

Introduction

By Norman W. Storer

If Robert K. Merton has not yet been publicly described as a founding father of the sociology of science, there is at least substantial agreement among those who know the field that its present strength and vitality are largely the result of his labors over the past forty years. His work has given the discipline its major paradigm. This judgment is perhaps most decisively affirmed when set forth not by the many whose work is guided by that paradigm but by those who find fault with some aspect of it. Barry Barnes, for instance, who with R. G. A. Dolby¹ has strongly argued the case against certain assumptions in the paradigm, sums things up by observing that

A dominant influence in this development [of the sociology of science as a separate academic specialty] was the work of Robert Merton, both as writer and teacher. By 1945 Merton had laid down an approach which identified science as a social institution with a characteristic ethos, and subjected it to functional analysis. This was for a long period the only theoretical approach available to sociologists in the area, and it remains productive and influential today. Its central ideas have received detailed elaboration, modification and reinterpretation by, among others, Barber, Hagstrom, Storer and Merton himself, making it the only maturely developed framework for the sociological study of science.²

1. S. B. Barnes and R.G.A. Dolby, "The Scientific Ethos: A Deviant Viewpoint," *European Journal of Sociology*, 11 (1970): 3-25.

2. Barry Barnes, ed., *Sociology of Science: Selected Readings* (London and Baltimore: Penguin Books, 1972), pp. 9-10. And again, Barnes notes: "The only long standing tradition in the sociology of science derives from Robert K. Merton's insights into the nature of its institutional structure" (Ibid., p 61). For similar observations, see the opening paragraph of the critical essay by Michael Mulkey, "Some Aspects of Cultural Growth in the Natural Sciences," *Social Research* 36 (1969): 22-52, and pages 244-46 of Kenneth J. Downey, "Sociology and the Modern Scientific Revolution" (*Sociological Quarterly* 8 [1967]: 239-54).

As a sociological specialty, the field has come alive only in the past fifteen years or so; the upward turn in the logistic curve describing its growth (which we know is typical of new, "hot" specialties in many fields of science) began in the mid-fifties. It would perhaps be a sign of premature senility, or at least of the flattening of the S-shaped curve, for any new field other than the sociology of science to begin so early to examine its own development. But this field has the peculiar character of being grist for its own mill. Yesterday's achievements—and failures—are data for today's research on the growth of scientific specialties, as is the case with no other specialized discipline. This unique property carries its own hazards. Too much thinking about one's own thinking can produce intellectual stasis; too much questioning of one's own questions can produce a kind of sociological anomie. Yet such difficulties can scarcely be allowed to dissuade us from trying to understand the character and development of this special field.

The papers collected here are intended to serve several purposes. Primarily, the volume brings together a number of articles that have been of central significance in the development of the sociology of science, together with others which are representative of certain stages in that process. At the same time, the collection may provide a sense of the intellectual continuity and coherence of the field; more clearly here than in some other fields of sociology, the seeds of future growth can be readily found in papers antedating this growth by ten years and more. In a more practical vein, enclosing these papers drawn from many different sources within a single cover will afford easy access to them for those wanting to make use of them in their own work. Finally, the collection pays tribute to the author; the substance and style of the papers themselves record, in a way mere panegyric could not, the enduring importance of his work.

The papers are not presented in strictly chronological order. The warp and woof of the entire corpus is drawn so tight—the intersections of different threads of thought are so frequent—that it has seemed better to separate and group the major elements in this mosaic for concentrated attention than to leave the task entirely to the reader. It is hoped that in this way the continued clarification of ideas and the ways they have been woven together to give added strength to this growing body of knowledge will be made more visible.

But the papers themselves, even with the extensive footnoting that has been characteristic of Merton's work since the beginning, cannot provide full perspective on the larger scene—the social and intellectual context within which they have been produced and to which they have contributed. It is the aim of this introduction to supply such perspective from the vantage point of 1973, aiming not at anything like a history of the sociology of science but rather at sketching the major landmarks and problems that

have provided its broad outlines. Additional detail will be found in the prefatory notices to each of the five parts of the volume.

The sociology of science is sometimes defined as a part of the sociology of knowledge, and yet the multifaceted problem of the relations between knowledge and reality (not to speak of the reality of knowledge) is a more general one, at the heart of the larger part of sociology. Studies of religion and ideology, of the mass media and public opinion, and of norms and values, to say nothing of the methodological concerns of sociologists, all implicate the chicken-and-egg question of the interdependence of these two fundamental components of human life in groups. How do existential, everyday experiences mold the ways in which people conceptualize the world? How, in turn, do their conceptualizations influence their actions *in* the world, and how, further, do they react to discrepancies between what they “know” and what they experience?

It is perhaps because *Wissenssoziologie*, the sociology of knowledge, in a sense defined its concerns so narrowly in the beginning, focusing almost exclusively on trying to reason out the extent to which men’s knowledge is shaped by their interests and experiences, that it had fallen into disarray by the 1930s. Indeed, as Merton’s examination of the field in 1945 (included here as “Paradigm for the Sociology of Knowledge”) demonstrates, this particular question contained within itself the petard by which it would eventually be hoist. To conclude that knowledge is *not* at all molded by men’s experiences would undermine the *raison d’être* of the field, while to conclude that it is altogether so molded would seem tantamount to questioning, if not denying, the validity of all knowledge—including that conclusion. This restricted construction of the problem led to a maze of internal contradictions, a cul-de-sac from which escape had to be sought by beginning anew with different questions.

Such questions were, of course, vigorously pursued in different sectors of the sociological community. Weber’s work on the importance of the Protestant *Weltanschauung* in producing capitalism in Europe had already had a long and effectively controversial history by the time Merton saw its relevance to his interest in the history of science. Durkheim’s work on primitive religion and his orientation to problems in the sociology of knowledge was beginning to attract the notice even of some American sociologists. The task was to put the various problems back into some sort of orderly array.

In the early 1930s, however, Merton’s interest was not primarily in the sociology of knowledge. During his graduate studies at Harvard, he undertook, at the suggestion of the economic historian E. F. Gay, an analytical book review of A. P. Usher’s *History of Mechanical Invention*. Gay liked it and suggested that George Sarton, also at Harvard, publish it in *Isis*, the prime journal in the history of science which he had founded and still

edited. Sarton did so, and he encouraged Merton's interest in the history of science by having him work in the renowned workshop in Widener Library. Noting his growing expertise in this field, Pitirim A. Sorokin recruited Merton to assist him in the studies of the development of science that would make up parts of his *Social and Cultural Dynamics*. This provided valuable experience in focusing on the development of quantitative measures of intellectual development and change, and perhaps paved the way to "prosopography"—"the study of the common background characteristics of a group of actors in history by means of a collective study of their lives"³—which Merton was to employ extensively in his later work.

Merton also studied with L. J. Henderson, the biochemist who had made a place for Sarton at Harvard and who was himself a gifted teacher of the history of science.⁴ He attended the course of lectures in the philosophy of science given by Alfred North Whitehead and the unique course on comparative "animal sociology" in which specialists on a score of social species were brought together by William Morton Wheeler, the dean of entomologists whose omnivorous intellectual appetite included the history of science. And his early work on social aspects of science was monitored by the polymath E. B. Wilson, then associated with the new department of sociology. Merton was thus responding to the many opportunities at Harvard to develop various perspectives on science by going beyond the conventional boundaries of sociology, even though he continued in the department to be the student of Sorokin and, increasingly, of the young instructor, Talcott Parsons.

It was apparently this confluence of varied intellectual currents, rather than immediate developments in the sociology of knowledge, that led Merton to attempt a sociological analysis of the growth and development of science and that laid the foundation for his continuing interest in science as a distinctive social activity. Not that he was at this time unconcerned with the broader conceptual framework in which science could be located. Two papers⁵ testify to this wider theoretical orientation. In 1935 he published in *Isis* a review of recent work in the sociology of knowledge by Max Scheler, Karl Mannheim, Alexander von Schelting, and Ernst Grünwald. In the next year he published "Civilization and Culture," a paper that located knowledge as a distinct focus of sociological interest in rela-

3. For an account of Merton's role in this development, see Lawrence Stone, "Prosopography," *Daedalus* 100 (1971): 46-79.

4. For an account of Henderson's role in sociology, see the introduction to L. J. Henderson, *On the Social System: Selected Writings*, ed. and with an introduction by Bernard Barber (Chicago: University of Chicago Press, 1970); for an account of Sarton's role in shaping the history of science, see Arnold Thackray and Robert K. Merton, "On Discipline-Building: The Paradoxes of George Sarton," *Isis* 63 (1972): 473-95.

5. The Bibliography lists all of Merton's writings cited here.

tion to concepts advanced by Alfred Weber and Robert MacIver. The concept of "culture" covered the realm of values and normative principles, while the concept of "civilization" included theoretical knowledge and practical technique, which tended—abstractly, not concretely—to be more accumulative than culture. In examining these concepts, Merton rejected the positivist interpretation of unilinear accumulation in science which was inherent in Alfred Weber's failure to deal adequately with the interdependence between culture and civilization. He claimed that this led Weber "virtually to revert to a theory of progress. What must be borne in mind is that accumulation is but an *abstractly* immanent characteristic of civilization. Hence *concrete* movements which always involve interaction with other spheres need not embody such a development."⁶

It was at this same time that Merton wrote his dissertation, *Science, Technology, and Society in Seventeenth-Century England* (begun in 1933 and completed two years later). Although the monograph did much to inaugurate the idea of systematic empirical investigation into the social matrix of science, it was not, of course, produced *ab initio*. In the United States, for instance, there had been W. F. Ogburn's major work, *Social Change*, and his paper with Dorothy S. Thomas, "Are Inventions Inevitable?" which developed basic conceptions about the social evolution of science and technology.⁷ Ogburn's longtime research associate, S. Colum Gilfillan, published *The Sociology of Invention* in the mid-30s, setting forth almost forty "social principles of invention."⁸ And in Europe a delegation from the Soviet Union, led by Bukharin, to the Second International Congress of the History of Science and Technology held in London (1931) had produced the volume of contributed papers, *Science at the Cross Roads*.⁹ The most noticed contribution was the essay, "The Social and Economic Roots of Newton's 'Principia'," by Boris Hessen, the director of the Moscow Institute of Physics, which helped to reinforce and to focus interest in the social aspects of scientific knowledge. But, as noted by Robert S.

6. "Civilization and Culture," pp. 110–11.

7. William F. Ogburn, *Social Change* (New York: B. W. Huebsch, 1922; new ed., New York: Viking Press, 1950); W. F. Ogburn and Dorothy S. Thomas, "Are Inventions Inevitable? A Note on Social Evolution," *Political Science Quarterly* 37 (1922): 83–98. See also W. F. Ogburn, *On Culture and Social Change: Selected Papers*, ed. and with an introduction by Otis Dudley Duncan (Chicago: University of Chicago Press, 1964).

8. Chicago: Follett Publishing Company, 1935. The field of study of the social aspects of invention was so thinly populated at this time that Gilfillan was moved to include Merton, then the author of just a few papers in the field, among the eight "fellow students" of the subject to whom the book is dedicated. See also S. Colum Gilfillan, *Supplement to the Sociology of Invention* (San Francisco: San Francisco Press, 1971).

9. London: Kniga Ltd., 1931. Reprinted with a new foreword by Joseph Needham and a new introduction by P. G. Werskey (London: Frank Cass, 1971).

Cohen in his introduction to the recent separate reprinting of that essay,¹⁰ its influence was mostly visible not in Stalin's Soviet Union, where Hessen soon disappeared from view, but in England, where it appeared in the far more discriminating historical work of scientists on the political left, such as Joseph Needham, J. D. Bernal, Lancelot Hogben, and J. B. S. Haldane, and in the rebuttals by such historians as Charles Singer, G. N. Clark, and Herbert Butterfield. In the United States, Hessen's essay and Clark's criticism of it were both taken into account in Merton's monograph.¹¹

It was still too soon, in the mid-1930s, for that monograph to concentrate on the social structure of the emerging scientific community. As its title indicates, Merton's attention was directed to science *in society*, both its emergence as a social institution, fostered by the particular value-complex which was the hallmark of Puritanism, and its response to contemporary social interests (for example, practical problems of military technology, mining, and navigation).

But in the emphasis placed on the values that characterized the seventeenth-century practitioners of science, the foundations had been laid for later work that would trace the ethos of science and use it to define science as a subsystem of society and civilization. Contemporary events probably served to reinforce interest in the problem. The annihilative fate of "non-Aryan science" in Hitler's Germany during the 1930s directed attention to the various social conditions under which science can lose its autonomy; in the paper "Science and the Social Order" (presented in 1937), we find Merton's first allusions to the "norms of pure science" and signs of his developing interest in the structure and dynamics of the scientific community as distinguished from (and later related to) its substantive concerns. As Joseph Ben-David has noted,¹² the concept of the "scientific community" as a collectivity that evolves its own norms and policies was brought into sharp focus by Michael Polanyi from the early 1940s onward, was devel-

10. New York: Howard Fertig, Inc., 1971.

11. Joseph Needham, for example, reports that, having an interest in the history of science and having long been on the political left, it was only natural for him to attend the Congress and to be "very ready to give a sympathetic hearing to the Russian delegation." A few years later, in his *History of Embryology* (1934), he referred favorably to the Hessen essay as providing one model for historical research and, in his foreword to the recently reprinted volume of the Congress (see the reference in footnote 9), he notes that "with all its unsophisticated bluntness, [it] had a great influence during the subsequent forty years." The extent of that influence and the differences between the Hessen and Merton formulations are indicated in I. B. Cohen's review-essay on Merton's book in *Scientific American* 228 (1973): 117-20.

12. Joseph Ben-David, *The Scientist's Role in Society* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971), pp. 3-4; and "The Profession of Science and Its Powers," *Minerva* 10 (1972): 377.

oped by Edward Shils in the 1950s, and became a basic conception in the sociology of science in the 1960s.¹³

Recently, it should be noted, there has been renewed observation that the nature and direction of scientific growth cannot be adequately understood without dealing specifically with the contents of science—its concepts, data, theories, paradigms, and methods. The idea that the development of science can be analyzed at all effectively, apart from the concrete research of scientists, is said to have proven false.¹⁴ The study of science, after all, begins with its product, scientific knowledge, rather than simply with those individuals who occupy the social position of “scientist.” (This, incidentally, may account for the dearth of sociological studies focused on run-of-the-mill or relatively unproductive scientists: so long as science is defined by its research product, those who contribute little directly to that product are difficult to fit into the picture.)

Regarding the strategy of inquiry, however, it can be argued rather forcefully that it is of basic importance, especially in the beginning of sociological inquiry into the subject, to distinguish the behavior of scientists *as* scientists from the details of their “output”—if only to attend to the diverse aspects of doing science and to reduce the number of variables being considered at a given time. A comparable strategy is in fact employed by Thomas S. Kuhn in *The Structure of Scientific Revolutions*,¹⁵ except that there the focus is on the formal organization of scientific knowledge and it is the social variables that need to be successively identified. Sociologically, it was necessary to identify the boundaries of the scientific community and to explore the bases of its place within society before the sociology of science could proceed to a range of other problems. (Indeed, the question of why science becomes established in any society, when most people can neither profit directly from the work of scientists nor comprehend and appreciate what they are doing, forms the central problem to which Joseph Ben-David addresses himself in his recent book on *The Scientist's Role in Society*.¹⁶)

13. Polanyi's early paper of 1942, “Self-Government of Science,” is included in his collection of essays, *The Logic of Liberty* (Chicago: University of Chicago Press, 1951), pp. 49–67; the general idea is developed in his many later books (see, for example, *Personal Knowledge* [Chicago: University of Chicago Press, 1958]). Edward Shils, “Scientific Community: Thoughts After Hamburg,” *Bulletin of the Atomic Scientists* 10 (1954): 1151–55, reprinted in Edward Shils, *The Intellectuals and the Powers, Selected Papers*, vol. 1 (Chicago: University of Chicago Press, 1972), pp. 204–12. The developments in the 1960s are considered later in this introduction.

14. See, for instance, Barnes and Dolby, “The Scientific Ethos”; Mulkay, “Some Aspects of Cultural Growth”; and M. D. King, “Reason, Tradition, and the Progressiveness of Science,” *History and Theory* 10 (1971): 3–32.

15. Second ed., enlarged (Chicago: University of Chicago Press, 1970).

16. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971.