authority of their assertions. But Collins's studies suggest the possibility that some dissenters might resist this rhetorical onslaught by bringing the use of the instruments into question. Trevor Pinch has captured the difference between these two situations by talking about the "black-boxing" of instruments. In a situation where scientific claims are controversial, the instruments used to make them may have their validity challenged. For example, in the controversy studied by Pinch, those who denied Raymond Davis's claims to have detected a deficiency in the solar neutrino flux were simultaneously questioning the adequacy of his instrument as a tool for measuring that flux. In this case the reliability of the instrument is as controversial as the phenomenon supposedly detected by its use. On the other hand, when phenomenal claims become accepted, the relevant instruments have their validity enhanced. They gradually

[20] Fleck, *Scientific Fact* (cit. n. 4), pp. 94–95, 98.

[21] This perspective, which derives initially from the philosophy of Martin Heidegger, is discussed illuminatingly in Rouse, *Knowledge and Power* (cit. n. 6), esp. Chs. 3, 4. See also Robert Allen Goff, "Wittgenstein's Tools and Heidegger's Implements," *Man and World*, 1968, *1*:447–462.

become black-boxed—taken as unproblematic means of access to a particular realm of phenomena. It is not impossible that instruments that have been black-boxed may be opened up again in subsequent controversies, but clearly this would be at the cost of disturbing what is now established knowledge.[22]

Those who take a "constructivist" position in sociology of science argue that scientists' decisions (to challenge a claim made by others or to open up a black-boxed instrument, for example) can be explained by reference to various active elements of their situation. That is to say, actors' choices are constrained by their aims or interests and by the resources they select to advance them. This constructivist view is not identical with what used to be called "externalism," as Shapin points out in the article cited at the beginning of this essay. He argues, for example, that scientists might be shown to be guided by a complex of skills and technical competences that "represent a set of vested social interests *within* the scientific community."[23] Andrew Pickering s study *Constructing Quarks* presents many examples of this from the recent history of high-energy physics. Pickering regards physicists as guided by an attitude of "opportunism in context"; they make the decisions they do because they seek to employ their particular specialist skills through developing new areas of work. In this case some physicists' investment in *theoretical* skills is seen as a very relevant constraint on the development of scientific practice. In fact, Pickering ascribes the peculiar dynamism of high-energy physics in recent years to a "symbiotic" relationship between its theoretical and experimental communities.[24]

However, defenders of the constructivist line have been criticized for ignoring the role of passive elements, which constrain the production of scientific knowledge in ways that are beyond the control of those involved. Peter Galison argues that such elements must be incorporated into historians' accounts in order to explain how experiments may have unexpected results that apparently cannot be renegotiated by those who conduct them. In *How Experiments End* he describes the work of a team at the National Accelerator Laboratory at Batavia, Illinois, in 1973, which contributed to the discovery of "neutral currents." This finding contradicted the group's earlier expectations and apparently thwarted their interest in opposing similar claims made by rival groups. And yet by December 1973 one of the team leaders was writing, "At present I do not see how to make these effects go away." However "constructed" the experiment was, and however social the processes of negotiation by which its result was hammered out, this experience of passive constraint was powerfully felt by the experimenter. For this reason, it surely deserves a place in the historian's narrative.[25]

The implication of this kind of incident, Galison argues, is that the indefinite renegotiability of experimental results, which exists "in principle," should not be

[22] Trevor Pinch, "Towards an Analysis of Scientific Observation: The Externality and Evidential Significance of Observation Reports in Physics," *Soc. Stud. Sci.*, 1985, *15*:3–36; and Pinch, *Confronting Nature* (cit. n. 2).

[23] Shapin, "History of Science" (cit. n. 1), pp. 164–169.

[24] Pickering, *Constructing Quarks* (cit. n. 2), esp. pp. 187–195, 403–414.

[25] Galison, *How Experiments End* (cit. n. 3), Ch. 4, esp. pp. 234–241. See also the remarks on Galison's view compared with Pickering's in Ian Hacking, "Philosophers of Experiment," pp. 147–156, and Peter Galison, "Multiple Constraints, Simultaneous Solutions," pp. 157–163, both in *PSA 1988: Proceedings of the 1988 Biennial Meeting of the Philosophy of Science Association*, Volume II: Symposia and Invited Papers, ed. Arthur Fine and Jarrett Leplin (East Lansing, Mich.: Philosophy of Science Association, 1989).

the primary factor in framing the historian's account. Although it might always be possible to maintain skeptical doubts about any experimental claim by bringing more of its underlying assumptions into question, in practice judgments are made and conclusions agreed. The historian's task is to explain how such practical judgments are achieved.

A similar point has been made by Yves Gingras and Silvan S. Schweber in a review of Pickering's book. Criticizing Pickering's frequent resort to a certain rhetorical trope to expose the contingency of physicists' choices, these reviewers write: "To state regularly that physicists *could* have made another choice than the one they actually made is not an *argument,* and it does not explain why they have not done so. Neither do such statements imply that the only explanation for their choice lies in the need for social cohesion and opportunity for further research."[26] Social circumstances are admittedly important, but Gingras and Schweber consider that scientists' choices cannot be fully explained without also admitting the influence of phenomenal and material constraints. This does not amount to asserting that such constraints can be encountered independently of socially located practices, nor does it entail doing what Pickering forcefully condemns, namely, "putting the phenomena first."[27]

Gingras and Schweber support their claim with a critique of the constructivists' use of arguments for the "theory-ladenness of observation." Granted that observations are indeed shaped by prior expectations and by beliefs about the capacities of instruments, these writers argue that this does not open the way to indefinite interpretive flexibility. In particular, one must keep in mind that problematic phenomena will typically be approached by more than one type of instrumentation and that instruments will be adjusted over the course of time so that a consistent and stable representation emerges. Any of these procedures might be contested, of course, as Collins for one has pointed out, but in most cases they do enable phenomena to appear through being implicated in a variety of instrumental practices.[28]

Having appeared, the phenomena are then usually put to work in a number of theoretical contexts. As Timothy Lenoir has argued, the way in which phenomena become connected with a range of theoretical realms enables them gradually to become more entrenched in accepted knowledge.[29] In this respect, the much-touted "Duhem-Quine thesis" cuts both ways: phenomena are indeed interpreted in relation to a network of assumptions and beliefs; but it is precisely by becoming entrenched within those networks that they are experienced as passive constraints upon the production of subsequent knowledge.[30]

[26] Yves Gingras and Silvan S. Schweber, "Constraints on Construction," *Soc. Stud. Sci.,* 1986, *16*:372–383, on p. 379 (italics in original).

[27] Andrew Pickering, "Against Putting the Phenomena First: The Discovery of the Weak Neutral Current," *Studies in History and Philosophy of Science,* 1984, *15*:85–117.

[28] Gingras and Schweber, "Constraints" (cit. n. 26), esp. pp. 376–381. See the remarks on calibration as an attempt to resolve the "experimenter's regress" in Collins, *Changing Order* (cit. n. 2), pp. 100–106.

[29] Lenoir, "Practice, Reason, Context" (cit. n. 3), pp. 10–12.

[30] Galison characterizes the thesis based on the work of Pierre Duhem and W. V. Quine as the view that "experiments confront no single hypothesis, but a web of interrelated beliefs . . . [and] an infinite number of auxiliary hypotheses": Galison, *How Experiments End* (cit. n. 3), p. 2. Sociologists tend to invoke more radical versions of the thesis, such as Pickering's "any theory can be brought into agreement with any set of data by appropriate adjustments within the overall conceptual framework": Pickering, "Constraints" (cit. n. 32), p. 90. See also Pinch, "Scientific Observation" (cit. n. 22), p. 14;

Galison's account further emphasizes the importance of time in this process. In fact, his model proposes a number of theoretical and instrumental constraints upon experimental practice, constraints that are located at three different levels of temporal duration. Theoretical programs, such as the quest to unify all physical forces, persist as long-term constraints, whereas specific models for interpreting phenomena come and go more quickly. Also embodied in time is the process of tinkering with instrumental set-ups. Such tinkering is characterized by Galison as oriented toward improving the "stability" and "directness" of the signal by which the phenomenon is perceived.[31] Active, time-embodied intervention thus shapes phenomenal experience, which is not to say that it creates it. Similarly, the acceptance of matters of fact is settled through processes of social negotiation in the scientific community: in this way also passive constraints are encountered in the context of active practice.

Some of Pickering's recent work has shown an increased readiness to allow for the experience of passive constraints in experimental practice. In a paper on the controversy that followed observations of a magnetic monopole announced in 1975, he describes how participants were constrained by prior agreement over the reality of a particular phenomenon, the "platinum peak." Of this incident, Pickering writes: "This episode, then, illustrates the role of socially-agreed phenomenal benchmarks as constraints upon instrumental practice. In principle, of course, [P. B.] Price was perfectly free to deny the existence of the platinum peak; had he done so, however, he would surely have been called upon by [Peter] Fowler, for example, to justify his dissolution of a natural phenomenon already agreed to exist."[32] Although Pickering chooses to gloss the relevant phenomenon here as a "socially-agreed" one, thus drawing attention to its context of production, there seems little doubt that it was experienced by the scientist in question as a passive constraint, one that he did not feel free to evade or renegotiate.

The relationship between active and passive constraints receives further consideration in Pickering's recently sketched "pragmatic realist" epistemology. In this account experimenters are pictured as simultaneously handling three elements: a material procedure, a model of how the apparatus works, and a model of the phenomenon under investigation. The experimenters manipulate all of these elements with the aim of achieving a stable configuration. Such a stabilization, inevitably a local and temporary achievement, constitutes both the "discovery" of a phenomenon and the black-boxing of a working instrument. In this way, "scientific knowledge is articulated in accommodation to *resistances* arising in the material world. . . . [But] material resistances are only manifest *relative to prior expectations:* they have no existence in the absence of such expectations."[33]

Pickering's formulation appears to accommodate a number of the objections

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and (for philosophical discussion) Sandra G. Harding, ed., *Can Theories be Refuted? Essays on the Duhem-Quine Thesis* (Dordrecht: Reidel, 1976).

[31] Galison, *How Experiments End* (cit. n. 3), Ch. 5.

[32] Andrew Pickering, "Constraints on Controversy: The Case of the Magnetic Monopole," *Soc. Stud. Sci.*, 1981, *11*:63–93, on p. 84.

[33] Andrew Pickering, "Living in the Material World: On Realism and Experimental Practice," in *Uses of Experiment*, ed. Gooding, Pinch, and Schaffer (cit. n. 3), pp. 275–297, on pp. 279–281 (italics in original).

against the constructivist account that we have surveyed. In apparent compliance with Gingras and Schweber, he acknowledges that the theory of the phenomenon and the theory of the instrument are usually not the same, though they are simultaneously under test in a particular experiment. He also admits the importance of the temporal dimension in allowing for "stabilization," thus conceding a point to Galison. At the same time, the model of phenomena produced by local practices survives, as does the notion of black-boxing instruments.

Pickering's model offers a promising line of development for further philosophical reflection. It also holds out the prospect of rewarding application to historical studies. There appears to be no need for such studies to "put the phenomena first" nor to bring in "nature" at the end of the narrative to explain the resolution of controversies. Constructivists may claim to have established the undesirability of these whiggish historiographical procedures, in view of the inseparability of natural "reality" from the complex of practices through which it is encountered. On the other hand, there seems equally to be no need to deny the experience of constraint encountered by experimental practitioners. The feelings of frustration and thwarted effort deserve their place in historians' narratives of the toil and trouble of experimental work, even if analysts cannot accept the participants' gloss on these experiences as direct contacts with reality.

Ludwik Fleck gave an excellent participant's account of this type of experience in 1935.

> The first, chaotically styled observation resembles a chaos of feeling: amazement, a searching for similarities, trial by experiment, retraction as well as hope and disappointment. Feeling, will, and intellect all function together as an indivisible unit. The research worker gropes but everything recedes, and nowhere is there a firm support. Everything seems to be an artificial effect inspired by his own personal will. Every formulation melts away at the next test. He looks for that resistance and thought constraint in the face of which he could feel passive. . . . The work of the research scientist means that in the complex confusion and chaos which he faces, he must distinguish that which obeys his will from that which arises spontaneously and opposes it. This is the firm ground that he, as representative of the thought collective, continuously seeks.[34]

Fleck's close and pensive account of the research scientist's experience continues to set a challenge and to impose a responsibility. Sociologists and historians will not simply want to reproduce the scientist's account of experimental work; they will want to transcend and contextualize it in order to address their own analytical concerns. But they might still profitably consider whether their visions of scientific practice can match the elegance and subtlety of Fleck's.

[34] Fleck, *Scientific Fact* (cit. n. 4), pp. 94–95.