

Merton's Sociology of Science: The First and the Last Sociology of Science?

KARIN KNORR CETINA
University of Bielefeld

Merton the founder of the
sociology of science

Even his enemies admit that Merton is the founder of the sociology of science. When he published his classic *Science, Technology, and Society in Seventeenth-Century England* in 1938, the sociology of science was not a recognized field. Fifty years later, an abun-

dance of programs of instruction and centers of research in social studies of science and technology can be found in the United States and Europe, and the status of sociology of science as an academic subdiscipline is beyond question. To be sure, there were other books produced on science, technology, and society at the time of Merton's publication, most notably Bernal's famous *The Social*

Function of Science (1939). Yet none of the works examining the social aspects of science had the range of professional interests, the depth of knowledge, and, later, the strategic position in sociology to help a whole subfield off the ground. Moreover, Merton's achievement in this respect was not just an institutional achievement. The many programs in the area that we have today came later, and most were only indirectly helped by Merton. Rather, in finding and motivating students, in producing over time a whole series of seminal papers on social aspects of science, and in building his "middle-range" theories in sociology in general, Merton created a framework of concepts and tools for the sociology of science. Most importantly, he proposed a program for research: to trace the way the institutional environment of science, including its norms and cultural values, impinges on science—not on the nature of science or the substance of its theories but on its progress and development. Merton's students fleshed out this program by emphasizing, in addition, the institutional structure *within* science—for example, its social structure (e.g., Cole and Cole 1973; Zuckerman and Merton 1971).^x

This research program set the new field apart from the conception of science studies as a pastime for natural scientists-turned-commentators of their field (a conception still dominant in some countries). Merton is sometimes criticized for *not* producing a systematic theory of society or a system of sociology, and for instead using his extraordinary talents to focus theoretical propositions on empirical research (see Bierstedt 1981). Yet in his stubborn insistence that theory must have utility for research and must adapt its range and conceptions to this role, Merton was ahead of his time rather than behind among American sociologists. Certainly, in sociology of science his research orientation has set a trend that continues today within and without the Mertonian program; and in sociology in general, grand, unifying systems of thought are now treated with widespread suspicion. If the Mertonian program has

failed, it has not failed in recommending and initiating professional sociological research.

But has it failed? Many of my colleagues today would say yes. There exists today a "new sociology of science," also known as the "sociology of scientific practice" or the "sociology of scientific knowledge," which has become a central part of the larger field of science, technology, and society.²

This new brand of social studies of science has created its research agenda in opposition to the Mertonian program. The following concerns itself with the criticism the Mertonian approach has attracted in relation to this new development. Stripped to its essentials, this criticism can be divided into two categories: first, it consists of an attack on the "normative" and "functionalist" orientation of Merton's sociology of science; second, it questions any research agenda that saves the effort of examining in detail the substance of science and of scientific work.

The critique of Merton

Interestingly enough, Merton's original work on Puritanism and the rise of modern science has not attracted the attention it deserves among sociologists. Sociologists more readily latched on to his later essays, particularly his work on the "ethos" of science (e.g., Stehr 1978, Collins 1982). Merton's first fully fledged discussion of this ethos is a paper from 1942 in which he described "four sets of institutional imperatives": universalism, communism, disinterestedness, and organized skepticism. These are the "guiding principles" of scientific work, the "canons" expressed through demands made upon scientists: scientific findings must be published (the norm of communism), knowledge claims must be subjected to impersonal criteria of evaluation (universalism), personal interests must be excluded from proper scientific procedures (disinterestedness), and criticism is permitted and encouraged (organized skepticism). The critics argued that these norms are neither stable properties nor exclusive sanctionable ideals of scientific activities. Where the norms are endorsed, "counternorms" also appear which to some degree cancel the original imperative.

¹ It is only fair to say that Merton's students (those associated with Merton) did not "just" extend his program. See, for example, the most recent book, by Stephen Cole (1991).

² For an overview over this program, see Knorr Cetina and Mulkay (1983) and Zuckerman (1988). For a review from the Mertonian perspective see Gieryn (1982).

Consequently, both norms and counternorms may be perceived as part of a larger (and changing) rhetoric of science whose relationship to scientific practice remains an open question. But the criticism runs deeper: it is directed against any approach that treats norms, or values, as primary explanatory principles of social behavior. The whole movement of sociological theory, after Parsons, is a move against such assumptions. Within the sociology of science, the goal of "explaining" scientific behavior itself became a point of contention. Where the goal was maintained, the normative model was replaced by the interest, conflict, and interaction models of scientific conduct.

In addition to the objections against any "normative" theory, the critique of functionalisms spilled over into debates about "Mertonianism" in science studies, although it was directed more against Parsons than against Merton. More interesting perhaps, and more specific to science studies, was the second major objection against the Mertonian sociology of science, an objection directed against the original research program. Since virtually the same objection has been raised independently by historians of science against the "Merton thesis," let me look at the latter first. The "Merton thesis"¹ is the upshot of his 1938 volume. In essence it says that Puritanism in seventeenth-century England promoted a favorable attitude toward science in virtue of the "inner-worldly" activism which it favored as a means to the Puritan's goal of achieving a state of grace. In modern science, inner-worldly activity translates into empiricism and rationalism—the presumed worldly means for revealing God's works. The ensuing debates concerned the question of whether the Puritan or Catholic context provided the social reinforcement that helped seventeenth-century science off the ground. Predictably, they also concerned the question of whether religion, commerce, or industry should be taken as the dominant sustaining factor in regard to science (Heilbron, FE, p. 11). Yet the deeper issue, from the point of view of recent sociology of science, is a methodological one. Could a thesis such as the Puritanism-science thesis even be addressed without adequate analysis of the nature of the new science? From this perspective, predominantly put forward by Hall (1963; see Rattansi. CMM. pp. 351-79). Merton was cavalier about characterizing

science. He essentially took science to be identical with its public face, as the embodiment of objective knowledge. Yet an adequate analysis, so Hall argued, would demand better care in establishing the institutional *reality* of science (and of course of Puritanism). If this care was taken, other features of science would emerge (such as mathematization and mechanization) whose congeniality with Puritanism would have to be proven by Merton.

The details of this debate are of no concern to us here. What is significant is the extent to which the problems historians had with Merton's approach and the problems on account of which the new social studies of science moved away from Mertonianism are identical. The point is that social influences upon science, and indeed the social makeup of science, cannot be adequately understood if the "cognitive"¹ beliefs, the methodical procedures, the ontological assumptions, and more generally the technical structure of this institution are not known, and not addressed in the analysis. Merton's definition of science remained an outsider's definition. Since then, the sociology of science has emulated the history and philosophy of science by becoming a field that *includes* the substance of science. It no longer shuns the responsibility of considering the technical content of scientific work. In fact, it often considers it in as much detail as do the scientists themselves. The result is that the sociology of science has turned into a sociology of scientific knowledge. It has become internalist as well as externalist[^] concerned with the content within as well as the context of scientific work. To be sure, Merton's thesis was also a thesis within the sociology of knowledge. In this respect, as in others, Merton anticipated the development of the field. Yet he did not consider enough the need for a sociology of scientific knowledge to develop not from external influences (such as Puritanism) upon science, but from a definition of the phenomenal structures and the technical activities of its objects.

The alternative definition of social studies of science

The Mertonian conception of sociology is a pure, coherent, but exclusionary conception. It is based on the deployment of important sociological concepts (social structure, function.

norm, value, social actor, social role, anomie, and the like) as defined by the classic studies of the field. These concepts are tools for classifying social relations and mechanisms of integration or disintegration. They go hand in hand with a set of predominantly "quantitative" methods that suitably support these concepts. Yet in choosing this paradigm, sociology had proposed a division of labor with other disciplines—for example, with history, linguistics, ethnography, or philosophy. The cognitive content, discourse, cosmology, and ontology of modern institutions were left to these fields. Stimulated by Kuhn, the new studies of knowledge proposed a more inclusionary definition. They rejected the special epistemic status of science and came to believe that cognition, discourse, cosmologies, and ontologies are also socially constituted. The inclusionary definition mirrors developments in sociology in general, where such subfields as ethnomethodology, discourse analysis, and micromethods, formerly left to historians and anthropologists, have taken hold. Yet within social studies of science, the move toward an internalist sociology of scientific knowledge raised further issues, which dominate the discussion in the field today. These are the issues of reflexivity and of the redefinition of sociology itself.

First reflexivity. The conception developed by new studies of knowledge sees scientific (and technological) reality and "facts" first and foremost as the outcome of a process of construction. "Truth" is seen as a consequence rather than a cause of this process. But if natural scientists' results are not unproblematic representations of natural reality, what about *social* scientists' representations? It is easy to see that the constructionist thesis applies equally to the "findings" of sociologists of science themselves. This awareness has led to a self-reflexive discussion of the "methodological horror" of reflexivity (e.g., Woolgar 1988), and to its further exploration through the study of the methods through which social scientists learn about science (e.g., Mulkay et al. 1983). To a certain degree, the exploration of reflexivity has promoted a problem shift in science studies: it has mingled the original problems on the research agenda of the field (e.g., the problem of understanding the practice of natural science) with methodological and epistemological questions, and has thereby contributed to a further alienation between

sociology of science as it once existed and its current developments. But the point I want to draw attention to is the weakening of social analysis that follows from the discussion of reflexivity. Traditional, Mertonian sociology applied the belief in the edge of objectivity of science to itself. It remained secure enough in its knowledge of the positivist foundation of science to carry the Mertonian research program through until today. The new-sociology of scientific knowledge, on the other hand—since it cannot shirk the duty of confronting reflexivity—more easily lends itself to discussions that lead away from, or continually redefine, a coherent research agenda. If the turn away from the Mertonian program has contributed to opening up the definition of science and nature for sociology, reflexivity has contributed to opening up the definition of sociology itself.

Of course, in a sense this definition has been at issue ever since sociology moved into science. Unlike the history of science, which used to be an intellectual history when it was internalist, the sociology of science has not become "cognitive" or "intellectual" through this move. But neither has it remained the strictly social relational, strictly social institutional sociology that it had been in the 1940s, '50s, and '60s. Merton's definition of sociology, too, was an outsider's definition, a definition applied to but not learned from a deeper understanding of the working of science. Complex systems that are organized around large bodies of esoteric knowledge which they construe using their own internal mechanisms have their own ways of enacting society. They enact society (and other matters) *within the technical body of their work*, not just within their organizational structures, financial departments, or other more obviously "social" components. They may, for example, achieve social goals through their own "technical" means, or use social means to further scientific goals. Such systems include a whole new level of reality, a whole new category of resources and mechanisms (the category of the "technical" or "scientific"), which they can bring to bear on or intertwine with more traditional social categories and resources. They may incorporate new definitions of the social, and revise distinctions that sociologists derive from other, less textured environments. In other words, they construct a cultural order of their own that is embedded within the technicalities of their work.

One consequence of learning from complex, knowledge-based institutions how they recreate the social is that sociologists have to begin to rebuild *their* conceptual framework for analyzing science. For one thing, the distinction between the social and the natural order, between social and natural forces, between the intentional human actors sociology assumes and intentionless nonhuman "things" has itself been called into question (e.g., Latour 1991). For another, comparisons between different sciences suggest that there exists a variety of "sociologies" embodied in the cultural orders of these sciences, and that abstract metaconcepts, or alternations between frameworks, will be needed to deal with this situation.³ At this point the redefinition of sociological resources in relation to the sciences is in full course. It seems likely, however, that students of science in this area will not revert to Merton's program. With regard to some questions, his institutional, pure, untainted *sociology* of science will retain its value. But for other questions which the study of science has opened up in recent years, the original sociology of science offers few tools. If the field develops along the present lines it will continue to blur the familiar division of labor between disciplines and the distinctions that have been foundational for sociology—as it blurred the distinction between cognitive and social analysis by finding society within the technical part of science. If this happens, Merton's program will nonetheless have been the first true professional sociology of science. But perhaps it will also remain the last true sociology of science.

³ For an elaboration of this point, see Knorr Cetina (1991).

Other Literature Cited

- Bernal, J. D. 1939. *The Social Function of Science*. New York: Macmillan.
- Bierstedt, Robert. 1981. *American Sociological Theory: A Critical History*. New York: Academic Press.
- Cole, Stephen. 1991. *The Sociology of Science*. Cambridge, MA: Harvard University Press.
- Cole, Jonathan R. and Stephen Cole. 1973. *Social Stratification and Science*. Chicago: University of Chicago Press.
- Collins, H. M. 1982. "Knowledge, Norms and Rules in the Sociology of Science." *Social Studies of Science* 12:299-309[^]
- Gieryn, Thomas 1982. "Relativist/Constructivist Programmes in the Sociology of Science: Redundance and Retreat." *Social Studies of Science* 12:279-97.
- Hall, A. Rupert. 1963. "Merton Revisited, or Science and Society in the Seventeenth Century." *History of Science* 2:1-16.
- Knorr Cetina, Karin and Michael Mulkay, eds. 1983. *Science Observed: Perspectives on the Social Study of Science*. London: Sage.
- Knorr Cetina, Karin D. 1991. *Epistemic Cultures* (forthcoming).
- Latour, Bruno. 1991. "The Impact of Science Studies on Political Philosophy." *Science, Technology and Human Values* 16:3-19.
- Merton, Robert K. 1938. "Science, Technology and Society in Seventeenth-Century England." *Osiris* 4:360-632.
- _____. 1942. "A Note on Science and Democracy." *Journal of Legal and Political Sociology* 1:115-26.
- Mulkay, Michael, J. Potter, and Steven Yearley. 1983. "Why an Analysis of Scientific Discourse is Needed." Pp. 171-203 in *Science Observed: Perspectives on the Social Study of Science*, edited by Karin Knorr Cetina and Michael Mulkay. London: Sage.
- Stehr, Nico. 1978. "The Ethos of Science Revisited." Pp. 172-96 in *The Sociology of Science: Problems, Approaches and Research*, edited by J. Gaston. San Francisco: Jossey-Bass.
- Woolgar, Steve, & A. L. & A. L. [^]*Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge*. London: Sage.
- Zuckerman, Harriet. 1988. "The Sociology of Science." Pp. 511-74 in *Handbook of Sociology*, edited by Neil J. Smelser. Newbury Park, CA: Sage.
- Zuckerman, Harriet and Robert K. Merton. 1971. "Patterns of Evaluation in Science: Institutionalization, Structure and Functions of the Referee System/" *Minerva* 9:66-100.