BOOK REVIEWS

Mertonian Concessions

Making Science. Between Nature and Society. STEPHEN COLE. Harvard University Press, Cambridge, MA, 1992. xiv, 290 pp. $39.95.

There is little evidence that scientists require the guidance of a formal sociology of science to get through a day's work. It can only be a small minority of scientists who have ever read the work of the few academic sociologists investigating them and the knowledge they make. Nevertheless, scientific practice cannot proceed without day-to-day, moment-by-moment solutions to problems of social order. How do scientists call each other to task? What are the grounds on which scientists recognize good and bad behavior in their disciplines? How is it that scientists come to agree about the existence of evidence and the validity of theories? How are scientific knowledge and technique reliably transmitted from one scientist to another, and from one generation to the next? These are central questions addressed by sociologists of science, and these same questions have to be practically solved by scientists as the condition for doing their work and for making scientific knowledge.

Unlike much philosophy of science, sociological analyses have tended to avoid a preachy tone. In the main, sociologists have accepted their task as the description and interpretation of the means scientists actually use to accomplish and maintain social and epistemic order. Yet some accounts of scientists as social animals have found readier acceptance than others in the academy and in corridors of scientific power.

Modern sociology of science was arguably ushered into being with the work of Robert K. Merton in the late 1930s, and the general form of Merton's answers to questions about scientific order and legitimacy—ceaselessly tested, elaborated, and modified by his associates and students—has become entrenched in the American academy. It is overwhelmingly Merton's framework for a sociology of science that is turned to by U.S. government agencies on the occasions when they have asked sociologists to advise them how the social system of science works and whether it is working well.

The general form of the Mertonian enterprise can be concisely summarized. Order and consensus within the scientific community are guaranteed by the articulation of a special set of moral "norms" and by practicing scientists' "internalization" of these norms. The norms have the form of general moral injunctions—be disinterested, be skeptical, share knowledge, evaluate claims without respect to the social identity of those who make them—and their effective regulation of scientists' actions is the condition for science fulfilling its "institutional goal" of producing and extending objective knowledge.

Note that, for this genre of sociology, questions about the bases of social order in science can be addressed and answered without attending to the structure and content of scientific knowledge or to the day-to-day vicissitudes of work at the laboratory bench. Accordingly, Mertonian sociologists rarely burdened their writings with the technicalities of scientific practice: you didn't have to know what bioflavonoids were or how paper chromatography worked in order to produce an account of social order in the community of plant biochemists. Sociological investigations of science were not to be confused with a sociology of scientific knowledge. On the one hand, social order was guaranteed by other considerations; on the other, a sociology of scientific knowledge was presumed to be impossible, since scientific truth was asocially generated by applying rules of right method to the evidence presented by nature.

This sociological story enjoyed—still enjoys—enormous prestige and cultural appeal. On the whole, and quite understandably, many modern Western scientists like to see their community portrayed as liberal, universalistic, and epistemically privileged. Yet, from the late 1960s, another, quite different, sociological story about science was emerging. Thomas Kuhn's *Structure of Scientific Revolutions* (1962) influentially argued that the resources scientists use to say "good" and "bad" about each other are constituted by scientific knowledge itself. The norms of science are not a set of special ethical maxims but the intellectual and technical tools with which scientific training equips members of various research communities and the deployment of which allows members to assess work as competent or incompetent, significant or trivial.

By the early 1970s a number of mainly non-American sociologists (including, to declare an interest, this reviewer) were exploring the profound implications that seemed to flow—Kuhn himself protesting otherwise—from this central "Kuhnian" insight. First, the "social order" of science was likely to be not one but many orders, as many as there were "paradigms" that structured researchers' epistemic judgments. Second, the appropriate methodological posture for a historian or sociologist concerned to interpret scientists' behavior was "relativistic," since epistemic judgment was relative to the local culture of scientific groups. Third, a sociology of scientific knowledge appeared not only possible but necessary if one was to give an account of social order in science. Indeed, the "neo-Kuhnian" framework, with its stress upon scientific knowledge as embodied collective practice, raised serious questions about the analytic legitimacy of traditional speech of "social versus cognitive factors" in science. If scientists were institutionally socialized into their stock of knowledge and associated evaluations, and if that very stock of knowledge constituted the normative structure of science, then how was it proper to distinguish the social and the cognitive?

Through the 1980s Mertonians and Kuhnians—the latter more widely known as sociologists of scientific knowledge or social constructivists—developed parallel sociologies of science, largely separated by a conceptual as well as a physical ocean. The social constructivists displayed little interest in shooting down the Mertonians, who might count as valuable targets but who were relatively rare, and trained their guns on the far more abundant flock of philosophical realists and rationalists, most of whom continued to produce highly stylized accounts of scientific method showing scant sensitivity to any view of science as historically embedded collective practice. And the Mertonians by and large kept silence about the sociology of scientific knowledge (SSK), only occasionally sallying forth to wag a minatory finger at the upstarts: too radical, too concerned with philosophical matters, and (darkly hinted) too likely to give aid and comfort to modern anti-science.

Making Science is the first systematic book-length assessment of SSK by a major Mertonian sociologist of science. Though some of the book revises previously published traditional work on the reward and reputational structure of modern American science, interest is bound to focus on the portion that addresses the relationship between the two genres of sociology of science. It will surprise many disinterested spectators of this particularly pugnacious part of the academic world that Cole's book comes adorned with olive branches, offering a "rapprochement" between the two sociological frameworks. The "best" SSK is said to "represent an important contribu-

tion" to the sociology of science, and Cole, irenically (if incredibly) goes so far as to describe social constructivism as the "dominant" genre.

In truth, Cole's book represents more a conversion than a compromise. He concedes much to the social constructivists, granting more to contingency and to "social factors" than even some constructivists have claimed. The "universalism" of Mertonian sociology is frankly given up; no special sociological basis is offered for demarcationism; the moral norms of science that formed the centerpiece of Mertonian solutions to the social-order problem are scarcely mentioned; idealized portrayals of high degrees of scientific consensus are rejected; the role of authority in shaping scientific judgment is freely acknowledged; and the positivist philosophy that underpinned Merton's treatment of scientific knowledge as a black box is abandoned.

The nakedness of conversion is, to be sure, covered by a generously cut fig leaf of eclecticism. Ideal-typical positivist philosophers have, Cole says, unwarrantably assumed that the logical assessment of empirical evidence was the ultimate arbiter of scientific judgment, while ideal-typical social constructivists have "gone too far" toward the other extreme. Cole sensibly sets himself against any position arguing that evidence from the natural world "has no influence" upon scientific belief. The proper view is that "social factors" play some role and that empirical evidence from nature plays some role. What role ought to be assigned to each awaits detailed investigation.

The credibility of an eclectic position crucially depends upon the incredibility of the extremes between which it is placed. Between two sheer cliffs what wise person would not prefer the safety of level ground? Such is the instinctive appeal of eclectic middle ways that it is always worth giving them a quick check-over to see whether the extremes to be avoided have been correctly portrayed. In the present case one fears that an ideal-type has been transformed first into a straw man and thence into a punching-bag. After his initial flourish Cole has little to say about positivist philosophers and gets on with the job of identifying two main failures of social constructivism.

First, social constructivists are said to claim that "the content of science is determined solely by social variables" and accordingly to "argue that the empirical world has little, if any, influence." Such a position is rightly judged to be patently absurd. Second, social constructivists are said to have manifestly failed in their attempts to produce convincing demonstrations of their claims: they have "failed to generate a single example or case-study" that shows that social processes "actually influence the specific content of science." A sociology of scientific knowledge, as opposed to a sociology of scientific foci of interest, remains impossible.

One would like to say that the first diagnosis of "failure" is simply a misunderstanding, albeit, unfortunately, a widely distributed one. Here Cole apparently has in mind the "strong programme" of British sociologists Barry Barnes and David Bloor, yet he has evidently missed such continually repeated sentiments as these: "No consistent sociology could ever present knowledge as a fantasy unconnected with our experience of the material world around us" (Bloor, 1976), or "There is indeed one world, one reality, 'out there,' the source of all our perceptions" (Barnes, 1977). Recognition that some prominent social constructivists, at least, do not correspond to his ideal-type is buried in an endnote, where Cole blandly suggests that there may be "little or no difference between their position and that taken in this book." Adopting an interpretative position alien to his usual style, Cole then urges sociologists to be realists because scientists

working within "normal" traditions are realists. But that is to take for granted more than we know. Many scientists are instrumentalists and pragmatists, still more probably have not got a position on the issue, and the comparison between "normal" traditions is widely recognized to create substantial problems for realism.

Resolution of the second "failure" of SSK is not so easily arranged. If social constructivists were indeed in the business of causally demonstrating the exclusive role of social factors in the production and evaluation of scientific knowledge, then there would be little problem in agreeing that the enterprise had miscarried. Yet Cole's familiarity with the SSK literature, in other respects quite impressive, fails him here. For a quite typical form of social constructivist case-study involves the examination of scientific controversy. How is one to account for variation in scientific judgment when both parties to a controversy have access to the same evidence and, presumably, to the same canons of right reasoning? Here social constructivists have argued that empirical evidence has a causal role but not a discriminating role. If nature is one and the same, then one has to look elsewhere to account for variation in belief and judgment. It is primarily for this reason that methodological—not ontological—relativism has recommended itself to sociologists of scientific knowledge.

Cole wants social constructivists to acknowledge the constraining role of nature in the formation of scientific belief. Amazingly, however, when he gets around to saying what he means by "nature" it turns out to overlap massively with what his opponents mean by "society": "The accepted body of knowledge is the functional equivalent of nature." For Cole the "accepted body of knowledge" is a "cognitive factor" to be juxtaposed eclectically to "social factors." Yet the processes by which members come to acquire "accepted knowledge" are widely designated by the term "socialization," just as the possession of different bodies of knowledge is a major means used to distinguish different social groups. On close inspection, Cole's eclectic sociological compromise looks more well-intentioned than well-conceived. The battle continues.

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General Inventor

Alexanderson. Pioneer in American Electrical Engineering. JAMES E. BRITTAIN. Johns Hopkins University Press, Baltimore, MD, 1992. xviii, 384 pp., illus. $45. Johns Hopkins Studies in the History of Technology.

If asked about technological innovation in the 20th century, most Americans today would tell you three things. First, they would insist that major breakthroughs come from science. Second, they would inform you that innovation is done by teams and that the age of heroic inventor is long gone. And third, most would tell you that innovation is performed by experts who devote their lives to mastering one esoteric subfield. In this well-researched book James E. Brittain challenges these assumptions by demonstrating how one broadly creative individual helped develop radio and electronics while working primarily in an engineering and not a scientific tradition.

Ernst Alexanderson was born in 1878 in Uppsala, Sweden, where his father taught at the university. Choosing engineering as his career, Alexanderson attended the Royal