THE IDEA OF SOCIAL STRUCTURE

Papers in Honor of Robert K. Merton

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The Emergence of A Scientific Specialty: The Self-Exemplifying Case of the Sociology of Science*

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OVER the course of their careers, all working scientists have the opportunity to observe the growth of new fields of inquiry and the demise of old ones. Yet the emergence of new scientific specialties as cognitive and social entities seems to be a fact of the modern scientific life that is little understood. Physical and biological scientists have understandably been impatient to get on with their own work and few have paused to examine the emergence of one or another special field.[1] Sociologists of knowledge have also not shown much interest in questions about the growth of scientific knowledge. They have occupied themselves primarily with inquiries into the social and existential bases of knowledge. It was not until a few sociologists began to study science as a social institution that more serious inquiry into the growth and differentiation of specialties began. In short, the emergence of scientific specialties became interesting only when a new scientific specialty came into being.

The sociology of science is curiously self-exemplifying. As a scientific specialty, it exhibits many of the social patterns its own practitioners study in other contexts, making it a convenient site for sociological study of emerging

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specialties. It is evolving its own system of stratification, its own arrangements for formal and informal communication, its own politics and its own lines of cognitive and social conflict just as these have become major foci of attention in research by sociologists of science. As participants and observers of these developments, we are in a strategic position to examine how the growth of knowledge in a special field is related to the emergence of its organizational infrastructure.

Tracing the emergence of the sociology of science also provides us with an occasion for examining Robert Merton's contributions to these developments. Since he has shown an almost obsessive fascination with scientific paternity—assiduously cataloguing the fathers of numerous sciences in his analysis of priority and eponymy in science[2] and more recently focusing on George Sarton's efforts to institutionalize the study of the history of science[3]—it seems fitting that he himself now become a subject of inquiry as a father of the sociology of science.[4]

THE SOCIOLOGY OF SCIENTIFIC SPECIALTIES

After a long and desultory incubation, the sociology of science now seems to have acquired its own cognitive and professional identity. In the last decade or so, sociological investigations of science have focused primarily on its social organization, in particular, on its reward-and-evaluation-systems, its system of communication and its ethos. Mapping these organizational features of science seemed to many investigators a necessary and congenial set of first steps toward understanding how scientific knowledge grows, becomes codified and institutionalized. By contrast, comparatively little attention has been given to studying the interplay of science and other social institutions.[5]

The research attention of sociologists of science may now be shifting. In the last few years, there has been increasing discussion of the connections between cognitive structures of the sciences and their social structures and some efforts to study them empirically.[6] Sociologists of science are now turning to such problems as the extent to which there is consensus among scientists in different disciplines on theory, method and substance, and whether there are systematic differences in social organization between sciences which can be linked to their differential degrees of theoretical codification. These studies derive in part from Thomas Kuhn's work on scientific revolutions,[7] Derek Price's studies of the parameters of scientific growth[8] and from the work of philosophers of science such as Karl Popper and Imré Lakatos.[9] It is in this intellectual context that recent studies of growth and institutionalization of scientific specialties can be located.

Kuhn's model of revolutionary change in the sciences has been especially influential in studies of scientific specialties. He focuses of course on the birth of new theoretical perspectives, or paradigms, on the recruitment of adherents to the new viewpoint and on the cognitive conflict attendant on the revolution. By working through the implications of the new paradigm and

gaining intellectual and social dominance, new recruits ultimately revolutionize the discipline. Even though Kuhn's analysis of paradigms and their role in scientific change has been widely criticized by historians and philosophers of science,[10] sociologists of science have continued to find it useful. As a consequence, sociological studies of the emergence of specialties in the Kuhnian mode have focused especially on cases representing major breaks with disciplinary tradition.[11]

Working from another perspective, Derek Price re-introduced the seventeenth century term, "invisible college"—originally used to describe the pioneer members of the group which later became the Royal Society of London—to characterize the informal network of investigators he takes to be the core of any specialty.[12] He links this emphasis on patterns of communication to his other work on rates of growth in scientific manpower and the scientific literature. The impact of both Kuhn and Price is easily discerned in sociological studies of the emergence of specialties.[13] But there are still no agreed-upon problematics for studying the institutionalization of specialties.

Most sociologists of science have assumed that patterns of growth and institutionalization are much the same in different specialties. This assumption ignores the variegated processes that characterize the birth of new specialties. The first step in understanding institutionalization of specialties is more precise description of their cognitive and social development with more systematic attention being given to *variability* in specialty differentiation and its sources. Griffith and Mullins[14] have hinted that the emergence of "elite" specialty groups may differ in important ways from what they call "revolutionary" groups but the implications of this distinction are not explored.

The cognitive orientation of new specialty groups should be a strong determinant of its rate of institutionalization and its successful establishment. "Cognitively radical" specialty groups which reject the legitimacy of established theoretical and methodological orientations should encounter more intellectual resistance, more difficulty in obtaining resources and recruits and engender more conflict in the process of their development than "cognitively conforming" specialties. The latter base their claims to specialty status on inquiry into new and previously unexamined phenomena or on the use of new research technologies and thus do not challenge prevailing views. The distinction between cognitively radical and cognitively conforming specialties seems central to understanding how scientific specialties emerge. It suggests why the sociology of science for example—as a specialty with a new subject matter—has encountered relatively little resistance albeit no great enthusiasm from sociological colleagues. We shall take up this matter again when we consider Merton's role in the development of the specialty.

The cognitive standing of a new specialty is only one element affecting its reception. Its goodness of fit with the prevailing structure of the academy should also affect its chances for survival and, if it survives, its pace of institutionalization. Those that grow up in the interstices between disciplines, such as biochemistry, astrophysics or social psychology, are thwarted by the lack of

ready-made academic niches; handicapped by the poor meshing of their intellectual interests with those of deans, journal editors, grant givers and reviewers, and other gate-keepers of resources.[15] These specialties are, in short, structurally atypical regardless of their cognitive content and should encounter different and more challenging functional problems than those, such as nuclear physics or the sociology of science, which have been firmly located within the bounds of established disciplines from their beginnings.

There is, then, some reason to suppose that the development of scientific specialties is highly variable. A paradigm in the early, Mertonian sense is needed that provides for systematic and comparative examination of the cognitive development of specialties and their institutionalization. Such a program would focus attention on at least three sets of problems and processes.

1. *Parameters of growth in personnel and in production of a literature, as they change over time.* Do they grow simultaneously? Do they exhibit patterns of lead and lag? To what extent are they independent of one another or causally connected? How are they linked to the cognitive state of specialties?

2. *Processes of Cognitive Development of Specialties.* To what extent is there consensus or conflict on problematics, methods of inquiry, and on principal contributors at various stages in specialty development? Are ongoing developments built upon a common base or diverse intellectual foundations? How rapidly are new contributions exploited and built upon? How are theory and empirical research linked, if at all, and how do these linkages change over time? When does theoretical codification begin, if ever, and what are its indicators? How do foci of attention in the specialty shift and are these shifts related to changes in the intellectual orientations of leading authors or influentials? What are the distinctive contributions of the founders to the cognitive development of the specialty? How long do their contributions continue to be used and in what ways?

3. *The Development of Organizational Infrastructures.* How rapidly is the specialty incorporated into the educational curriculum and how much resistance does it encounter? In what respects do the social and historical contexts of institutionalization affect its pace and success? How rapidly and from what sources are funds and facilities acquired and with what consequences for cognitive development? What provisions are made for routinizing communication between specialists? In what ways, if at all, is the system of communication among specialists connected to the *general scientific* communication system? How effective have its intellectual founders been as institution builders? Are the tasks of organizing the specialty assumed by its intellectual leaders or is there a division of labor in these activities? And finally, when do specialists develop a sense of professional identity such that they consider themselves, and are considered by others, to be working at a common task?

Having repeatedly referred to cognitive elements in science, we note that the concept of cognitive structure has not yet been defined. In fact, its precise

meaning for sociologists of science is still evolving. At present, sociologists of science, cognitive psychologists, and philosophers of science focus on different aspects of cognitive structures. Sociologists regard these structures as multidimensional; they include:

1. scientific knowledge as it is reported in theoretical and experimental investigations;
2. the standards by which scientists judge methods, instruments, techniques, and evidence to be acceptable;
3. theoretical orientations which provide criteria for assessing the significance of new problems, new data, and proposed solutions;[16]
4. commonly accepted problematics for further inquiry; and
5. responses to new contributions, particularly the extent and forms of consensus and dissensus.

The cognitive structures of the separate sciences and specialties differ from one another and vary over time.

Analysis of cognitives structures of the sciences from a sociological perspective attempts, for example, to identify their basic theoretical orientations and to determine whether one predominates or whether several compete for scientists' attention. It attempts to determine whether fields or specialties are intellectually fragmented or cohesive; the extent to which theories are interconnected; how intellectual work is organized in terms of substantive problems and theoretical schools; and how theory and experiments are related.

Development and elaboration of the cognitive structure of new specialties appear to depend in part on correlative development of their social structures —on the routinization of an evaluation and reward system, procedures of communication, acquisition of resources and the socialization of new recruits. In short, the tandem development of both cognitive and social structures of specialties seems central to their institutionalization and establishment as legitimate areas of inquiry. Since institutionalization is more usefully considered as a process than as a product, studies of the decline of specialties[17] should be on the agenda for sociologists of science along with studies of their development.

We turn now to the sociology of science as a case study in the emergence of a specialty. Laying claim to the institution of science as a legitimate subject for sociological inquiry, the specialty is cognitively conforming and located firmly within the established disciplinary structure of sociology although it has increasingly elaborate connections to other fields, principally the philosophy and history of science. These cognitive and structural attributes suggest that its growth and institutionalization should be comparatively untraumatic, uncontested and more rapid than specialties which are cognitively or structurally radical. Had we studied the emergence of platetectonics or numerical taxonomy, two cognitively radical specialties, quite different patterns of growth might well have been found. Still, as we shall see, the sociological study of science despite its benign character was not enthusiastically embraced by sociologists when it was first introduced.

PARAMETERS OF GROWTH

Although Robert Merton believed that sociological analysis of science was a promising line of inquiry in the mid-1930s, few shared his enthusiasm. He recruited his student and friend Bernard Barber to work on social aspects of science, but their efforts to convert others mostly failed. By 1949, just one percent of the members of what was then the American Sociological Society counted the sociology of knowledge among their three fields of competence;[18] no separate tabulation was even made for the sociology of science. A decade later, sociologists had not changed their minds. The *Directory* for 1959 shows that just one percent of the membership reported competence in the sociology of knowledge. Again in 1970, things were much the same: 1.4 percent of the American Sociological Association's membership who answered the questionnaire on areas of competence—about two-thirds of the total of 13,000—selected the category newly rechristened as the "sociology of knowledge and science" as one of two principal areas of competence.[19] But by 1973, the figure had risen to 2.2 percent, or 301 out of a total of 13,700.

These figures need to be seen in context. First, the most frequently mentioned specialty in 1970 was selected by 9 percent of all sociologists and only four of the thirty-three specialties were selected by more than 5 percent. Second, between 1970 and 1973, the overall membership of the association increased by 5.4 percent, but the numbers reporting competence in the sociology of knowledge rose by a half, suggesting that interest in the composite field grew considerably even though the small numbers involved produce rather high rates of change.

More intriguing than data on increasing numbers are differences between age groups in declarations of competence in the sociology of knowledge and science. Stehr and Larsen find that the specialty is mentioned most often as an area of competence by the youngest members of the American Sociological Association. For sociologists in their 20s, it ranked 17th out of the 33 mentioned, 22nd for those in their 30s, 26th for those in their 40s and 28th for the oldest cohorts who were in their 50s and 60s.[20] These figures imply a growing interest in the field among the young and no marked pattern of conversion among the old.

Information on the changing subject matter of doctoral dissertations is consistent with this interpretation. Drawing upon listings in *Dissertation Abstracts* and the new *Comprehensive Dissertation Index, 1861–1972,* we took inventory of dissertations on science and technology written by students in sociology departments in American universities.[21] In all, we identified 105 titles, 65 in the sociology of science, 28 in the sociology of sociology and 12 in the sociology of engineers, engineering, and technology. The two earliest, both submitted in 1929, were sociological studies of sociology.[22] Seven years went by before S. C. Gilfillan, then a student at Columbia, finished his study of the sociology of invention for his sponsor, W. F. Ogburn. The same year, Robert Merton submitted his study of sociological aspects of scientific development in seventeenth century England to his doctoral committee at Harvard. That committee included the dean of historians of science, George Sarton, the dis-

tinguished physiologist and Pareto scholar L. J. Henderson, and the sociologists P. A. Sorokin and Talcott Parsons.

Despite these auspicious beginnings, few graduate students tried their hands at dissertations on the social aspects of science for some time. Just 25 dissertations were completed in the thirty years between 1937 and 1967. But things changed markedly after that. In the five years that followed, 40 dissertations were turned in, suggesting a sudden increase in interest in the specialty. This number is about 1.4 times as large as would be expected if dissertations in the sociology of science were multiplying at the same rate as they did in sociology as a whole.[23] Students writing dissertations in the sociology of science have been clustered at a few departments: Columbia, Cornell, Chicago, Purdue, and Michigan State, in that order, account for almost half of all dissertations in this specialty. The same universities produced only 28 percent of all Ph.D.'s in sociology in roughly the same period.[24] It would seem that a critical mass of students may be developing at a small number of research centers.

Such data only hint at the social processes of specialty growth. Consider the following questions: Do all specialties require an equal number of students to get moving? How rapidly must their numbers grow? What is the ratio between masters and apprentices required for sustained growth? Do new specialties require a higher density of talented newcomers than established ones? What constitutes a "critical mass" of students and practitioners needed for the development of a cognitive and professional identity? How important for the recruitment of students and the forging of a group identity are the personal characteristics of leaders in the field? How much proselytizing is needed? And what form does missionary work take before the specialty gains legitimacy among potential recruits, young and old?

While a growing number who claim an interest and competence in a specialty would seem to signal its developing professional identity, a growing literature indicates an authentic commitment on the part of practitioners to work in a field. Price and others[25] who work on parameters of scientific growth report almost without exception that the literature of scientific disciplines and scientific specialties first grows slowly and erratically and then exponentially, with a doubling time of ten to fifteen years.[26] In order to examine the growth and texture chiefly of the American literature in the sociology of science, we compiled a bibliography and citation index for publications appearing in nine scholarly journals since 1950.[27] In all, 195 papers published over 24 years were identified.[28] In its early years, the specialty had no clear intellectual identity, no shared problematics or techniques of investigation. Early papers were often vague and speculative. It is not surprising that it was far more difficult to decide whether papers published in the nineteen fifties belonged in the bibliography than those published later on.

As in the literature of disciplines and specialties previously examined, the number of papers in the sociology of science published since 1950[29] has grown exponentially with a doubling time of five to eight years. Taking all 195 publications into account, a third or so (37 percent) were published between 1958 and 1965 and almost half (48 percent) since 1966.[30] In all then, 85 percent of all papers we identified appeared after 1957, the year of Merton's influential

paper on priorities in scientific discovery.[31] We also find a marked increase in the relative number of papers reporting quantitative studies in the sociology of science, with 38 percent of the papers published in the 1950s being quantitative in one or another respect, 52 percent of publications in the 1960s and 56 percent of those appearing in the 1970s. There is little doubt that these data represent an authentic change in the character of research in the sociology of science in recent years, but they clearly do not tell the whole story. Content analysis of books and monographs is needed before firm conclusions can be drawn about changes in the extent of quantification and its relation to other aspects of cognitive development.

Although a growing literature may indicate increased scholarly effort, it is not necessarily evidence for a shared intellectual focus among those at work in the specialty. There are however other reasons to think that such a focus was emerging: among them a growing consensus on the usefulness of particular publications, a consolidating research front in which new papers built directly upon those just published, and increased rates of collaborative publication. We want now to consider these and other indications of a developing cognitive structure in the specialty.

SELECTED ASPECTS OF COGNITIVE DEVELOPMENT

COGNITIVE CONSENSUS

A growing consensus among specialists on the usefulness of certain publications is a prime indicator that a specialty is developing distinctive problematics and thus a cognitive identity. The extent of convergence of citations to particular papers and to the work of particular authors is a rough measure of such consensus. If there were no common orientation in the specialty, citations would be widely dispersed among cited authors. The emergence of a common orientation however should be reflected in increasingly large proportions of citations going to the work of a small group of influential authors whose work is judged useful.[32] Converging citations do not mean that all agree on the significance of cited research or that all highly cited authors have a common orientation but only that the cited work is influential in some respect.

Three measures of convergence of citations or consensus are employed here: the proportion of cited authors receiving two or more citations, the proportion of citations going to the top 10 percent of cited authors, and the overall concentration of citations as measured by Gini-coefficients. The first is a rough gauge of the extent of dispersion of citations among cited authors while the latter two are more sensitive measures of the same variable. Drawing upon references in the 195 papers, these three measures were computed for five successive time periods and the results are presented in Tables 1A through 1C.

Consider first the findings for the sociology of science reported in Columns 2 and 3 of Table 1A. (Comparisons with the sociology of deviance will be made presently.) Only 18 percent of all authors cited in the early 1950s were cited more than once. By the early 1960s, that proportion had increased

slightly to 26 percent. A substantial increase in focusing can, however, be observed by the late 1960s. Nearly half of all cited authors were cited two or more times, suggesting growing agreement about the usefulness of work by particular authors. The data reported in Columns 2 and 3 of Table 1B for the sociology of science show an increasing reliance on the work of a few authors. In the early 1950s, there was little consensus in the sociology of science about whose research was useful. The top 10 percent of all cited authors received approximately one-quarter of all citations. Twenty years later, the top 10 percent received 44 percent of all citations. The Gini-concentration ratios reported in Column 2 of Table 1C tell a similar story. In the early years, there is considerable dispersion in the distribution of citations. Later on, in the 1960s and 1970s, the data show increasing concentration in the distribution of citations, suggesting the emergence of a cadre of recognized intellectual leaders and convergence in judgments of usefulness among those publishing in the journals examined.[33]

Table 1A. COMPARISON OF LEVELS OF COGNITIVE CONSENSUS FROM 1950–1973 IN SOCIOLOGY OF SCIENCE AND SOCIOLOGY OF DEVIANCE

	Percent of Cited Authors Receiving Two or More Citations			
	Sociology of Science		Sociology of Deviance	
Period	Percent	Total Authors Cited	Percent	Total Authors Cited
1950–54	18	(148)	44	(242)
1955–59	24	(323)	50	(538)
1960–64	26	(571)	53	(884)
1965–69	44	(775)	52	(1347)
1970–73	45	(899)	45	(723)*

*The literature in the sociology of deviance covers the period 1950–72 inclusive.

Table 1B. PERCENT OF CITATIONS RECEIVED BY TOP 10 PERCENT OF CITED AUTHORS

	Specialty			
	Sociology of Science		Sociology of Deviance	
Period	Percent	Total Citations	Percent	Total Citations
1950–54	24	(198)	36	(1178)
1955–59	28	(444)	38	(2753)
1960–64	35	(931)	49	(5560)
1965–69	43	(1698)	46	(7469)
1970–73	44	(2145)	36	(3514)*

*The literature in the sociology of deviance covers the period 1950–72 inclusive.

Table 1C. GINI-COEFFICIENTS FOR DISTRIBUTION OF CITATIONS IN FIVE TIME PERIODS*

Period	Sociology of Science	Sociology of Deviance
1950–54	.22	.23
1955–59	.23	.29
1960–64	.33	.40
1965–69	.42	.48
1970–73	.47	.30

*We are indebted to Stephen Cole for providing us with these data for the sociology of deviance. Extended discussion of these and other measures of intellectual structures can be found in his paper in this volume. See O. D. Duncan, "The Measurement of Population Distribution," *Population Studies* 11 (July 1957): 27, for discussion of the Gini-coefficient.

As we noted earlier, the sociology of science derives its claim to specialty status from its focus on phenomena not previously studied by sociologists. In the early fifties, there was no relevant literature to cite, to respond to critically, to correct, or to elaborate. Each author who published in those early years brought his own highly individualized apperceptive mass to bear on his research. Only as the intellectual identity of the field became fixed did a pattern of increasing consensus on influential authors appear. Once a specialty becomes a recognizable entity, however, the extent of consensus on influential authors need not continue to grow. It may even decline, if rival theoretical or methodological orientations develop. Such is apparently the case in the recent history of the sociology of deviance. (See Stephen Cole's essay in this volume, pages 175–220.)

As Tables 1A and 1C suggest, the larger literature on deviance contains more citations. More authors are cited even though the data are drawn from just four journals, as compared with the nine sampled for the sociology of science. Since "deviance" was the fifth most "popular" on the list of thirty-three areas of sociological competence selected by members of the ASA in 1970 and the sociology of science and knowledge, twenty-fifth, the differences in the size of their journal literatures are not surprising. The significant fact however is that these literatures also contain differences of dispersion of citations over time. The sociology of deviance shows a curvilinear pattern while a linear increase is observed for the sociology of science. Table 1A shows that nearly half (44%) of all authors cited in the deviance literature between 1950 and 1954 received multiple citations. Ten years later that proportion increased to a high of 53 percent. In the next ten years the rate of multiple citation began to taper off, reaching 45 percent for the current period.

Consider now the more sensitive indicators of consensus. The sociology of deviance shows some convergence in judgments about its most influential authors in the early 1950s: the top 10 percent of authors cited in the journals received 36 percent of all citations in this period. Cognitive consensus in the deviance literature by this measure increased and peaked in the years 1960–

1964, when the top 10 percent received almost half of all citations, the same level as has been observed in physics.[34] The Gini-coefficients indicate the same pattern. These years were the high point in the functional analytic study of deviant behavior, as Stephen Cole's essay in this volume reports (pp. 188–205). Toward the end of the 1960s, however, the functional orientation came under attack by ethnomethodologists and symbolic interactionists. The development of these rival perspectives appears to be reflected in the citation data.

Similar developments are not yet discernible in the referencing behavior of sociologists of science. Although in recent years some young English and European sociologists of science have proposed what they consider to be an alternative to the perspective exemplified in Merton's work, their papers are still largely programmatic and critical. They continue to cite Merton and those pursuing similar inquiries and to use their research.[35] Thus the global citation measures of consensus presented here are not an early warning system for cognitive conflict but only provide cues to the emergence of alternative and competing orientations after such orientations are embodied in new self-contained research literature.

THE CONSOLIDATION OF A RESEARCH FRONT

We have observed increasing convergence in citations in the sociology of science and have suggested that this is a signal of developing intellectual coherence and consensus. Such a convergence may indicate a consolidation of new work or mere reiteration of older inquiries. In order to determine whether current work is built on comparatively new contributions rather than on older ones—that is, whether a research front has developed and when—we need to examine the age of cited papers. Derek Price has observed that active research fields rely heavily on recent publications. Citations in active research areas are relatively younger than would be expected on the basis of sheer growth in the literature. This he calls "immediacy."[36] It has also been suggested that rates of citation to recently published work are correlated with the extent of theoretical codification in a science such that the more codified sciences exhibit higher proportions of citations to newly published work. This should be so because codification of theoretical and empirical knowledge makes it possible to identify the connections between new work and old; to gauge the significance of new contributions; and to facilitate their rapid incorporation into ongoing work.[37] Thus, if a specialty were becoming increasingly codified, the age of publications being cited in its literature should decline and "immediacy" measures should rise.

This is precisely what we observe in the literature of the sociology of science. In the first half of the 1960s, when the specialty appeared to "take off" and move toward a high growth rate and institutionalization, cited papers were an average of twelve years old, 48 percent of citations went to papers published in the preceding five years. Between 1965 and 1969, the average age of cited papers dropped to nine years and 56 percent of the references were to papers published no more than five years before. These data on citation of

recent publications can be better understood when juxtaposed with figures on production of journal literature in the sociology of science. Although 48 percent of citations in papers published between 1960 and 1964 went to literature published in the same five years, over half the literature (57 percent) then in print appeared during this period. This makes for an immediacy score of −9 percent, a figure that would surely have been larger if literature published before 1950 had been surveyed. Thus in the years when the sociology of science began to grow rapidly, recent research was not used as quickly as it was produced. In the following five years, things changed markedly. Thirty-six percent of the total literature was published, but 56 percent of citations went to recent work, making for an immediacy score of + 20 percent, a figure comparable to rapidly growing specialties in the physical sciences and much higher than that for sociology as a whole.[38]

Although this trend toward citing new publications may be related to the influx of young people into the field and their characteristic interest in new work, it is not confined to their publications. References in Merton's own work reflect this increasing reliance on new research. An avid student of the history of science and obsessed with documenting the filiation of ideas, Merton punctuated his early papers with references to works several centuries old. His recent papers, however, reveal that his perspective has shifted along with those of other sociologists of science. References in his own most cited papers in the sociology of science, the "Priorities" paper published in 1957 and "The Matthew Effect" published in 1968,[39] exemplify this trend, with the median age of references in the "Priorities" paper being 18 years and in "The Matthew Effect," 8 years. Not surprisingly, references in the same papers also reveal Merton's increased reliance on very recent work: 29 percent of the references in the "Priorities" paper were to publications no more than five years old as against half of the references in "The Matthew Effect." This does not, we think, reflect a conviction that pertinent older references are already well established in the bibliography of the specialty and thus no longer require citation even though they are used. Rather it is testimony to a genuine shift of attention to newer publications.

THE AGE STRUCTURE OF INFLUENTIALS

Turning from the age of citations to the ages of authors being cited, we see one of the important consequences of the pattern of recruitment among the young that we noted earlier. Not only is the sociology of science more popular among young sociologists, but their intellectual influence on the field has also increased dramatically. Although there are reasons for supposing that the age of contributors of important work to a science declines as that science becomes more codified,[40] there is no reason to think that youthful investigators are primarily responsible for increasing the extent of codification; rather it enables them to deal with important problems in the field. Keeping this observation in mind, we note the decline in average age of the thirty most-cited authors in successive five-year periods. It drops from 53 for the years 1950–64 to 46 for the

years 1965–72. The principal shift occurs between two five-year periods: 1960–64 when the mean age of influentials was 54, and 1965–69 when it fell to 44.[41] These data also reveal a slight increase in the age of influentials in the 1970s, a finding easily understood when we note that the young people who first became influential in the late 1960s continue to be so in the 1970s. In fact, the lists of the thirty most influential authors in the late sixties and early seventies overlap by 60 percent. Since each author on both lists was growing older, their average ages increased. New entrants among the influentials in the seventies were not young enough to make up for this trend.

COLLABORATIVE PUBLICATION

As specialties become organized around a set of problems, the extent of collaboration between specialists increases. This is one outcome of greater numbers simultaneously at work in research centers, increased requirements for specialized skills, and greater agreement on the nature of researchable problems. There is a marked increase in collaborative publication over the twenty-four years covered by our bibliography. In the first eight years, not one of the 29 papers abstracted had more than one author, but in the second 14 percent, or 10 out of 72, were multi-authored; and in the third, 31 percent or 29 out of 94 papers were collaborative. Altogether, only 3 percent of the 195 papers had three authors and none more than that. The figures for the last period are slightly lower than that for the journal literature in sociology as a whole.[42]

THE STRUCTURE OF INFLUENCE IN THE SOCIOLOGY OF SCIENCE

Thus far three points emerge from citation analysis of the journal literature of the sociology of science: (1) consensus on the work of particular contributors has been growing; (2) younger authors are increasingly represented among contributors cited most frequently; and (3) recent publications are more often cited now than in earlier years. Nonetheless Robert Merton has had greater influence on the evolving cognitive identity of the field from its beginnings than any other author. His impact on his colleagues' research is signaled by the extent of citation to his work. With a total of 154 citations in the journal literature surveyed here, his work is used roughly twice as often as that of the second most cited author in our list.[43] But apart from Merton and several others, the social and intellectual composition of the most influential group of authors has changed greatly. These changes are cues to marked shifts in foci of attention in the field and in types of work being published. (Given the high correlation in science between intellectual influence and authority, we suspect that they are also cues of changes in the intellectual composition of those occupying gate-keeping positions.) We increase the resolution of our citation analysis to the micro level by turning now to the question of continuity and change in the influence structure in the sociology of science.

Table 2 presents rank-ordered lists of the authors most often cited in the journal literature of the sociology of science in five time periods.[44] We have

Table 2. MOST CITED AUTHORS IN THE SOCIOLOGY OF SCIENCE, 1950–1973 (SELF-CITATIONS EXCLUDED)

Period and Rank Order				
1950–54	1955–59	1960–64	1965–69	1970–73
Gilfillan, S. C.	Merton, R. K.	Merton, R. K.	Merton, R. K.	Merton, R. K.
Lundberg, G.	Lazarsfeld, P. F.	Crombie, A. C.	Price, Derek	Price, Derek
Dewey, J.	Gaudet, H.	Barber, B.	Garfield, E.	Hagstrom, W. O.
Hart, H.	Wilkening, E. A.	Gillispie, C. C.	Hagstrom, W. O.	Cole, J. R.
Parsons, T.	Wilson, L.	Lazarsfeld, P. F.	Zuckerman, H.	Ben-David, J.
Merton, R. K.	Stimson, D. L.	Kornhauser, W.	Gordon, G.	Cole, S.
Weber, M.	Compton, A. H.	Flexner, A.	Glaser, B.	Zuckerman, H.
Shils, E.	Kellner, A.	Goodrich, H. B.	Garvey, W. D.	Gaston, J. C.
Conant, J. B.	Robertson, T.	Kuhn, T. S.	Kessler, M. M.	Kuhn, T. S.
Leighton, A. H.	Parsons, T.	Caplow, T.	Cartter, A. M.	Crane, D.
Isard, W.	Richards, I. A.	Shepard, H. A.	Ben-David, J.	Barber, B.
Kautsky, K.	Sarton, G.	Shryock, R. H.	Barber, B.	Cartter, A. M.
Lerner, D.	Ryan, B.	Wilson, L.	Pelz, D.	Glaser, B.
Lasswell, H. D.	Kluckhohn, C.	Glaser, B.	Cole, S.	Ogburn, W. F.
Kuhn, T. S.	Gross, N. C.	Gilfillan, S.	Cole, J. R.	McGee, R.
Chase, S.	Berelson, B.	Holland, J. L.	Gamson, W.	Parsons, T.
Durkheim, E.	Shepard, H. A.	Marcson, S.	Kaplan, N.	Polanyi, M.
Corey, L.		McGee, R.	Storer, N. W.	Shils, E.
Goren, G.		Pelz, D.	Lazarsfeld, P. F.	Storer, N. W.
Ogburn, W. F.		Parsons, T.	Kuhn, T. S.	Gouldner, A. W.
Gold, H.		Knapp, R. H.	Berelson, B.	Gordon, G.
Gee, W.		Price, Derek		Caplow, T.
Myrdal, G.				Watson, J. D.
Usher, A. P.				Pelz, D. C.
Sibley, E.				Hirsch, W.
Whitney, V.				Hargens, L. L.
				Berelson, B.
Number of Citations:				
Range 8–2	Range 12–3	Range 32–5	Range 39–9	Range 67–9

focused on the approximately twenty most-cited authors in each period because patterns are difficult to discern in longer lists. Our more extended enumerations show many of the same attributes as the shorter ones, and in no way contradict our general observations.

The list of most-cited authors in the first decade is striking in several respects. Major figures in the wider discipline of sociology—both historical and contemporary—seem to have dominated the literature. Since the sociology of science had not yet developed its own intellectual identity in the form of subject-specific ideas and techniques, specialists applied what they could from the prevailing theoretical and methodological corpus. Thus, Paul Lazarsfeld is among the most highly cited authors in the 1950s. Closer inspection of citations to Lazarsfeld's work reveals that it is his logic of multivariate analysis that is most often used, not his work in the history of quantification in sociology, which he had not even begun to publish at this time. We can detect no clusters of researchers on the list who worked on related problems and no identifiable similarities in the problems they addressed—in short, no signs of a shared intellectual orientation.

Table 2 also shows that few authors frequently cited in the early 1950s are included among those often cited in the later 1950s. In order to convey the extent of continuity from one period to the next, we constructed the transition matrix—or partial "turnover table"—that appears as Table 3. Two estimates of continuity are presented here. The first, located *above* the main diagonal, shows the proportion of authors in the most-cited decile in one period also in the most-cited decile in subsequent periods. Since the number comprising the most-cited decile grows as the number of cited authors grows, this measure of continuity is less demanding than the second measure, presented *below* the diagonal in Table 3. This second set of data reports the proportion of the thirty most-cited authors for each period who also appear in subsequent periods. Over time, the top thirty make up smaller and smaller proportions of all cited authors and thus represent an increasingly elite group.

Table 3. CONTINUITY AMONG INFLUENTIAL CONTRIBUTORS TO THE SOCIOLOGY OF SCIENCE

(THE MOST CITED 10 PERCENT ARE PRESENTED ABOVE THE DIAGONAL; THE THIRTY MOST-CITED INDIVIDUALS, BELOW THE DIAGONAL.)

Percent Carried Over Between Periods

Period	1950–54*	1955–59	1960–64	1965–69	1970–73
1950–54	—	13	33	27	40
1955–59	8	—	19	22	19
1960–64	15	20	—	40	37
1965–69	12	17	27	—	47
1970–73	19	13	30	60	—

*The base figure for continuity for the period 1950–54 was 26, since there were only 26 of the 148 cited authors whose work was cited two or more times.

Confining ourselves to the extent of continuity of influentials in adjacent periods, we note a sharp increase in continuity in the most-cited decile in the late 1960s, as the data above the main diagonal show. The extent of continuity in the top decile approximately doubled from nineteen to forty percent in the 1960s and then increased slightly to forty-seven percent in the early 1970s. Data reported below the main diagonal show the same pattern even more sharply. Only two of the thirty authors (Parsons and Merton) whose work was most often used in the early 1950s also appear among those most often cited in the later 1950s. The extent of continuity increases somewhat in the 1960s but it is not until the early 1970s that a major shift in continuity is discernible. As many as eighteen out of the top thirty, or 60 percent, of the most-cited authors appear on both lists. Work in the sociology of science finally was focused on an identifiable set of problems, formally presented in the publications of a limited number of authors. Further, these same authors continued to work in the field and to produce research that was useful to their colleagues instead of moving on to other areas of substantive concern. This marked increase in continuity of influentials between the late 1960s and early 1970s is also reflected in the aging of influentials in these years reported earlier.

For those familiar with the literature of the sociology of science, a glance at the names of the most-cited authors listed in Table 2 will immediately convey the extent of change in the intellectual interests of the influentials. The lists are dominated after 1965 by sociologists of science and a handful of historians and philosophers of science. Distinguished figures from the physical, biological, and other behavioral sciences disappear from the list of most-cited authors.[45] The sociologists of science whose work is frequently cited are, with notable exceptions, quantitative empirical researchers. Given the increasing number of quantitative studies published and the tendency for authors of such studies to cite prior work of the same kind, this finding is not surprising. It does suggest that citation analysis of journal articles may overestimate the impact of quantitative empirical research and underestimate the role of authors who do not fit this mould. T. S. Kuhn is of course the most conspicuous example of this group.

These lists contain cues to a second kind of continuity among influentials: not simply across adjacent time periods but through more extended periods of time. Although Merton alone turns up in each of the five lists covering twenty-three years of literature, Thomas Kuhn, Derek Price, Bernard Barber, and Donald Pelz, all appear on the last three lists.[46] Thus, at least a small number of leaders have been working at the research front for some time. Mullins' contention that leaders of research specialties often turn to new problems before their specialty is fully institutionalized is not borne out in this case.[47] There are no signs that Merton, Price, or Kuhn, among the other leading influentials, are turning away from their interest in the sociology of science.

Thus we return once again to the question we raised earlier in other contexts: How much variability is there in the emergence of new specialties? Under what conditions do research leaders lose interest in specialties they helped to establish? Are founders of "cognitively radical" specialties, and those

which are structurally atypical more apt to move on to entirely new problems than leaders of conforming-specialties? Or does the exodus of founders depend on the extent to which the specialty remains fertile ground for studying fundamental issues.

"Insiders" in the sociology of science will also notice a third kind of continuity among the most cited authors listed in the fourth and fifth columns in Table 2. For the first time, a significant number of students of influentials appear along with those who trained them. Of the thirty-three different authors who were cited most between 1965–73, seven are Merton's former students.[48] Insiders will also note that a four-generation chain of masters and apprentices appears here, a chain much like those observed in the physical and biological sciences.[49] Starting with Talcott Parsons, one of Robert Merton's teachers at Harvard, we can trace an intellectual lineage through Norman Kaplan, who studied with Merton at Columbia and went on to supervise Norman Storer's doctoral work at Cornell. Had we set our cutoff point in citations just one step lower, the 1970–72 list would also have included Nicholas Mullins, who did his doctoral work with Storer at Harvard. These master-apprentice chains reflect the twin processes of self-selection and selective recruitment among different generations of influentials. By providing structural supports for continuing social relations between members of the same intellectual family, they make for a degree of cohesion between academic generations.

The next steps in citation analysis of specialty formation are clear. First, analysis of citations in books must be undertaken. Second, these lists of most-cited authors contain names of the most influential scholars in the specialty, but the intellectual linkages between them is not explored. Quantitative and qualitative analysis of such linkages between papers and influentials is needed. This will enable us to learn whether some authors are influential for all their colleagues, whether schools actually exist or are in formation, how the cognitive texture of the specialty has changed, and the extent to which the boundaries of groups identified are permeable. This analysis is in process.[50]

ROBERT K. MERTON: TEACHER, FOUNDER AND INFLUENTIAL

So much for the intellectual concerns and social composition of leading authors in the sociology of science. We turn now to Merton's impact on the specialty.

Unlike his own teacher, George Sarton, Merton has had some success in recruiting students to the discipline. In his concern to establish the history of science as a respectable scholarly enterprise, Sarton made demands on students so severe as to be self defeating. Not many learned the classical and oriental languages whose mastery, along with five or six major modern languages, Sarton deemed necessary. And still fewer obtained the equivalent of advanced degrees in both the physical and the biological sciences he also considered necessary for historians of science. He also failed to develop a coherent formulation of principal problems in the field and a set of usable research techniques. Although Sarton developed a distinctive perspective on the history of science

it was not one that could be readily adopted by potential recruits. It is not surprising then that few historians of science count themselves among Sarton's students.[51] Although Merton shares some of Sartons' perfectionism in his demands on students (a characteristic not all of them find endearing), his work has brought many students into the specialty. It lays out a series of problems in the sociology of science and provides an orientation to sociological work in general. This becomes evident when we look closely at the uses made of Merton's work by different intellectual constituencies.

Although Merton continued his studies in the sociology of science and published more than twenty papers in this area in the two decades after he completed his dissertation, these efforts were not immediately recognized by sociologists, historians of science, or, for that matter, anyone else. This was so in spite of the considerable attention paid his theoretical work and his studies in the sociology of organizations, professions, and mass communications.[52] It now seems obvious that Merton's early papers (and, we think, those of any founder) had limited initial impact because they had no audience specifically attuned to publications on this subject. Merton's "Priorities in Scientific Discovery,"[53] his presidential address to the American Sociological Society, was warmly received. Sociologists clearly knew about this paper and may even have read it. Yet it had little immediate impact even in the literature of the sociology of science. Of all the citations the "Priorities" paper has received—which are more numerous than for any other paper in the literature—fewer than a third came in the eight years following its publication and only half in the first ten. This is most unusual considering the general pattern of intensive citation immediately following publication and a gradual tailing off afterward.[54] It also turns out that the average time elapsed—mean and median—between the publication of this paper and its use is ten years. Increasing citation of the "Priorities" paper in recent years no doubt reflects growth in the literature of the sociology of science. But over and above this artifactual element, the pattern of use observed here suggests how little influence ideas will have until a core of professional researchers with a common orientation are around to use them.

Although Merton's work overall has been increasingly influential in recent years, this is not uniformly true of all his papers. His recent publications, those appearing since 1959, seem to have had disproportionately great impact on the specialty. Comprising fewer than half of the papers he has published on science, they have received sixty-five percent of all citations to his work. With the exception of his studies of the normative structure of science, his early papers on the compatibility of Puritanism and the scientific ethos, on the sociology of knowledge, and on science and totalitarian politics are far less often used by sociologists of science than his later inquiries into competition for priority,[55] multiple discovery,[56] the Matthew Effect,[57] the evaluation-system in science,[58] and Insiders and Outsiders.[59] Thus the active interest taken in his later papers does not appear to have triggered renewed interest in his earlier ones.

The reasons for the differential influence of these earlier and later papers are more complex than it would first appear. As we noted, new publications

are more apt to be cited in growing literatures than older ones. In no small part, this is so because older publications are "dated" or their content has already been incorporated into the cognitive structure of the field. Neither of these explanations appears to fit here. Instead, sociologists of science found in Merton's later work on priorities, stratification in science and its reward-system greater "potential for elaboration"[60] and a reasonably clear program of research. It focused attention on the operation of the evaluation and reward-systems, their efficacy in extending scientific knowledge, and the role of recognition in scientists' motivations to continue their work. Close inspection of the papers by newcomers to the field who appear on the list of most cited authors in the 1960s and 1970s shows that much of their empirical work begins with a problem posed in one or another of Merton's later papers.

Moreover the "potential for elaboration" of Merton's recent work has been much enhanced by three developments beyond the confines of the sociology of science: establishment of the Science Citation Index, publication of the American Council of Education's appraisals of graduate education, and growing interest in empirical studies of social stratification.

Developments in quantitative analysis of social stratification fed concurrent studies of stratification in science and, it would appear, gave research in the esoteric specialty a more general appeal. At the same time, sociologists have made little progress in studying structures of norms and values or the relations between social institutions, the most important issues Merton addressed in his earlier work in the sociology of science. Their potentials for elaboration are largely untested by sociologists of science.[61]

Historians of science have had different concerns. Had our bibliographic search and citation index been extended to recent publications in the history of science, greater interest in Merton's studies of Puritanism and the rise of science would have been registered. This would be particularly so among younger historians of science working on the social contexts of scientific development.[62] The influence of Merton's work on seventeenth-century science was invisible for some time, as Henry Guerlac tells us, since it ran counter to the dominant trend toward "internalist" or "idealist" history of science.[63] Merton's influence on historians of science has surfaced in the last decade or so. Although we find only one other sociologist even mentioned in papers dealing with the history of science in *Past and Present* and in *History of Science*, references to the Merton thesis have multiplied considerably in recent years. And such established "internalist" historians of science as I. Bernard Cohen,[64] A. Rupert Hall,[65] Charles Gillispie,[66] and Guerlac appear to be increasingly sympathetic to considerations of the social and ideological contexts of science, Guerlac calling Merton's dissertation a "landmark" in the literature of the history of science,[67] and Gillispie remarking, "Not many a thesis furnishes fuel for a controversy lasting as long as its author's career, much less bidding (as this one is beginning to do) for immortality."[68]

There are however a few signs that Merton's sociological perspective was being incorporated into the history of science. Charles Gillispie reports that on first reading the "Priorities" paper it appeared "a bit trivial. I don't believe I also said 'unworthy' but recollect that such a dark thought was in my mind."

He goes on to say that he and his colleagues in the history of science did not really understand what it was all about: "Only a few years later, when I began to study and teach materials in the social and institutional as well as the more traditional internal and intellectual history of science, did I come to take the full thrust of what he [Merton] had in fact said, and said clearly and convincingly."[69] Nevertheless, of the entire corpus of Merton's writings in the sociology of science it is the Puritan roots of English science which still preoccupy most historians of science. Merton's recent work that sociologists of science have found so "puzzle-producing" has not yet found its way into the thinking of the historians. But, nonetheless, he is an intellectual presence for them. Commenting in a book review on an error in indexing, John Murdoch writes, "Robert Merton . . . has been classified under 'Merton College.' (This kind of transformation of Merton into an institution is presumably a bit premature!)"[70]

Returning to the impact of Merton's work on sociologists of science, we note that citation analysis can not take us very far toward understanding how authors are influenced by publications they cite even though it does tell us how often they do so in our limited sample of journals. Procedures for analyzing types of citations and their frequency are not well developed. In fact, sociologists of science have not even settled on a standardized classification of citations.[71] Our own efforts at a content analysis of citations to Merton's work are, as a consequence, rather tentative. Altogether fifty-eight different authors writing in the nine journals sampled have used thirty-four of Merton's papers. Although his later publications have been disproportionately cited in recent research in the sociology of science, the kinds of citations these papers receive are not much different from those given his earlier publications at comparable times in their own life histories. By and large, Merton's work is cited for two purposes: to confer authority on statements authors make and to identify the source of problems. There are few ceremonial or perfunctory citations, and a few citations which are disparaging. In the absence of statistical norms on the relative frequency of different kinds of citations in the sociological literature it is not possible to interpret the distribution observed here. Authors who draw on Merton's work as a point of departure for their own research typically do so by developing ideas he originated or crystallized rather than using it as a source for specific hypotheses along the same lines as Stephen Cole's study of the "Matthew Effect."[72] Clearly sociologists of science are now taking the first steps toward understanding the ways in which knowledge develops, and how research by different generations of scholars is linked together as these are reflected in citations.

Thus, citation analysis at its present stage of development can only take us so far toward understanding the character of intellectual influence. Serious content analysis of manuscripts is required to fill out the skeletal facts provided by citations. The same is true when we attempt an account of networks of influence. The gross connections between publications can of course be mapped. But citations do not even hint at the decisive if only partly visible effects scholars have on one another via informal discussions and critical readings of manuscripts and grant applications. Painstaking documentary research

and interviewing are needed to get at these informal linkages and their diverse consequences. Not only would studies of authors' acknowledgments be germane here, but also notes on seminars, research memoranda, letters, diaries, and commentaries on texts. Consider this fragmentary exchange in which Merton serves as facilitator and reference individual. In June, 1959, while at the Center for Advanced Study in the Behavioral Sciences, Thomas Kuhn wrote to Merton thanking him for his close reading of the paper Kuhn had sent him on measurement in science. He added:

> I am sending you . . . a much-revised draft of my first chapter on "Scientific Revolutions." If you have a chance to look at it, I shall be very grateful for your reactions. Meanwhile, or at least until you call me off, I shall continue to pester you with pieces in this vein as they become available.[73]

Merton was deeply impressed by the manuscript. When Kuhn was ready to send *The Structure of Scientific Revolutions* to the University of Chicago Press, he wrote to Merton asking that Merton intercede with the Press if it proved reluctant to publish the volume independently of the Encyclopedia of Unified Science. Merton replied:

> Of course, I'll be glad to write to the Chicago Press along the lines you suggest. After all, I've read the earlier draft and this alone is enough to justify a strong recommendation to the Press that they proceed as you would have them do.[74]

Kuhn's apprehensions proved groundless. The Press agreed to publish the book as Kuhn requested and it appeared at the end of 1962. Merton wrote to Kuhn:

> I have just this day received a copy of your new book . . . Having read this version in its entirety, I must say that it is merely brilliant. More than any other historian of science I know, you combine a penetrating sense of scientists at work, of patterns of historical development, and of sociological processes in that development.[75]

Kuhn replied:

> I think you know how much your good opinion of the sort of work I have tried to do in the book means to me. . . . Of course I'll inscribe your copy. . . . I always hate that particular task, but it will be a small price to pay for the chance to talk the whole area over with you.[76]

Bernard Barber, in his essay on L. J. Henderson, and Nicholas Mullins in his analysis of specialty formation, both conclude that the people who fill the role of the "trusted assessor" critically affect scientific development not only by setting standards but also by providing social and psychological support for those working in new areas.[77] The physicist and knowledgeable observer of science John Ziman goes even farther and asserts that "the creation of a group of reliable experts who can be trusted to give fair consideration to all new work . . . [is] one of the difficulties in the establishment of an entirely new science."[78]

And, finally, crude quantitative analysis of citations is not fine-grained enough to detect the subtle and often unacknowledged influences of general theoretical and methodological orientations. In the process of "obliteration by incorporation,"[79] which is especially marked in the case of general ideas, original sources are lost to view. We want now to consider how some of Merton's generic sociological concepts have found their way into the sociology of science and, in doing so, to illustrate the interplay between the cognitive structures of specialties and those of the larger disciplines.

Merton's analysis of science and scientists did not develop independently of his other theoretical efforts. On the contrary, the problems he has selected and the mode of attack he has used are clearly related to his general interest in applying structural and functional analysis to social patterns in various institutional spheres. Four theoretical themes will serve the purpose:

1. Anomie and deviant behavior
2. Multiple consequences, manifest and latent functions
3. Over-conformity and maladaptation
4. Self-reinforcing social processes.

Anomie and deviant behavior. Merton's analysis of competition for priority in science closely parallels his widely known work on deviant behavior. The origins of deviant behavior in science and in the larger culture are located in a disjunction between goals and normatively prescribed means. And its incidence depends in large part on the structure of opportunities to conform to the norms.[80] He suggests that the strong emphasis in science on the extension of certified knowledge and thus on original discovery has much the same effect as the comparably strong emphasis in American culture on financial success. Although scientists are enjoined to humility and to disinterestedness, they are, at the same time, driven to seek recognition of their originality, since that is the only way they can be sure they have truly made contributions to science. As a consequence, they are under great pressure to stake claims to what they take to be their scientific property, to assert their priority of discovery, and, however uncontentious they might be personally, to engage in priority disputes. Rarely however do these efforts shade over into thoroughly deviant acts including plagiary, data manipulation, and slander of competitors for priority. Merton's analysis of competition for priority is formally similar to his treatment of other kinds of patterned nonconformity in two important respects: he is unwilling to accept psychological accounts of what he takes to be socially structured behavior and he looks for explanations of its origins and frequency in particular in normative ambivalence (norms and counter norms), in cultural inconsistencies, and in socially structured opportunities. Storer,[81] Gaston,[82] and Hagstrom,[83] among others, have used this analysis in studies of the sources of competition in science, its incidence, and the forms it assumes in various social contexts.

Multiple consequences, manifest and latent functions. Merton's analysis of the Matthew Effect in science[84] draws upon the perspective laid out in his "Para-

digm for Functional Analysis."[85] He begins with the observation that scientific recognition tends to accrue to those who already have it. Without deliberate intent, the Matthew Effect[86] penalizes the young and the unknown and, in the process, reinforces the already unequal distribution of rewards. But this is not all. Merton characteristically takes a step back from the phenomenon he has been examining from the perspective of individuals and asks how things look for the system as a whole and finds multiple and diverse consequences. He argues that the misallocation of credit which results from the Matthew Effect is unjust and exacts a high emotional cost from individuals. But at the same time it has the surprising effect of making the communication system more efficient; it thus contributes to the extension of certified scientific knowledge. For one thing, the Matthew Effect calls attention to the work of proven scientists and thereby increases the probability that work of value will get noticed and read. It also calls attention to different parts of the output of distinguished scientists, increasing the visibility of reappearing themes in their work that might otherwise go unnoticed. Merton's technique of shifting the angle of theoretical vision from social consequences for individuals to those for systems and from manifest to latent outcomes has been fruitfully used by sociologists of science. Two diverse examples are Menzel's[87] analysis of the functions of scientific communication and our own studies on the intended and unintended effects of rewards on scientists' subsequent productivity.[88]

Overconformity and maladaptive behavior. The notion that the same social pattern is likely to have multiple consequences for different units of a system is closely allied to the view that the same social patterns have different consequences over time and in diverse circumstances. For more than thirty years, Merton has been concerned with the idea that conforming behavior that is adaptive in some circumstances readily becomes maladaptive when circumstances change. One aspect of the analysis of "Bureaucratic Structure and Personality" illustrates this idea. For their effective operation, bureaucracies require their members to be punctual and methodical. However, characteristics which have positive consequences under one set of conditions can have negative consequences under other conditions. When temporal and procedural flexibility are required, excessive concern with temporal and formal rules makes for bureaucratic obsessiveness, red tape, and the development of bureaucratic virtuosity. Merton continually reminds us of the troubles created by too much of a good thing. In science, he observes, an excess of commitment to the norm of disinterestedness—to science for its own sake—has social consequences that make for public alienation from science. Similarly, overconformity to the norms requiring recognition of all participants in scientific research has made for increasingly large author-sets and, in some fields, great difficulty in identifying those responsible for the research who should be credited or tarred for it.

Self-reinforcing social processes. Among the self-reinforcing processes Merton has examined,[89] the accumulation of advantage has special interest for sociologists of science. Introduced in his 1942 essay on normative structure, the

idea suggests that even evaluation which is impersonal and universalistic and perceived as fair can lead to the accumulation of differential advantage among certain segments of a population.[90] This idea has been taken up and elaborated by Crane,[91] the Coles, Zuckerman, and by Merton himself in the Matthew Effect. The process begins with an initial definition of some individuals as promising. These individuals are then given more resources and facilities for their work than those who are initially defined as less talented. Assuming that those who are thus advantaged are sufficiently competent to use the tools placed at their disposal effectively, it is hardly surprising that they often produce better research than others who are less advantaged. Once in operation, differences in performance between the "haves" and the "have nots" increase as the "haves" are, on universalistic grounds, consistently given more resources for their research and more recognition for it. This process and its components are difficult to study empirically because it is not evident how the performance effects of differential access to resources can be distinguished from effects attributable to differences in capacity. It is clear, however, that systems of evaluation operating in this fashion tend to be self-confirming. However effective they may in fact be in allocating resources to those who can best use them, their effectiveness cannot be judged by simple comparisons of performance between the advantaged and disadvantaged.

This brief review suggests how the sociology of science has drawn upon generic sociological ideas and how they have been adapted to its distinctive purposes. As special fields of inquiry develop, they typically elaborate ideas and techniques that are subject-specific. At the same time, the problems they take to be central as well as the concepts and procedures used to study these problems are often drawn from the larger discipline of which they are a part. How the relative proportions of these two aspects of cognitive structure, generic ideas and procedures, and those that are subject-specific, vary among types of specialties and over time is still another unanswered question in the cognitive and social evolution of science.

THE DEVELOPMENT OF AN ORGANIZATIONAL INFRASTRUCTURE

New specialties, radical and conforming, require more than a developing cognitive identity for institutionalization. We do not yet know the extent to which cognitive development in special fields and the development of their organizational infrastructures are interdependent or how this interdependence varies at different stages of specialty development. It is clear, however, that mundane problems such as training students, arranging for jobs, obtaining funds for research, and finding outlets for publication have to be dealt with in routinized ways for specialties to develop.

Specialties can grow without provision for regular training if there are new recruits sufficiently interested and willing to teach themselves. However, chances for recruitment are greatly improved if undergraduate and graduate

students have access to systematic course work in the special field. On this assumption, we surveyed the catalogues of the twenty-one departments of sociology receiving the highest rank in the 1969 American Council on Education study of the quality of graduate faculties[92] for courses being offered in the sociology of science in each of three academic years: 1960–61, 1964–65, and 1973–74. At the beginning of the sixties, Norman Kaplan, then at Cornell, offered the only course in the sociology of science in the United States. Four years later, six courses were offered by four departments. At the last reading, fourteen of the twenty-one ranking departments offered eighteen courses in the sociology of science.

Some implications of these data are obvious. Not only are there more students but there are also faculty members of leading departments willing to teach and presumably qualified by work in the field to do so. These signal that the specialty has acquired a certain legitimacy among sociologists—a legitimacy related to its compatibility with the fundamental theoretical and methodological commitments of most American sociologists. The same developments are also related to the historical fact that the take-off points in interest in the sociology of science coincided with a period of expansion and affluence in American science and in universities. The increased funds available in the 1960s led to substantial increases in the size of university faculties. Consequently, sociology departments were willing to hire people trained in new specialties such as the sociology of science. Growing interest among sociologists in science and increasing resources for it coincided. Both seem to be outcomes of the same underlying fact. Scientific development had become both a great national asset and a global problem.

Nonetheless, there still is little undergraduate teaching of the sociology of science. No general textbook has been published, Joseph Ben-David's *The Scientist's Role in Society*[93] being more monographic than didactic. In fact, few general introductory texts deal systematically with science.

RESEARCH SUPPORT

One of the distinctive features of American science is pluralism in funding. Consequently, information about who has gotten research money, how much, and for what purpose is scattered. The records of the National Science Foundation,[94] the principal source of support for work in sociology of science, indicate that a steady increase in funding paralleled growth in publications, dissertations, and recruitment. Less than one percent of NSF expenditures in sociology went to the sociology of science between 1957 and fiscal year 1964–65, 2.3 percent between 1965–66 and 1969–70, and 5.7 percent between 1970–71 and 1971–72. In this sixteen-year period, NSF grants to the sociology of science increased seven times compared to an increase of 1.75 times for sociology as a whole. Since sociological research is supported by multiple government agencies and by a variety of private foundations, NSF figures for the discipline underestimate total expenditures on sociological research. Nonetheless, whatever share of funds the sociology of science has received, it has been supported with

increasing generosity. Since the NSF employs the procedure of peer review in allocating its resources, the increase in funds registers growing confidence among sociologists (and government administrators) that research in the sociology of science is worth doing.

FORMAL COMMUNICATION AND FORMAL ORGANIZATION

There is other evidence that the specialty was becoming interesting to sociologists and gaining a measure of legitimacy among them. An increasing number of the papers in the sociology of science appearing in sociology journals were published in the two principal journals in the field, the *American Sociological Review* and the *American Journal of Sociology*. Before 1957, too few papers were published to permit meaningful comparisons, but afterward things changed markedly. The number of papers published in the *ASR* and the *AJS* more than doubled from the 10 appearing between 1957 and 1965 to the 22 appearing between 1965 and 1972. During the same period, the average number of articles published in these journals remained roughly constant. Since the prime journals have by far the greatest circulation among sociologists, the specialty was clearly gaining visibility. This is of no small importance to young people interested in working in the specialty who want their papers to be read by sociological colleagues. That one could publish in one of the important journals in the field (and, as we have seen, have a position in a leading department) means that doing the sociology of science could now be a career as well as a labor of love.

Unlike other new and growing specialties, the sociology of science has not yet acquired its own journal. The willingness of the principal sociological journals to publish papers in the sociology of science, the limited size of the specialty, and the presence of functional alternatives in the form of other journals addressed to the wider audience of historians, philosophers, political scientists, and sociologists of science—*Minerva* (founded in 1962) and *Science Studies* (founded in 1970)—probably mean that a specialty journal will not be needed for some time to come.

Still another sign of its growing legitimacy among sociologists is the appearance of sociologists of science on the programs of the various national, regional and international meetings. Since the early 1960s almost every national meeting of the American Sociological Association has had a session devoted to the specialty and since 1966, there has been a Research Committee in the Sociology of Science in the International Sociological Association. Robert Merton's influence and efforts to build an organizational infrastructure are highly visible in this domain. Merton encouraged the scheduling of sessions at ASA meetings in the early 1960s by agreeing to chair them or to prepare papers, and was one of the chief organizers of the ISA Committee. But he does not find these activities congenial. He does not like to organize things or to run them. Unlike his teacher, George Sarton, who avidly devoted himself to establishing an elaborate organizational infrastructure for the history of science, Merton has set about most of these tasks reluctantly and has been far less

effective than Sarton. But among physical and biological scientists his standing has helped him to call attention to the sociology of science in quarters such as the National Academy of Sciences and the National Science Foundation.

PROFESSIONAL IDENTITY

Patterns of consensus in citation practices noted so far are outcomes of unself-conscious behavior of sociologists of science. For the specialty to develop a full-fledged cognitive and social identity, however, specialists, particularly influential ones, must self-consciously define themselves as having a common task. We use the citation data once again to see if a growing proportion of influentials specifically identify themselves as sociologists of science. Limiting our survey to two overlapping groups of cited authors, the thirty most often cited and the top ten percent of cited authors in successive five-year periods, we tabulated their self-described areas of specialization as they appeared in standard directories of the ASA and *American Men (and Women) of Science*.[95] Table 4 shows the sharp increase in the proportion of the thirty most influential authors who consider themselves sociologists of science. It rose from 10 percent in the early 1960s to 36 percent in the late sixties and to 40 percent in the seventies. While there are also significant increases in the extent of identification with the specialty among the top decile of influentials, the figures suggest that the major shift has occurred among the thirty most-cited authors. Such changes in self-definitions among influentials register a growing consolidation of the specialty and new commitment to it. This heightens its visibility and helps accord it legitimacy.

Table 4. SELF-DEFINITIONS OF INFLUENTIAL CONTRIBUTORS TO THE SOCIOLOGY OF SCIENCE 1950–1972

Percentage Who Define Themselves as Sociologists of Science

	Top Thirty Authors			Top 10 Percent of All Cited Authors*		
Period	1st choice	1st and 2nd choice	(N)	1st choice	1st and 2nd choice	(N)
1950–54	7**	7**	(15)**	7	7	(15)
1955–59	7	10	(30)	7	9	(32)
1960–64	10	17	(30)	7	11	(57)
1965–69	36	40	(30)	16	18	(78)
1970–72	40	47	(30)	18	22	(90)

*The specialties of two authors on the 1955–59 list could not be identified along with one on the 1960–64 list, four on the 1965–69 list, and five on the 1970–72 list. For totals, see Table 1A.
**Only fifteen authors had more than a single citation in this period.

More than twenty years ago, in his Foreword to Bernard Barber's *Science and the Social Order*, Merton asked why "the sociology of science is still a

largely unfulfilled promise rather than a highly developed special field of knowledge, cultivated jointly by social, physical and biological scientists?"[96] His inventory of neglect was dismaying indeed. Few courses were offered in the sociology of science and no standard textbook took notice of it; little empirical research was in process and what was being done was divorced from theory; publications were speculative, relying more on historical examples than on systematic historical evidence. Merton's own content analysis of Barber's bibliography of the field showed that half of all the referenced works were by "practicing physical and life scientists or by scientists who have turned to administration; more than a quarter by historians and philosophers of science and only the remaining fraction by sociologists."[97] In short, the field showed no signs then of impending institutionalization.

Things have changed. Taken together, the evidence we have examined suggests that the sociology of science is emerging as a special field of interest with a distinctive cognitive and professional identity. Having passed through an initial takeoff point in the middle 1960s, the field is still growing. It is too soon yet to tell how damaging hard times will be to further consolidation of the field. But it is not too soon to report that the sociological analysis of science is no longer the province of amateurs.

The next steps in studying the development of research specialties are reasonably clear. Continuing work will have to take account of the larger context of the growth of scientific knowledge and the relations between cognitive and social structures of science. Merton himself has now shifted his attention to developing an historical sociology of scientific knowledge. The growing emphasis among sociologists of science on the interplay of substantive and social aspects of science signals the beginnings of a new phase in the specialty.

NOTES

1. See Cornelis B. Van Niel, "The Microbe as a Whole," in S. A. Waksman, ed., *Perspectives in Microbiology* (New Brunswick, N.J.: Rutgers University Press, 1955), pp. 3–12; Gerald Holton, "Models for Understanding the Growth and Excellence of Scientific Research," in S. R. Graubard and G. Holton, eds., *Excellence and Leadership in a Democracy* (New York: Columbia University Press, 1962), pp. 94–131; D. R. Stoddart, "Growth and Structure of Geography," transactions and papers, Institute of British Geographers, 1967, no. 41, pp. 1–19; and J. S. Hey, *The Evolution of Radio Astronomy* (London: Paul Elek, 1973).
2. Robert K. Merton, "Priorities in Scientific Discovery," (1957) reprinted in *The Sociology of Science: Theoretical and Empirical Investigations*, ed. by Norman Storer (Chicago: University of Chicago Press, 1973), pp. 286–324. First published in 1957.
3. Arnold Thackray and Robert K. Merton, "On Discipline Building: The Paradoxes of George Sarton," *ISIS* 63 (1972): 473–95.
4. This is not to say, of course, that Merton was the first sociologist to study science and invention. Durkheim, Marx, Mannheim, Scheler, Znaniecki, Sorokin and others had addressed questions about the social determination of scientific knowledge long

before him as did W. F. Ogburn and Dorothy Thomas in their studies of the role of cultural accumulation in scientific and technical innovation. See their "Are Inventions Inevitable? A Note on Social Evolution," *Political Science Quarterly* 37 (1922): 83–98. Rather Merton took the lead in the systematic study of science as a social institution and provided models for how this might be extended. Sociologists of science generally agree that Merton established the field as an intellectual and social activity. For examples see Michael Mulkay, "Some Aspects of Cultural Growth in the Natural Sciences," *Social Research* 36 (1969): 22–53; Kenneth Downey, "Sociology and the Modern Scientific Revolution," *Sociological Quarterly* 8 (1967): 239–54; and Barry Barnes, ed., *Sociology of Science: Selected Readings* (Harmondsworth, Eng.: Penguin Books, 1972).

5. Joseph Ben-David, "Scientific Productivity and Academic Organization in Nineteenth-Century Medicine," *American Sociological Review* 25 (1960): 828–43, and Joseph Ben-David and Awraham Zloczower, "Universities and Academic Systems in Modern Societies," *European Journal of Sociology* 3 (1962): 45–84; and Joseph Ben-David, *The Scientist's Role in Society: A Comparative Study* (Englewood Cliffs, N.J.: Prentice-Hall, 1971).
6. For examples of Merton's early interest in cognitive as well as social aspects of scientific knowledge, see his collaborative papers with Sorokin in Pitirim A. Sorokin, *Social and Cultural Dynamics*, 4 vols. (New York: American Book Co., 1937) II, pp. 125–80, 439–76; Chapters 7–11 of his *Science, Technology and Society in Seventeenth-Century England* (Bruges, Belgium: St. Catherine Press, 1938); reprinted with new Introduction (New York: Howard Fertig and Harper & Row, 1970). For recent examples of empirical and theoretical sociological studies of cognitive aspects of science, see Stephen Cole's paper in this volume (pp. 175–200) and John Law, "The Development of Specialties in Science: The Case of X-ray Protein Crystallography," *Science Studies* 3 (July 1973): 275–303; Michael J. Mulkay and David O. Edge, "Cognitive, Technical and Social Factors in the Growth of Radio Astronomy," *Social Science Information* 13 (1974): 25–61.
7. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962; new, enlarged edition, 1972).
8. Derek J. deS. Price, *Science Since Babylon* (New Haven: Yale University Press, 1961); *Little Science, Big Science* (New York: Columbia University Press, 1963); "Networks of Scientific Papers," *Science* 149 (1965): 510–15; and "Citation Measures of Hard Science and Soft Science, Technology and Non-Science," in Carnot E. Nelson and Donald K. Pollak, eds., *Communication Among Scientists and Engineers* (Lexington, Mass.: Heath, 1970); and Derek J. deS. Price and Donald Beaver, "Collaboration in an Invisible College," *American Psychologist* 21 (November 1966): 1011–18.
9. See for example, Karl R. Popper, *Logic of Scientific Discovery* (New York: Basic Books, 1959), a translation of *Logik der Forschung* (1935); *Conjectures and Refutations* (London: Routledge and Kegan Paul, 1963); *Objective Knowledge* (Oxford: Clarendon Press, 1972). Much of Imre Lakatos' seminal work is now being prepared for posthumous publication. His influential publications include "History of Science and Its Rational Reconstructs," in R. C. Buck and R. S. Cohen, eds., *Boston Studies in the Philosophy of Science* 8 (1971): 91–136, 174–82; "Falsification and the Methodology of Scientific Research Programmes," in I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), pp. 91–195; "Popper on Demarcation and Induction" in the two-volume collection of critical essays on the Popperian tradition, P. A. Schilpp, ed., *The Philosophy of Karl Popper* (LaSalle, Ill.: Open Court,

1974), I, pp. 241–73; and the paper which he earmarked as best epitomizing his concept of "research programme," Imre Lakatos and Elie Zahar, "Why Did Copernicus's Programme Supersede Ptolemy's?" Presented at the Quincentenary Symposium on Copernicus of the British Society for the History of Science. London: January 5, 1973. Mimeod.

10. See in addition to Lakatos and Musgrave, *op. cit.*, Israel Scheffler, *Science and Subjectivity* (Minneapolis: Bobbs-Merrill, 1967) and Dudley Shapere, "The Structure of Scientific Revolutions," *Philosophical Review* 73 (1964): 383–94.
11. See Nicholas C. Mullins, "The Development of a Scientific Specialty," *Minerva* 10 (January 1972): 51–82, and "The Development of Specialties in Social Science: The Case of Ethnomethodology," *Science Studies* 3 (1973): 245–73; and Belver C. Griffith and Nicholas C. Mullins, "Coherent Social Groups in Scientific Change," *Science* 177 (September 15, 1972): 959–64.
12. Price, *Little Science, Big Science*, Chapter 3, "Invisible Colleges . . ."
13. Among those who have discussed the growth and change of scientific specialties are: Warren Hagstrom, *The Scientific Community* (New York: Basic Books, 1965); Joseph Ben-David and Randall Collins, "Social Factors in the Origins of a New Science: The Case of Psychology," *American Sociological Review* 31 (August 1966): 451–65; Charles S. Fisher, "The Death of a Mathematical Theory: A Study in the Sociology of Knowledge," *Archive for the History of Exact Sciences* 3 (1966): 137–59; Terry N. Clark, "Émile Durkheim and the Institutionalization of Sociology in the French University System," *European Journal of Sociology* 9 (1968): 37–91; Diana Crane, "Social Structure in a Group of Scientists: A Test of the 'Invisible College' Hypothesis," *American Sociological Review* 34 (1969): 335–52; Diana Crane, *Invisible Colleges* (Chicago: University of Chicago Press, 1972); Nicholas C. Mullins, "The Development of a Scientific Specialty," "The Development of Specialties in Social Science," and *Theories and Theory Groups in Contemporary American Sociology* (New York: Harper & Row, 1973); Griffith and Mullins, "Coherent Social Groups in Scientific Change"; M. J. Mulkay and D. O. Edge, "Cognitive, Technical and Social Factors"; John Law, "The Development of Specialties in Science"; Thackray and Merton, "On Discipline Building"; Griffith and Mullins, "Coherent Social Groups in Scientific Change," p. 960; Michael J. Apter, "Cybernetics: A Case Study of a Scientific Subject-Complex," in P. Halmos, ed., *The Sociological Review Monograph*, No. 18 (September 1972): 93–115.
14. Mullins, whose work *Theories and Theory Groups in Contemporary American Sociology, op. cit.*, is the most ambitious to date, proposes a four-stage model of specialty development. It emphasizes the structure of communication between specialty group members rather than the content of scientific innovations. The principal components are the roles of intellectual and social leaders, the role of programatic statements, the diffusion of group members from centers of activity to the periphery, and the thickening of communication nets with growth. The model involves problems of defining boundaries of groups and their orientations and requires revision to apply to cognitively dissident specialties. Further investigation is needed to specify the structural and intellectual conditions of movement from the "normal" to the "network" to the "cluster" and finally to the "specialty" stages of development. See also Paul D. Allison, "Social Aspects of Scientific Innovation: The Cases of Parapsychology," (Master's Thesis, University of Wisconsin, 1973).
15. For two pertinent case studies, see Aaron J. Ihde, "An Inquiry into the Origins of Hybrid Sciences: Astrophysics and Biochemistry," *Journal of Chemical Education*

46 (April 1969): 193–96, and Robert E. Kohler, "The Enzyme Theory and the Origin of Biochemistry," *ISIS* 64 (1973): 181–96.

16. Harriet Zuckerman and Robert K. Merton, "Age, Aging and Age Structure in Science," reprinted in Merton *The Sociology of Science*, pp. 497–559.
17. Charles S. Fisher, "The Death of a Mathematical Theory: A Study in the Sociology of Knowledge."
18. Matilda W. Riley, "Membership of the American Sociological Association, 1950–1959," *American Sociological Review* 25 (1960): 914–26.
19. Nico Stehr and Lyle E. Larson, "The Rise and Decline of Areas of Specialization," *American Sociologist* 7 (August 1972): 5.
20. *Ibid.*, p. 6.
21. Five colleagues in the sociology of science, Warren Hagstrom, Walter Hirsch, Norman Kaplan, Janice Lodahl, and Norman Storer, reviewed entries in the inventory, suggested additional titles, and identified sponsors of dissertations.
22. See Theodore F. Abel, "Analysis of Attempts to Establish Sociology as an Independent Science," doctoral dissertation, Columbia University, 1929; and Wilfred Binnewies, "A History and Evaluation of the Quantitative Trend in Sociological Analysis," doctoral dissertation, University of Nebraska, 1929.
23. Tabulated from *Doctoral Dissertations Accepted by American Universities* (Washington, D.C.: U.S. Office of Education), and *Earned Degrees Conferred* and *Doctoral Records File* (Washington, D.C.: National Research Council, National Academy of Sciences).
24. Calculated from Lindsey R. Harmon and Herbert Soldz, comps., *Doctorate Production in United States Universities, 1920–1962* (Washington, D.C.: National Research Council, National Academy of Sciences, 1963), no. 1142, Appendix 3, pp. 74–85.
25. Price, *Little Science, Big Science;* Crane, *Invisible Colleges;* Henry Menard, *Science, Growth and Change* (Cambridge, Mass.: Harvard University Press, 1971); and David L. Krantz, "Research Activity in 'Normal' and 'Anomalous' Areas," *Journal of the History of the Behavioral Sciences* 1 (January 1965): 39–41.
26. Holton has proposed several models for interpreting comparable data. See Chapter 12 in Gerald Holton, *Thematic Origins of Scientific Thought: Kepler to Einstein* (Cambridge, Mass.: Harvard University Press, 1973).
27. Six of these nine journals are wholly sociological: *American Sociological Review, American Journal of Sociology, Social Forces, Social Problems, Sociology of Education,* and the *British Journal of Sociology*. The others include *Minerva,* first published in 1962, which focuses on various aspects of the history, politics, philosophy, and sociology of science, the *American Behavioral Scientist,* which has devoted issues to problems in the sociology of science, and *Science,* the official publication of the American Association for the Advancement of Science, which has included papers on the sociology of science. A preliminary check of the very small number of papers published in other books or journals shows that their age and references do not differ systematically from those we have analyzed. Nevertheless, this sample underestimates growth in the literature since 1971, since it does not include newer journals such as *Science Studies* and *Research Policy*.

    There is *prima facie* evidence that few papers were published before 1950. Bibliographies of work in the field compiled by sociologists show few entries before that date. Bernard Barber and Robert K. Merton, "Brief Bibliography for the Sociology of Science," *Proceedings,* American Academy of Arts and Sciences 80 (1952): 140–54; Norman Kaplan, "Science and Society: An Introduction," in Norman Kaplan, ed., *Science and Society* (Chicago: Rand-McNally and Co., 1965),

pp. 1–8; and Bernard Barber and Walter Hirsch, eds., *The Sociology of Science* (New York: Free Press, 1962).

28. Of the total 195 papers, 165 were published in journals in print for the full twenty-four-year period under examination. Analysis of trends in publication is confined to these 165 papers. All papers were included that considered scientists and the institution of science from the sociological perspective. Papers on science were excluded if they were wholly historical or philosophical or if they focused on questions of science policy. Two lists of candidate papers were compiled independently and discrepancies between them resolved by two judges.

    The citation index was constructed by compiling information on the citing authors, place and year of publication, and authors and dates of publications cited. Since citations and pairs of citations are the unit of analysis, multiauthored papers produced a multiplication of citations. When a publication is cited by collaborating authors, every author is counted as having been influenced by the cited publication. This gives extra weight to papers cited in empirical studies, since they are more often multiauthored. Self-citations were excluded. An additional procedure was used when Robert Merton was cited. In addition to registering his name and the year of the publication, its title and page number were recorded. This permitted a detailed content analysis of citations to Merton's work and enabled us to identify precisely which of his ideas had been influential.
29. The two journals established in the 1960s were excluded to avoid the biasing effects of increased publication outlets.
30. The slope of a scatterplot of publications over time approximates an exponential curve. We have not included the figure in the text, since it looks similar to other exponential growth curves. We suggest that in the future authors consider presenting temporal data on productivity in the form of a log-normal transformation. For illustrations of growth curves in science, see D. J. deS. Price, *Little Science, Big Science;* Diana Crane, *Invisible Colleges.*
31. Reprinted in Merton, *The Sociology of Science,* pp. 286–324. For observations on the significance of this paper, see Norman W. Storer, "Prefatory Note: The Reward-System of Science," *Ibid.,* pp. 281–85; and Joseph Ben-David, "The Sociology of Science," *New York Times Book Review,* November 11, 1973, p. 32.

    The "Priorities" paper does not constitute a formal paradigm in the Mertonian or Kuhnian sense or a research program in the Lakatos sense. It does propose a general orientation for the sociological study of science and directs attention to certain central problems.
32. On the use of citations to measure scientific influence and significance, see M. M. Kessler, "The M.I.T. Technical Information Project," *Physics Today* 18 (March 1965): 28–36; Eugene Garfield, "Citation Indexing for Studying Science," *Nature* 227 (1970): 669–71; and "Citation Indexing Historico-Bibliography and the Sociology of Science," E. Davis and W. D. Sweeny, eds., *Proceedings of the Third International Congress of Medical Librarianship* (Amsterdam: Exerpta Medica, 1970), pp. 187–204; Jonathan R. Cole and Stephen Cole, "Measuring the Quality of Sociological Research: Problems in the Use of the Science Citation Index," *American Sociologist* 6 (February 1971): 23–29, and *Social Stratification in Science* (Chicago: University of Chicago Press, 1973).
33. For other examples, see S. Cole, "Scientific Reward Systems."
34. Cole and Cole, *Social Stratification in Science.*
35. For a recent collection of papers adopting this point of view, see Richard D. Whitley, ed., *Social Processes of Scientific Development* (London: Routledge & Kegan Paul, 1974), pp. 69–95; Mulkay, "Some Aspects of Cultural Growth in the

Natural Sciences"; S. B. Barnes and R. G. A. Dolby, "The Scientific Ethos: A Deviant Viewpoint," *European Journal of Sociology* 11, no. 1 (1970): 3–25; and Barnes, ed., *Sociology of Science.*

Since journal articles are subjected to refereeing, they may be less polemical than books. Content analysis of citations drawn exclusively from journals may underestimate "critical" and overestimate adulatory comments. In order to determine whether this is so, we examined citations to Merton and other authors in the indexes of several books recently published by Merton's critics. Using B. Barnes, ed., *Sociology of Science*, Leslie Sklair, *Organized Knowledge* (London: Hart-Davis, MacGibbon, 1973), and R. D. Whitley, *Social Processes of Scientific Development* as a crude sample, we found evidence consistent with our hypothesis. We also found fewer references to empirical literature.

3. Price, "Citation Measures of Hard Science and Soft Science, Technology and Non-Science"; see also Price, "Networks of Scientific Papers," and J. Margolis, "Citation Indexing and the Evaluation of Scientific Papers," *Science* 185 (March 1967): 1213–19.
7. See Zuckerman and Merton, "Age, Aging and Age Structure in Science," pp. 506 ff.
8. Stephen Cole, Jonathan R. Cole, and Lorraine Dietrich, "Measuring Consensus in Scientific Research Areas," in Y. Elkana, J. Lederberg, R. K. Merton, A. Thackray, and H. Zuckerman, eds., *Toward a Metric of Science: Thoughts Occasioned by the Advent of 'Science Indicators'* (New York: Wiley-Interscience, in press). For comparable data, see also Price, "Citation Measures of Hard and Soft Science."
9. Both reprinted in Merton, *The Sociology of Science.*
10. Zuckerman and Merton, "Age, Aging and Age Structure in Science," pp. 510–19.
11. Average ages for the extended periods 1950–64 and 1965–72 were calculated from the ages of the thirty most-cited authors at the midpoint of each of five periods.
12. For changing rates of collaboration in sociology, see Narsi Patel, "Quantitative and Collaborative Trends in American Sociological Research," *The American Sociologist* 7 (1972): 5–6.
13. Merton is, of course, heavily cited in the literature of general sociology and in its neighboring disciplines of anthropology and psychology. Recent studies of the impact of leading sociologists' work show that he is the most cited author in the current literature and is cited more often than all authors but Durkheim in current textbooks. Mark Oromaner, "The Most Cited Sociologists," *The American Sociologist* 3 (May 1968): 124–26; "The Structure of Influence in Contemporary Academic Sociology," *The American Sociologist* 7 (May 1970): 11–13; "Comparison of Influentials in Contemporary American and British Sociology," *British Journal of Sociology* 13 (1970): 324–32; Frank R. Westie, "Academic Expectations for Professional Immortality: A Study of Legitimation," *The American Sociologist* 8 (February 1973): 19–32. See also Howard M. Bahr, T. J. Johnson, and M. R. Seitz, "Influential Scholars and Works in the Sociology of Race and Minority Relations, 1944–68," *The American Sociologist* 8 (November 1971): 296–98; and William H. Swatos, Jr., and Priscilla L. Swatos, "Name Citations in Introductory Sociology Texts," *The American Sociologist* 9 (November 1974): 225–28.
14. Citation counts for individuals are omitted because the addition of journals other than those surveyed here would have changed the absolute numbers, if not the approximate relative positions. Moreover, we are less concerned with individual scores than with changing patterns of citation over time.
15. We note that just two authors (Barber and Merton) in the recent lists have also worked in the sociology of knowledge. To some extent, the sociology of science and of knowledge have developed independently. By way of illustration, a compendium of recent work in the sociology of knowledge includes just one paper in the sociology of science and one in the sociology of sociology. The remaining twenty-five papers, classified as "Later Perspectives," contain no reference to any work in the sociology of science except the several in Merton's "Paradigm for the Sociology of Knowledge." See James E. Curtis and John W. Petras, eds., *The Sociology of Knowledge: A Reader* (New York: Praeger, 1970).
16. Kuhn's work on revolutions in science, Price's studies of patterns of scientific growth, Barber's comprehensive analysis of the institution of science, and Pelz's investigation of organizational climates of research have continued to be widely used since their publication.
17. Mullins, *Theories and Theory Groups in Contemporary American Sociology.*
18. The seven are: Bernard Barber, Stephen Cole, Barney Glaser, Alvin Gouldner, Norman Kaplan, Jonathan Cole, and Harriet Zuckerman. An eighth, Diana Crane, studied with Merton but did not do her doctoral work under his supervision.
19. For master-apprentice links among psychologists, see Joseph Ben-David and Randall Collins, "Social Factors in the Origins of a New Science: The Case of Psychology," *American Sociological Review* 31 (1966): 451–65; and among Nobel laureates, see Harriet Zuckerman, "Nobel Laureates in Science: Patterns of Productivity, Collaboration and Authorship," *American Sociological Review* 32 (1967): 391–403, and *Scientific Elites: Nobel Laureates in the United States* (Chicago: University of Chicago Press, 1975).
20. The Institute for Scientific Information, the organizational home of the Science Citation Index, is now undertaking different but related cluster analyses in order to identify groups of linked and frequently cited papers believed to presage the development of specialties. See Eugene Garfield, Morton V. Malin, and Henry Small, "Citation Data as Indicators of Scientific Activity" in Elkana *et al., Toward a Metric of Science.*
21. Thackray and Merton, "On Discipline Building."
22. Oromaner, "The Structure of Influence in Contemporary Academic Sociology."
23. Merton, "Priorities in Scientific Discovery."
24. See P. E. Burton and R. W. Keebler, " 'Half-life' of Some Scientific and Technical Literature," *American Documentation* 11 (1960): 18–22.
25. Merton, "Priorities in Scientific Discovery."
26. Robert K. Merton, "Singletons and Multiples in Scientific Discovery," in *Proceedings,* American Philosophical Society 105 (October 1961): 470–86; also in *The Sociology of Science,* pp. 343–70.
27. Merton, "The Matthew Effect," reprinted in *The Sociology of Science,* pp. 439–59.
28. Harriet Zuckerman and Robert K. Merton, "Patterns of Evaluation in Science: Institutionalization, Structure and Function of the Referee System," *Minerva* 9 (January 1971): 66–100; also in Merton, *The Sociology of Science,* pp. 460–96.
29. Robert K. Merton, "Insiders and Outsiders, A Chapter in the Sociology of Knowledge," *American Journal of Sociology* 78 (July 1972): 9–47. Reprinted in *The Sociology of Science,* pp. 99–136.
30. Allison, "Social Aspects of Scientific Innovation: The Case of Parapsychology."
31. Robert K. Merton, "The Normative Structure of Science," (1942) reprinted in *The Sociology of Science,* pp. 267–78. Ben-David has argued that Merton's analysis of the normative structure of science "gave a static and idealized picture of science as a social system and did not reveal how the system actually worked" ("The Sociology of Science," *New York Times Book Review,* p. 32). We would add that the later discussion of ambivalence in the norms ("The Ambivalence of Scientists,"

[1963] reprinted in *The Sociology of Science*, pp. 383–412) is not only sociologically instructive but also the point of departure for intriguing empirical studies of ambivalence by Ian Mitroff ("Norms and Counter-Norms in a Select Group of Apollo Moon Scientists: A Case Study of the Ambivalence of Scientists," *American Sociological Review* 38 [June 1974]: 579–95; and *The Subjective Side of Science: A Philosophical Inquiry into the Psychology of the Apollo Moon Scientists* [Amsterdam and San Francisco: Elsevier and Jossey-Bass, 1974]). The time has surely come for thorough empirical investigation of the distribution of norms among members of the scientific community and their conformity to them (see Marlan Blissett, *Politics in Science* [Boston: Little, Brown, 1972], for one effort) and for an end to speculation about whether scientists actually are conforming or deviant.

62. Until recently, the "Merton Thesis" has had greater influence on historians of science than any other part of his work. But the fact that Thomas Kuhn devotes practically one-third of his review of the "History of Science" in the *International Encyclopedia of the Social Sciences* (1968, 14: 74–83) to that thesis may say as much about what Kuhn finds interesting as about actual foci of attention in the field itself. Not surprisingly, Marxist historians of science find Merton's sociological analysis of science more congenial than do their colleagues doing traditional internalist history of science. The Marxists argue, however, that Merton does not confront the central issue of scientific knowledge as being "objective or value neutral." See Robert Young, "The Historiographic and Ideological Contexts of the Nineteenth-Century Debate on Man's Place in Nature," in M. Teich and R. Young, eds., *Changing Perspectives in the History of Science: Essays in Honour of Joseph Needham* (London: Heinemann, 1973), pp. 69–95.
63. Henry Guerlac, "History of Science: The Landmarks of the Literature," *The Times Literary Supplement*, April 26, 1974, p. 450.
64. I. Bernard Cohen, "Science, Technology and Society in Seventeenth-Century England," review, *Scientific American* 228 (1974): 117–20.
65. A. Rupert Hall, "History of Science: Microscopic Analyses and the General Picture," *The Times Literary Supplement*, April 26, 1974, pp. 437–38.
66. Charles C. Gillispie, "Mertonian Theses," *Science* 184 (May 10, 1974): 656–60.
67. Guerlac, "History of Science," p. 450. In the last fifteen years, a parallel concern with the history and the sociology of science has developed among some philosophers of science, especially Lakatos and Feyerabend. See R. N. Griere, "History and Philosophy of Science: Intimate Relationship or Marriage of Convenience," *British Journal for the Philosophy of Science* 24 (September 1973): 282–97.
68. Gillispie, "Mertonian Theses," p. 658.
69. *Ibid.*, p. 656.
70. John Murdoch, "Review of M. Wittrow, *ISIS Cumulative Bibliography*," *The British Journal for the Philosophy of Science* 25 (March 1974): 89–91.
71. Norman Kaplan, "The Norms of Citation Behavior: Prolegomenona to the Footnote," *American Documentation* 16 (July 1962): 179–84.
72. Stephen Cole, "Professional Standing and the Reception of Scientific Discoveries," *American Journal of Sociology* 76 (September 1970): 286–306.
73. Professor Kuhn has kindly given us permission to quote from his letters. Letter, June 1959.
74. Professor Merton has kindly given us access to his files of manuscripts and correspondence dealing with the sociology of science. Letter, 4 May 1961.
75. Letter, 13 December 1962.
76. Letter, 21 January 1963.
77. "Introduction" in Bernard Barber, ed., *L. J. Henderson, On the Social System*

(Chicago: University of Chicago Press, 1970), pp. 42–43; Mullins, *Theory and Theory Groups in Contemporary American Sociology*.

78. John Ziman, "Science Is Social," *The Listener* (August 18, 1960), p. 251.
79. On the process of obliteration by incorporation, see Robert K. Merton, *On Theoretical Sociology* (New York: Free Press, 1967), pp. 27–35.
80. Merton, "Social Structure and Anomie," in *STSS*, pp. 185–214.
81. Norman Storer, *The Social System of Science* (New York: Holt, Rinehart & Winston, 1966).
82. Jerry C. Gaston, *Originality and Competition in Science* (Chicago: University of Chicago Press, 1973).
83. Warren Hagstrom, *The Scientific Community* (New York: Basic Books, 1965), and "Competition in Science," *American Sociological Review* 39 (February 1974): 1–18.
84. Merton, "The Matthew Effect," in *The Sociology of Science*.
85. Robert K. Merton, "Manifest and Latent Functions," (1949) in *STSS*, pp. 73–138.
86. In the words of St. Matthew, "For unto everyone that hath shall be given, and he shall have abundance: but from him that hath not shall be taken away even that which he hath."
87. Herbert Menzel, "Scientific Communication: Five Themes from Social Research," *American Psychologist* 21 (1966): 999–1004.
88. Cole and Cole, *Social Stratification in Science*, pp. 113–15; and Zuckerman, "Nobel Laureates in Science," pp. 398–403.
89. Briefly introduced in Merton's 1942 paper on the ethos of science, the notion of "accumulation of advantage" and of disadvantage in systems of social stratification, which relates to the notions of the self-fulfilling prophecy and the Matthew Effect, has been developed in a series of investigations: Merton, *The Sociology of Science*, pp. 273, 416, 439–59, 497–559; Harriet Zuckerman, "Stratification in American Science," *Sociological Inquiry* 40 (Spring 1970): 235–57; Zuckerman and Merton, "Age, Aging and Age Structure in Science"; Cole and Cole, *Social Stratification in Science*, pp. 237–47; Paul D. Allison and John A. Stewart, "Productivity Differences Among Scientists: Evidence for Accumulative Advantage," *American Sociological Review* 39 (August 1974): 596–606; Zuckerman, *Scientific Elites*, Chapter 3.
90. Merton, "The Normative Structure of Science," in *The Sociology of Science*.
91. Diana Crane, "The Academic Marketplace Revisited: A Study of Faculty Mobility Using Cartter Ratings," *American Journal of Sociology* 75 (May 1970): 953–64.
92. K. D. Roose and Charles J. Andersen, *A Rating of Graduate Programs* (Washington, D.C.: American Council of Education, 1970). Although the ranks of some departments change slightly, the intercorrelations between readings taken in 1957, 1964, and 1969 are above 0.95. The twenty-one departments receiving the highest ratings granted 77 percent of all doctorates in sociology in 1968–69.
93. Ben-David, *The Scientist's Role in Society*.
94. These data were drawn from National Science Foundation, *Annual Reports*, fiscal years 1950–51 to 1971–72, and from its *Grants and Awards* series, first published in fiscal year 1963–64. Information for 1972–73 was generously provided by Dr. Donald Ploch of the National Science Foundation.
95. The self-designations of sociologists were tabulated but not those of information scientists, historians, or philosophers of science. These data refer only to the increasing tendency for sociologists to identify themselves as sociologists of science.
96. Robert K. Merton, "Foreword," in Bernard Barber, *Science and the Social Order* (Glencoe, Ill.: Free Press, 1952); also in *The Sociology of Science*, pp. 210–20.
97. *Ibid.*, p. 212.