History of Science 255

Sociology of Scientific Knowledge

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There are a number of problems that have to be confronted by any
sociological framework for studying science, technology, or
medicine. Loosely following Kant, it might be said that these
are <u>transcendental</u> problems: all sociological traditions are
obliged to offer some way of dealing with them, and, for this
reason, they recur in otherwise widely differing sociological
genres. Among these problems are those of describing and
accounting for the ordered behavior of technical communities;
describing and accounting for the knowledge they produce; and
similarly with respect to the distribution of that knowledge,
its credibility, authority, potency, and its relationship to
non-technical forms of knowledge.

These readings are no more than an introduction to some of the
resources for thinking about such transcendental problems. Some
of the problems attach equally to science, technology, and
medicine; some manifest themselves in different ways in each of
these technical practices.

Assessment: Course work is assessed on the basis of an extended
final paper (normally 25 -> 35 pages) on a topic to be agreed
between students and myself (70%) and seminar participation
(30%). Expect to settle on an paper topic <u>no later than the
third week of the semester</u>.

1. Introduction

No essential reading; general discussion of seminar scope and
conventions.

2. The Problem of Scientific Order (I):

The cohesion of any social group appears to call for explanation: what is the basis of its solidarity and how does it distinguish itself from other social groups? The social order of the scientific community seemed to the founders of the sociology of science to require a quite special explanation, as this community was held to subscribe to unique goals and values and to display a unique form of social order. From the early 1940s, Robert Merton and his followers elaborated and extended their notion that the scientific community was effectively regulated by a special set of <u>norms</u>, which together made up its "institutional ethos." It is not clear, however, just what a Mertonian norm is, nor how norms are supposed to determine scientific behavior. We shall follow some of the history of scientific norms in sociological thought. By the 1950s and 1960s, Merton's own thinking made the notion of a norm and its function both more complex and more ambiguous.

Robert K. Merton, "The Normative Structure of Science," in idem, <u>The Sociology of Science</u>, ed. Norman W. Storer (Chicago: University of Chicago Press, 1973; art. orig. publ. 1942), pp. 267-278.

Robert K. Merton, "Resistance to the Systematic Study of Multiple Discoveries in Science," <u>European Journal of Sociology</u> 4 (1963), 250-282 (reprinted as "The Ambivalence of Scientists" in idem, <u>The Sociology of Science: Theoretical and Empirical Investigations</u>, ed. Norman W. Storer [Chicago: University of Chicago Press, 1973], pp. 383-412, and also under the same title in idem, <u>Sociological Ambivalence and Other Essays</u> [New York: Free Press, 1976], pp. 32-64); see also idem, "The Ambivalence of Scientists: A Postscript," in <u>ibid</u>., pp. 56-64.

Robert K. Merton, "Some Preliminaries to a Sociology of Medical Education," in <u>The Student-Physician: Introductory Studies in the Sociology of Medical Education</u>, eds Merton, George G. Reader, and Patricia L. Kendall (Cambridge, Mass.: Harvard University Press, 1957), pp. 3-79, see especially pp. 71-79 on the management of "incompatible norms."

Robert K. Merton, "Social Problems and Sociological Theory," <u>Social Research and the Practicing Professions</u>, eds Aaron

Q's
• What is M's soc. project? What does he want to explain? Scope? — e.g. no expts, eg's, not practice oriented.
• Shared values 'norms', ethos — what do they/don't they expt about science.
• What is a norm? How does it work? SS d/n know (phil. of soc. sci.) Hobbes Weber
• how do you account for society hanging together — A) Big perception B) Shared internalized sense of right

Rosenblatt and Thomas F. Gieryn (Cambridge, Mass.: Abt
Books, 1982), pp. 43-99, especially pp. 75-76 for the
"elasticity of social norms."

3. The Problem of Scientific Order (II):

By the 1960s and 1970s, a few sociologists were beginning
to express dissatisfaction with Merton's picture of science,
even as it was elaborated and modified: it was said that there
were more norms than Merton allowed for; that behavior in
violation of the original set of norms was more common than
Merton supposed; that the supposed institutional ethos of
science was not in fact unique to science; and, most pertinently
perhaps, that it might not even be right to see norms as
determinants of behavior. Norms be might be viewed as
situationally available justifications of behavior, and the very
notion of what it would be to follow any social rule might be
far more problematic than sociologists had traditionally
conceived. Here the influence of Thomas Kuhn, the
phenomenologists, and the later writings of Wittgenstein were
decisive for the sociology of science. So how <u>do</u> we account for
social order in science? What <u>is</u> the order that actually
requires explanation?

Barry Barnes, "Making Out in Industrial Research," <u>Science
Studies</u> 1 (1971), 157-175.

Michael J. Mulkay, "Norms and Ideology in Science," <u>Social
Science Information</u> 15 (1976), 627-656.

Michael J. Mulkay, "Interpretation and the Use of Rules: The
Case of Norms of Science," in <u>Science and Social Structure:
A Festschrift for Robert K. Merton</u>, ed. Thomas F. Gieryn
(New York: New York Academy of Sciences, 1980), pp. 111-
125.

Harold Garfinkel, <u>Studies in Ethnomethodology</u> (Englewood Cliffs,
N.J.: Prentice-Hall, 1967), ch. 2.

4. The Problem of Scientific Knowledge (I)

What is scientific knowledge, and what is it possible and proper for the sociologist to say about it? Traditional sociology of science, in Merton's idiom, delivered treatment of the content and practice of science to philosophers: sociology, so to speak, stopped at the laboratory door. "Social factors," so called, were conceived as external to science, and admitting their role within science itself was as much as to say that something not properly scientific was happening. Thomas Kuhn's work was decisive in encouraging some sociologists to conceive of a sociology of scientific knowledge, in which "social factors" were constitutive of the very idea of science. It is hoped that members of this seminar will already be familiar with Kuhn's Structure (1962), and the purpose of this session is to introduce some post-Kuhnian sociological frameworks for the naturalistic study of scientific knowledge. Harry Collins' work starts with an invitation to explore "the fundamental problems of order in conceptual and social life," and argues that patterns of inductive inference are entrenched in the fabric of our social life.

H. M. Collins, Changing Order: Replication and Induction in Scientific Practice, 2nd ed. (Chicago: University of Chicago Press, 1992; orig. publ. 1985), esp. Introduction and chs 1-2, 6.

5. The Problem of Scientific Knowledge (II)

The sociology of scientific knowledge is not supposed to be epistemology: that is to say, the customary philosopher's task of justifying beliefs, or sorting beliefs into true and false, is not a sociological concern, save in the special sense that evaluations and classifications are topics for sociological inquiry. How do people come to believe what they do about the natural world? "Methodological relativism" asks practitioners to set aside the evaluations they bring to a body of knowledge as the condition for understanding how their subjects of study evaluate knowledge. Put another way, the topic of the sociology of scientific knowledge is not the validity of beliefs but their credibility. How to study credibility in sensitive and interesting ways? Few aspects of the sociology of scientific knowledge are more widely misunderstood than this methodological

self-denying ordinance, so, while it is important to look at some relaxed and disengaged approaches to the credibility of beliefs, it is also pertinent here to appreciate some of the obstacles that stand in the way of a naturalistic engagement with our culture's most authoritative forms of knowledge.

Richard Rorty, "Science as Solidarity," in The Rhetoric of the Human Sciences, eds John S. Nelson, Allan Megill, and Donald N. McCloskey (Madison: University of Wisconsin Press, 1987), pp. 38-52.

Richard Rorty, "Does Academic Freedom Have Philosophical Presuppositions?" in The Future of Academic Freedom, ed. Louis Menand (Chicago: University of Chicago Press, 1996), pp. 21-42.

Steven Shapin, "Cordelia's Love: Credibility and the Social Studies of Science," Perspectives on Science 3 (1995), 255-275.

Steven Shapin, "Rarely Pure and Never Simple: Talking about Truth," Configurations 7 (1999), 1-14.

David Bloor, Knowledge and Social Imagery, 2nd ed. (Chicago: University of Chicago Press, 1991; orig. publ. 1976), ch. 4.

6. The Problem of Knowledge as the Problem of Order

Solutions to the problem of knowledge are solutions to the problem of order (it has been said), and, if this is so, then understanding what people know something, and how they come to know it, is just another way of understanding how they order themselves in society or in some specialized bit of society. This rejection of the traditional dualism that treats knowledge (or culture) and society as distinct entities has taken root most strongly in science studies. An early expression of this sensibility was Leviathan and the Air-Pump (1985), which some members of this seminar may have already looked at. A Social History of Truth was a subsequent attempt to give this programmatic sensibility as much social-historical grounding as possible, while Bruno Latour's more programmatic work raises

issues about the relationship between social studies of science and metaphysical projects.

Steven Shapin, <u>A Social History of Truth: Civility and Science in Seventeenth-Century England</u> (Chicago: University of Chicago Press, 1994), esp. chs 1-3, 6.

Bruno Latour, <u>We Have Never Been Modern</u> (Cambridge, Mass.: Harvard University Press, 1993), ch. 2.

7. The Problem of Meaning

'New historicist' sensibilities in the history of ideas, and indeed most current orthodoxies in intellectual history, insist on retrieving authentically historical meanings and rejecting the imposition of meanings by the historian on subjects' beliefs. Such sensibilities now almost define the nature of a legitimately historical approach. Nevertheless, there are some seldom-acknowledged problems attending the possibilities of retrieving and writing about past belief, and especially attaching to questions of "intention." We shall look at a programmatic statement of 'contextualism' that is well known among intellectual historians-- Quentin Skinner's "Meaning and Understanding." Then we will confront several less familiar sociological cautions about the way in which projects broadly sharing Skinner's sensibilities can be realized.

Quentin Skinner, "Meaning and Understanding in the History of Ideas," <u>History and Theory</u> 8 (1969), 3-53 (also in James Tully, ed., <u>Meaning and Context: Quentin Skinner and His Critics</u> [Cambridge: Polity Press, 1988, pp. 29-67]).

C. Wright Mills, "Situated Actions and Vocabularies of Motive," <u>American Sociological Review</u> 5 (1940), 904-913.

H. M. Collins, "Understanding Science," <u>Fundamenta Scientiae</u> 2 (1981), 367-380.

8. The Problem of Efficacy

The supposition that scientific knowledge <u>works</u> is, for many people, a powerful inducement to accord it a privileged status, even to say that it must for that reason be <u>true</u>. Simplistically put, true knowledge is the condition for successful practice, and, conversely, the efficacy of a practice can be taken as a sign that the knowledge on which it is predicated is the genuine stuff. There is something to recommend this sentiment, but its simplistic version is full of problems. Firstly, there is a categorical difference between beliefs and deeds, so to say that true belief <u>works</u> can only be a short-hand for some more complex set of relations. Secondly, it is clear that successful practices have flowed from beliefs we do not now recognize as true and that the credibility of those beliefs was thereby recommended. Thirdly, the efficacy of many modern technical practices, and of the knowledge said to underpin them, are widely secured through test situations whose resemblance to "natural" versions is established largely through conventional agreement.

Michael J. Mulkay, "Knowledge and Utility: Implications for the Sociology of Knowledge," <u>Social Studies of Science</u> 9 (1979), 63-80.

Charles E. Rosenberg, "The Therapeutic Revolution: Medicine, Meaning, and Social Change in Nineteenth-Century America," <u>Perspectives in Biology and Medicine</u> 20 (1977), 485-506 (reprinted in idem, <u>Explaining Epidemics and Other Studies in the History of Medicine</u> [Cambridge: Cambridge University Press, 1992], pp. 9-31).

Donald MacKenzie, "From Kwajalein to Armageddon? Testing and the Social Construction of Missile Accuracy," in <u>The Uses of Experiment: Studies in the Natural Sciences</u>, eds David Gooding, Trevor Pinch, and Simon Schaffer (Cambridge: Cambridge University Press, 1989), pp. 409-435.

9. The Problem of Travel

Bruno Latour has influentially suggested that power is constituted through representations and objects that can travel

freely while remaining undeformed ("immutable mobiles"). From this perspective, science and technology are uniquely powerful since they both produce and depend upon <u>universal standards</u>. Much recent work in the history and sociology of modern science has described the <u>metrological</u> projects which integrate science and technology into the power-wielding apparatuses of the state and of globalized economic and political configurations. On the other hand, an important sensibility in science and technology studies insists on the <u>local origins</u> and <u>contextual embeddedness</u> of representations and artefacts to which undeformed universality is customarily attributed. Are we faced with a choice between seeing science and technology as universals or as locally embedded? Are there ways of appreciating universals as extensions of local circumstances?

Bruno Latour, <u>Science in Action: How to Follow Scientists and Engineers through Society</u> (Cambridge, Mass.: Harvard University Press, 1987), ch. 6.

David P. Miller, "Joseph Banks, Empire, and 'Centers of Calculation' in Late Hanoverian London," in <u>Visions of Empire: Voyages, Botany, and Representations of Nature</u>, eds David Philip Miller and Peter Hanns Reill (Cambridge: Cambridge University Press, 1996), pp. 21-37.

Simon Schaffer, "Visions of Empire: Afterword," in <u>ibid</u>., pp. 335-352. *Charge TBA*

Steven Shapin, "Placing the View from Nowhere: Historical and Sociological Problems in the Location of Science," <u>Transactions of the Institute of British Geographers</u>, n.s. 23 (1998), 5-12.

10. The Problem of Expertise

Scientists, engineers, and physicians claim authority by virtue of being <u>experts</u>. That is to say, what they know is supposed to be both more than, and better than, what the laity know. And sometimes it is said that certain experts possess a unique and effective <u>method</u> that guarantees the quality of their knowledge. The issue of technical knowledge vis-à-vis common sense is endemic and enduring. Ideas of common-sense and ideas of expertise have marched in lock-step throughout history. Here

we take a look at some historical studies of how scientific and medical expertise have been presented, at some philosophical skepticism about certain ways of claiming superiority for scientific expertise over common sense, and at currently influential strands of cognitive psychology that purport to identify some of the flaws of everyday reasoning.

Gilbert Ryle, "The World of Science and the Everyday World," in idem, Dilemmas: The Tarner Lectures 1953 (Cambridge: Cambridge University Press, 1964), pp. 68-81.

Amos Tversky and Daniel Kahneman, "Judgment under Uncertainty: Heuristics and Biases," Science 185 (27 September 1974), 1124-1131.

Steven Shapin, "Proverbial Philosophy: How an Understanding of Some Linguistic and Social Features of Common Sense Can Throw Light on More Prestigious Bodies of Knowledge, Science For Example," Social Studies of Science 31 (2001), 731-769.

Steven Shapin, "Descartes the Doctor: Rationalism and Its Therapies," The British Journal for the History of Science 33 (2000), 131-154.

11. The Problem of Persons

Robert Merton's account of the regulative norms of science was predicated on the view that scientists were neither constitutionally nor motivationally different from anybody else in society. There was no reason to be interested in what they were like as individuals, since their individual and collective characteristics were in no way implicated in the knowledge they produced or in its cultural authority. Similarly, followers of Max Weber see the instrumentality of modern science and technology, and its characteristic modes of organization, as squeezing charismatic authority out of the culture. Science is not only, as has been said, "the view from nowhere," it is also the knowledge of no one. Few sensibilities so strongly mark the official discourses of late modern culture and its presumptions about the authority of scientists. How and why have presumptions about the relationship between scientific knowledge and personal virtue changed in recent times? What might be the consequences

of these changes for the authority of science? Is it quite clear that presumptions about the link between knowledge and personal virtue <u>have</u> disappeared from late modern culture?

Max Weber, "Science as a Vocation [1918]," in <u>From Max Weber: Essays in Sociology</u>, trans. and eds H. H. Gerth and C. Wright Mills (London: Routledge, 1991), pp. 129-156 (also in <u>Max Weber's 'Science as a Vocation'</u>, eds Peter Lassman, Irving Velodny, and Herminio Martins [London: Unwin Hyman, 1989], pp. 3-31).

Steven Shapin, "The Philosopher and the Chicken: On the Dietetics of Disembodied Knowledge," in <u>Science Incarnate: Historical Embodiments of Natural Knowledge</u>, eds Christopher Lawrence and Steven Shapin (Chicago: University of Chicago Press, 1998), pp. 21-50.

Steven Shapin, "Nobel Savage" [Essay review of Kary Mullis, <u>Dancing Naked in the Mind Field</u>], <u>London Review of Books</u> 21, no. 13 (1 July 1999), pp. 17-18. Or: <u>http://www.lrb.co.uk/v21/n13/shap01_.html</u>

Richard Preston, "The Genome Warrior [Craig Venter]," <u>The New Yorker</u> (12 June 2000), 66-83.

Weeks 12 and 13:

I would like to reserve the last two weeks for seminar members' suggestions about additional topics they would like to address or further engagement with materials already discussed.