

Robert K. Merton

*Sociology of Science
and Sociology as Science*

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Introduction

On Merton's Legacy and Contemporary Sociology

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Robert K. Merton was among the most influential sociologists of the twentieth century. His influence stemmed from intellectual innovation and institutional leadership. It was enhanced by his pellucid prose style, which made his work easy for teachers and research-oriented sociologists to grasp and put to use. It was extended by a combination of modeling and mentoring that inspired and shaped a generation of students who themselves included a range of remarkable leaders.

Context also mattered. As much as anyone, Merton shaped the institutions and style of American sociology during the era of its remarkable postwar expansion. He sought to make the discipline academically respectable but also to make it matter. One of sociology's most remarkable and polymathic intellectuals, he sought to establish the discipline not on the basis of individual genius but on high standards for consistent productivity in the sociological craft.

Along with Talcott Parsons, the noted Harvard sociologist, Merton introduced a new level of explicit theoretical rigor into American sociology. Parsons positioned himself as the importer and synthesizer of crucial European work, and then as the auteur of his own theoretical system

ready to stand alongside Weber and Durkheim in a sociological pantheon. Merton, by contrast, made himself the empirical researcher's theorist. He channeled American as well as European forebears into conceptual frameworks, paradigms, and middle-range theories that focused empirical research on explanatory problems beyond the immediate data. At the same time, he probed empirical research for theoretically useful ideas initially left implicit.

Unlike Parsons, Merton was himself an empirical researcher of note. Seeking to understand the influence of religion, economy, and other factors on early modern science, for his dissertation research he coded the biographies of six thousand entries in the *Dictionary of National Biography* by hand (Merton 1938a). Seeking ways to explore opinion formation and response to media, Merton invented the focus group, or as he initially called it, focused group interview (Merton, Fiske, and Kendall 1956). With his long-term friend and collaborator Paul Lazarsfeld, and through the institution of the Bureau of Applied Social Research, he pursued through much of his career what one might think of as a professional practice in applied and problem-oriented research. Bureau research projects sometimes addressed issues the two men cared about as left-liberals, or to use a term more European than American, social democrats.¹ More consistently, the Bureau garnered resources from corporate and foundation sponsors to conduct research that enabled them to explore innovations in theory and methods (and not coincidentally to keep generations of graduate students employed and learning research by practical experience).

Merton's influence on twentieth century sociology derives from his example and passionate advocacy for an integration of theory and research; his emphasis on lucidity in prose, analysis, and most especially in concepts; and his training of an extraordinary group of early graduate students. It derives also from his formulations of explanatory paradigms, as he called them, that deeply shaped, reshaped, or even launched whole fields of research: anomie and deviance, bureaucracy, mass media, and science as a social institution. And it derives from his extraordinary ability to encapsulate whole intellectual agendas in crisp concepts: unanticipated consequences, opportunity structure, self-fulfilling prophecy, role model, and others.

Many of the concepts Merton coined passed into everyday usage. And in a similar way, many of his scholarly contributions passed into the everyday practice and collective history of sociology without any continuing

attribution. His influence, thus, is obscured by what he himself labeled "obliteration by incorporation." It is also obscured, however, by the tendency to read his work in fragmentary ways defined by his contributions to what has become a wide range of separate, specialized fields, and by sociology's own weakness in integrating knowledge across its subfields. Merton's contributions are many, but the significance of rereading Merton does not lie in the sum of them. It lies at least as much and perhaps more importantly in reinvigorating connections between theory and research and between different subfields in order to advance sociology in general, as a common enterprise, not a collection of discrete particulars. It is to this that the present volume is especially addressed, a project of history with systematic intent.²

Early Career

Robert Merton was born July 4, 1910, and his extraordinary life story evokes both the universalism of science and an American trajectory appropriate to his holiday birthday. Merton's parents were Jewish immigrants from Eastern Europe, and the future R. K. M. was born Meyer R. Schkolnick. The family lived above his father's small dairy products shop in South Philadelphia until it burned down, without insurance, and his father became a carpenter's assistant. Merton's family lacked wealth, but he insisted his childhood did not lack opportunity, and cited such institutions as a very decent public high school and the library donated by Andrew Carnegie in which he first read *Tristan Shandy* and more generally pursued a passionate self-education. Indeed, suggested Merton in 1994, the seemingly deprived South Philadelphia slum in which he grew up provided "a youngster with every sort of capital—social capital, cultural capital, human capital, and, above all, what we may call public capital—that is, with every sort of capital except the personally financial" (Merton 1994a).³

The name Robert King Merton evolved out of a teenage career as an amateur magician. Merton took up conjuring partly through taking his sister's boyfriend as a "role model" (to borrow a phrase literally his own).⁴ As his own skill improved, he sought a stage name, initially "Merlin." Advised that this was hackneyed, he changed it to Merton. Already devoted to tracing origins, he chose a first name after Robert Houdin, the French magician whose name Harry Houdini (himself originally Erich Weiss) had

adapted. And when he won a scholarship to Temple College he was content to let the new name (with its echoes of one of the oldest and greatest colleges at each of Cambridge and Oxford) become permanent. He entered the legal name change at nineteen.

At Temple, a school founded for "the poor boys and girls of Philadelphia" and not yet fully accredited or matured into a university, Merton chanced on a wonderful undergraduate teacher. It was serendipity, he later insisted. The sociologist George E. Simpson took him on as a research assistant in a project on race and the media—"the Philadelphia Negro and the Press"—and introduced him not only to sociology but also to Ralph Bunche and E. Franklin Frazier. Simpson also took Merton to the annual meeting of the American Sociological Society (as the ASA was called in those pre-acronym days), where he met Pitirim Sorokin, founding chair of the Harvard sociology department. He applied to Harvard, even though his teachers told him this was usually beyond the reach of those graduating from Temple. And when he arrived, Sorokin took him on as a research assistant. By Merton's third year they were publishing together—though note that in his second year Merton wrote articles that appeared in *Social Forces* and the *American Journal of Sociology*.⁵

In addition to Sorokin, Merton apprenticed himself to the historian of science George Sarton, not just for his stay at Harvard but for years of epistolary exchanges that Merton loved. It was Sarton who arranged publication of his doctoral dissertation. Merton resembled Sorokin and Sarton in his extraordinarily wide-ranging reading, but as he developed his specific style of sociological analysis other influences were central. He participated in Lawrence J. Henderson's famed Pareto reading group (alongside Talcott Parsons, George C. Homans, Joseph Schumpeter, Crane Brinton, and Elton Mayo). Pareto's idea of "motivating sentiments" was an enduring influence. And he decided late in his graduate student career to sit in on the first theory course offered by the young Talcott Parsons, just back from Europe and working through the ideas that would become *The Structure of Social Action*.⁶

The encounter with Parsons—serendipity again (perhaps)—did not just inform Merton's knowledge of European theory, but deepened his idea of sociology itself. Still, as he wrote later, "although much impressed by Parsons as a master-builder of sociological theory, I found myself departing from his mode of theorizing (as well as his mode of exposition)" (Merton 1994a:4).⁷ The laconic parenthesis is telling. Merton is among

the clearest and most careful prose stylists in sociology. He edited each essay over and again, even after publication, and left behind added footnotes and revisions both large and small to a host of his writings. It is easy to imagine that he might have been a professional editor had he not been an academic.

Indeed, it is easy to imagine the young Merton turning in any of several directions. His first articles, written as a graduate student and published in 1934–6, addressed the concepts of "Civilization and Culture," "The Course of Arabian Intellectual Development, 700–1300 A.D.," "Fluctuations in the Rate of Industrial Invention," "Science and Military Technique," and "The Unintended Consequences of Purposive Social Action." They appeared in journals of sociology, the history of science, economics, and simply science. As Alan Sica suggests in Chapter 8, Merton's early engagements were deeply informed by German approaches to the sociology of knowledge as well as by recent French sociology and by his own teachers. Merton turned away from this hermeneutic framework, Sica thinks mistakenly, as he did from Sorokin's approach to synthetic history. He was ultimately perhaps more Durkheimian.

Merton wrote his dissertation on *Science, Technology and Society in Seventeenth Century England* (Merton 1938a).⁸ This argued a "Merton Thesis" about the influence of Puritanism on early modern science, complementary to that of Max Weber on the relationship between the Protestant ethic and the spirit of capitalism. Narrowly, this was that "Puritanism, and as-cetic Protestantism generally, emerges as an emotionally consistent system of beliefs, sentiments and action which played no small part in arousing a sustained interest in science" (Merton 1938a:493). More broadly, Merton argued that social and cultural factors (including religion, economics, and military pursuits) shaped interest in science, scientific problem choice, and the public reception and influence of science. He resisted, however, the relativist conclusion that such external influences so shaped the internal content of science as to undermine its truth-value. But the study broke new ground simply for taking the explanation of the behavior of scientists as an empirical, sociological research problem. In the process, the book helped to invent the sociology of science.

Merton argued that science is misunderstood as the product of individual geniuses able to break free from conventions and norms. Instead, he stressed the "ethos of science," the normative structure specific to the field that encouraged productivity, critical thinking, and the pursuit of

continually improved understanding (Merton 1938b, 1942).⁹ This was significant not only as part of his historical explanatory project but also as the basis for grasping why science needed relative autonomy in the contemporary era. It offered the basis for a pointed critique of “Nazi science.”

To his disappointment, Merton was not hired on as a regular Harvard faculty member. He served a stint as an instructor, then got a good job at Tulane. Neither was a small thing in the midst of the Great Depression. Yet neither was a Harvard professorship, something denied to Merton at least partly because he was a Jew. At Tulane he became a full professor and Chair of the Department of Sociology within a year of his arrival. Then in 1941 he moved to Columbia University. His situation there was propitious. Columbia was entering an era of leadership and intellectual excitement, particularly in the social sciences.¹⁰ But this was not entirely an accident. Columbia distinguished itself among the elite universities of the Ivy League partly by shedding its anti-Semitism earlier and attracting distinguished Jewish faculty members. Location in New York was an added advantage.

The issue of Jewish identity is worth raising as a reminder that Merton, and also immigrants like Paul Lazarsfeld, had reason to feel insecure about their status in the university even as their prestige grew. Merton’s name change was not simply whimsical (even if the specific choice of a new name was in part). Nor was it idiosyncratically individual. It was part of the generational experience of Jewish immigrants and their children in the mid-twentieth-century United States. This no doubt attuned Merton to the relationship between social structure and social psychology generally, and to problems of race in particular. In 1940 he joined with the more extravagantly renamed Montague Francis Ashley Montagu to write a stinging rebuttal to an effort to explain crime in biological and partially racial terms (Merton 1939).¹¹ Montagu was born Israel Ehrenberg. It seems all too likely that the authors would have been received differently without the name changes (Haber 2008).

Still, by the time he was forty Merton was one of America’s most influential social scientists and had embarked on a lengthy career at Columbia University. A crucial component of this career was friendship and collegiality with Paul Lazarsfeld. This not only could not have been predicted, but one imagines that their senior colleagues at Columbia would have bet against it. The two men were hired to resolve a conflict between senior leaders of the department. Robert Lynd wanted an empirical re-

searcher; Robert MacIver wanted a theorist. Instead of hiring either at a senior level they hired relatively junior sociologists in each category. The two were wary of each other at first, but Lazarsfeld (slightly the senior) decided to reach out and invite Merton to dinner. In Merton’s reminiscence, friendship and collaboration alike were born when Lazarsfeld took him to watch a group interview session that was part of a study of audience response to war propaganda (run for the delightfully named Office of Facts and Figures, predecessor to the Office of War Information). Merton watched with interest then was quick to critique the research approach. The two men fell almost immediately into eager collaboration (at least as they used to tell the story), forgetting dinner and the wives they had left behind. This was the beginning of the trajectory that led Merton to develop the focused group interview (working in part with Patricia Kendall, a graduate assistant who was to become Lazarsfeld’s next wife). It was also the beginning of a remarkable collaboration. Ample honors and achievements would follow.

Context

Although Merton published enduringly important work while still a student and junior faculty member in the 1930s, he became truly famous only after World War II. He played a central role in shaping American sociology in an era of enormous expansion that was also an era of decisive professionalization. The field grew not only in numbers but also in public recognition and academic institutionalization. Two generations of remarkable researchers—those like Merton who received doctorates just before the war and those who entered sociology just after—at once established major lines of specialized research and ranged across them in major studies that influenced the field as a whole. Ambitions for the field were great and indeed many remember the period as sociology’s golden age. Yet the climax of this golden age came during the crisis of the 1960s. Protests inspired partly by sociological analyses were joined to criticism of older sociologists for accommodating themselves too much to dominant structures of American society.

American universities grew dramatically through the 1950s and ‘60s, supported by the GI Bill, a growing economy, a population boom, an infusion of immigrant scholars, and a renewed optimism that knowledge

could bring progress. Sociology was a relatively new discipline and the era of general expansion in higher education gave it a chance to grow. Indeed, sociology grew very disproportionately as one of the most popular fields for the burgeoning undergraduate population.

Still, the legitimacy of sociology was often in question.¹² Departments of sociology dated only from the 1890s. The field had grown in the early twentieth century with strong extra-academic ties—to Chautauqua, settlement houses, Christian socialism, and labor and reform movements. It had more than its share of radicals. This raised the hackles of more conservative academics, as it would again in the 1960s and '70s. During the 1930s, the ranks of sociologists actually shrank—the ASA lost a quarter of its members. Sociologists had not been central players in Roosevelt's New Deal (like economists and political scientists) and were concerned about bolstering their professional standing. Research in support of the war effort in the 1940s gave sociology renewed momentum (for historical context see Calhoun 2007). Merton himself did some of this research, partly in collaboration with Paul Lazarsfeld, and one project led to his second book, *Mass Persuasion: the Social Psychology of a War Bond Drive* (Merton, Fiske, and Curtis 1946; Merton and Lazarsfeld 1950).

After the War, sociology not only expanded and gained firm institutional bases in universities, but also brought research to bear on many of the major issues that animated public discussions, private anxieties, social conflicts, and government interventions. Foundations and government agencies called on sociologists for “applied” research. Merton was among those who responded. He was a leader in the sociological study of bureaucracy, including studies of business organizations and government agencies (e.g., Merton 1940; Merton and Devereux 1956).¹³ He addressed issues of technology and the transformation of work and workplaces (see Merton 1947). He examined the sociology of housing in an era of massive construction in cities and especially suburbs (see Merton et al. 1951a, 1951b).¹⁴ He conducted influential research on prejudice and racial integration.¹⁵ In the wake of McCarthyism and widespread censorship, he coauthored a defense and analysis of the freedom to read (see Merton, McKeon, and Gellhorn 1957). He analyzed the nature of medical education, and professionalization more generally (see Merton et al. 1957). And he addressed the nature and social significance

of mass communication in several studies (Merton, Fiske, and Curtis 1946; Merton 1949).¹⁶

Merton and other leaders sought both to lay strong foundations for the maturation of the discipline and to defend it against detractors from older, more established fields. To this end they also sought to strengthen its internal quality control and adherence to professional norms. Indeed, Merton presented his major book, *Social Theory and Social Structure*, as an effort to bring theoretical rigor to qualitative analysis (Merton 1968a).¹⁷ Likewise, particularly in partnership with Paul Lazarsfeld, Merton also sought to build institutions to strengthen knowledge production. Columbia was already a major center for graduate education in sociology when Merton arrived in 1941, but with Lazarsfeld and other colleagues he made it the single most influential base for Ph.D. training. Lazarsfeld's Austrian roots, Merton's knowledge of European theory and languages, and Columbia's location in New York made it especially attractive to a number of European immigrants like Lewis Coser and Peter Blau as well as to younger Americans like Alvin Gouldner, Peter Rossi, and James Coleman. Merton and Lazarsfeld built the Bureau for Applied Social Research as a research base, simultaneously gaining financial support from projects undertaken for corporate and foundation sponsors and involving graduate students as apprentices in projects from which they would learn the trade. At an interdisciplinary level, and emphasizing more purely academic work rather than applied research, Merton and Lazarsfeld played central roles in creating the Center for Advanced Study in the Behavioral Sciences in California.¹⁸

In effect, Merton and others resumed a sort of professionalization campaign that had started in sociology during the 1930s. The *American Sociological Review* was founded in 1936 as the official journal of the American Sociological Association amid a struggle in which the winning faction claimed to be professionalizers while others, more loosely organized but associated broadly with “Chicago-style” sociology, resisted both institutionalization of a new disciplinary hierarchy and a more exclusive emphasis on academic research over activism and public engagement. Merton was involved even as a graduate student, writing “The Unintended Consequences of Purposive Social Action” for the first volume of the *ASR* in 1936. But if Merton was unambiguously a professionalizer, he was nonetheless much more interested in and knowledgeable about earlier

American sociology than his fellow professionalizer and theorist Talcott Parsons. He drew extensively, for example, on W. I. Thomas and others associated with the Chicago School—not least in his use of “the Thomas Theorem” to develop his own idea of the “self-fulfilling prophecy” (Merton 1968b, 1993). He engaged actively in the study of social problems and publication to advance teaching (Merton and Nisbet 1961).¹⁹ He sought to integrate the different branches of sociology, seeking to avoid pitched battles. Already there were tensions between theorists and researchers, qualitative and quantitative researchers, advocates for pure science and for applied research, seekers after academic status and proponents of public engagement. It is instructive that Merton did important work on each side of each of these divisions.

Above all, Merton published journal articles that formulated issues in systematic ways, addressed them by developing concepts and theory that were informed by empirical research but still abstracted from particular cases, and suggested programs for continuing research. “Social Structure and Anomie” appeared in the *ASR* in 1938; “Bureaucratic Structure and Personality” followed in *Social Forces* in 1939 (see Merton 1968c, 1968d). Each became widely influential in the postwar period, partly because Merton included them in *Social Theory and Social Structure*, because his own growing prominence gave them added weight, and because he encouraged Columbia graduate students to follow them up with new work. Each began to be reprinted in anthologies from the late 1940s on. Each helped to define a subfield of sociology and enlisted other sociologists to advance the work each inaugurated. Merton celebrated the ongoing research process in articles and books on the “continuities” in different lines of work (see Merton and Lazarsfeld 1950; Merton 1968e, 1968f). This was different from offering a synthesis of existing theory, as Parsons did with great distinction by publishing *The Structure of Social Action* at about the same time (Parsons [1937] 1961).

Merton’s approach made his work influential but also encouraged what he would later term “obliteration by incorporation” (Merton 1968g, 1979).²⁰ He embraced the idea that good scientific work should contribute to making itself obsolete as science (though it might remain interesting as history). This he signaled by selecting an epigraph from Alfred North Whitehead: “A science which hesitates to forget its founders is lost.” As he explained in considering the development of reference group theory,

William James, Charles Horton Cooley, and W. I. Thomas all contributed insights that anticipated the eventual theory. But,

their conceptions were treated, not as a beginning but as a virtual conclusion, repeatedly quoted and illustrated with new examples of multiple selves, the looking-glass self, responses to the significant gestures of “others” and so on. And because the words of the forefathers became final words, little was built upon their insightful suggestions. They were honored, not in the manner in which men of science do honor to their predecessors, by extending and elaborating their formulations on the basis of cumulatively developed problems and systematic researches bearing on these problems, but in the manner in which litterateurs honor their predecessors, by repeatedly quoting “definitive” passages from the masters’ works (Merton and Rossi 1968:332).

Merton saw the process of scientific-knowledge creation as inherently incomplete, and saw premature closure as a problem. This was closely related to another crucial argument of Merton’s early work: that science is misunderstood as the product of individual geniuses able to break free from conventions and norms. Instead, he stressed the “ethos” of science, the normative structure specific to the field that encouraged productivity, critical thinking, and the pursuit of continually improved understanding (e.g., Merton [1937] 1973, 1973a). He was seldom happy when students left the Mertonian fold in their efforts to push sociology forward, but he recognized that this was how science worked—and analysis of scientific and sociological ambivalence was among his themes (Merton 1976).

In the same spirit, the present book does not remain entirely at the level of celebration. To be sure, its engagement with the work of Robert Merton is partly an effort to strengthen our grasp of the history of the discipline. But it is even more an effort to invigorate sociology today, strengthening connections among subfields and between theory and research so that sociology can keep improving. It is intended to help sociologists take up a range of issues and see how they could be addressed more clearly and productively. This includes examining critically the limits of Merton’s formulations or their received interpretations, as for example Robert Sampson (in Chapter 3 below) considers how well Merton’s influential theory of deviance squares with contemporary research, and Thomas Gieryn and

Aaron Panofsky consider Merton's sociology of science in light of more recent developments in Chapters 6 and 7.

Influence

Robert Merton was the primary founder of the sociology of science, an enormously influential sociological theorist, and an innovator in empirical research methods. His work continues to be cited and used in the study of social structure, social psychology, deviance, professions, organizations, and culture as well as science. But, though cited frequently, Merton's work is often not read deeply—the citations are part of a ritual of the reproduction of status and legitimacy that Merton himself analyzed (Merton 1973b; Cronin 1984; Small 2004).

On the one hand, there are citations, especially in science studies, that use Merton to identify an older approach to which authors contrast their own claims to be part of the new. Real disagreements did indeed separate Merton from new trends in science studies in the 1970s and '80s, principally about the capacities of social-institutional analysis and about how much respect to accord science as a successful project of knowledge production. But a generation later many writers cite without reading, and seek simply to symbolize their distance from a rejected approach. Commonly, they misrepresent Merton, for example treating his account of the normative order of science as though it were a simplistic (and therefore false and naïve) account of actual scientific practice. And it is not only in science studies that certain of Merton's publications are cited more as icons of a caricatured position than for the substance of their arguments.

On the other hand there are citations that claim Merton for the lineage of Great Thinkers on the shoulders of which a current analyst seeks to stand. While some of these are thoughtful, many are based on little more actual engagement with Merton's texts than the dismissive citations of those who want to distance themselves from him. Several of Merton's contributions have yielded phrases in common usage, but their provenance is often forgotten and their intellectual significance frequently reduced by remembering the catchphrase and not the context in which it was introduced.

Obliteration by incorporation is perhaps the happiest reason Merton's work is not as well known today as his enduring fame and his influence during the postwar era would suggest. Obliteration by incorporation may

be bittersweet, but there was no doubt satisfaction in seeing ideas he introduced and fields he helped create both absorb what he had offered and move forward in continued creativity. Merton lived to see his concept of opportunity structure become prominent in new contexts, and to see at least the beginning of the recent vogue for identifying causal "mechanisms" that can function in explanations of disparate phenomena, which of course reproduces important aspects of his notion of middle-range theories (see discussion by Charles Tilly in Chapter 2 below).

But the significance of Merton's work is also obscured for three other reasons, each a bit less happy. First, there was a broad turn against functionalism in the 1960s and 1970s. Though Parsons was a more central target, there was a tendency to see Merton's work as part of an undifferentiated mass of functionalist theory. Merton was indeed a functional analyst, but this categorization both misleads generally and obscures many specific contributions not dependent on functionalism or any other paradigm. Merton also suffered simply from being placed on the "old" side of a generational divide reinforced by both political and theoretical objections. This happened even though Merton had distanced himself from doctrinaire, all-encompassing functionalist synthesis, emphasizing for example that middle-range theories such as role sets could be compatible with Marxism as well as functionalism and other very different theoretical frameworks (Merton 1968h:43). It happened even though he had been much more sympathetic to critical perspectives than many others in sociology's elite—in different ways sponsoring both Alvin Gouldner and C. Wright Mills.²¹ And indeed it happened even though Merton denied that social cohesion could be assumed as "normal" and was more attentive to the role of conflict than other leading functionalist theorists.²² Despite all these ways in which Merton was less extreme in his functionalism, he was in the end still perceived as arguing that overall "the system" worked.

Whatever intellectual reasons may have mattered, younger sociologists also saw Merton as much too identified with a normatively "professional" idea of the sociologist's proper role in an era that they thought demanded activist engagements. Not least, when faced with the intra-university struggles of 1968—as intense at Columbia as anywhere—Merton found himself unable to side with protesting students against the administration (including his friend Jacques Barzun) and it was a moment when there was not much of a liberal middle ground to claim.

The charged political context made rejecting functionalism something of a litmus test that seemed to reveal theoretical sophistication as well as political correctness. What self-respecting graduate student would want to support a theory so thoroughly criticized? Ironically, this was reinforced by Parsons's presentation of functionalism as an all-encompassing system (not to mention his prose). What this obscured was the possibility of recognizing functional analysis (or, as Merton preferred, structural-functional analysis) as one exceptionally useful sociological approach, but only one tool among several.

Merton tried to avoid elevation (or reduction) of a theoretical framework or analytic perspective to an orthodoxy or "ism." This approach fits poorly with the tendency to teach theory as a matter of great systems associated with individual authors. But taking Merton's approach seriously would not mean giving license to empirical researchers to produce analyses devoid of theoretical reflection. On the contrary, it would demand more self-reflective explanatory work, integrating reflection on analytic strategies such as functional analysis, including the critiques of functional analysis, into work that would be simultaneously empirical and theoretical.

This raises the second reason the importance of Merton's work is systematically obscured, the common practice of teaching sociology in three tracks: (1) general theory (often bundled with the history of sociology, and presented as the succession of theoretical orientations more than the cumulative development of explanatory power); (2) methods (focused heavily on techniques of statistical analysis, rather than "methodology" as the understanding of the how different methods work and how the choice of methods influences research); and (3) empirical subfields, each more or less disconnected from each other and from general theory and methods. It was a virtue of Merton's work to combine the three. And the effort to improve their mutual connections should be a goal now.

Indeed, one might interpret Merton's work as an implicit critique of the way in which sociology has separated these domains. Merton saw the project of sociology as a matter of producing increasingly clear accounts of social life that identified general processes and thereby allowed for the study of variation and change and that revealed the connections between different empirical instances and dimensions. As he put it:

Each to his last, and the last of the sociologist is that of lucidly presenting claims to logically interconnected and empirical confirmed propositions

about the structure of society and its changes, the behavior of man within that structure and the consequences of that behavior (Merton 1968h:70).

Merton's metaphor is noteworthy: the shoemaker's last invokes an image of sociology as a craft. The sociologist uses tools (theory, methods) to produce a specific sort of object (systematic knowledge of society and social behavior). The production process includes empirical investigations that generate new findings but also, centrally, efforts to interpret the significance of those findings in light of other research, careful conceptualization, and a continual process of integration of sociological knowledge.

Merton does not argue against seeing sociology as a science. On the contrary, the quoted passage follows a discussion of why codification of sociological theory into paradigms is crucial if sociology is to become a science like chemistry, physics, or biology. Nor does he favor a purely empiricist approach. Paradigms have the function of bringing central concepts and their interrelations into simultaneous view. They lessen the likelihood of smuggling hidden assumptions and concepts into theory. They advance cumulation of theoretical interpretation, call attention to gaps, inconsistencies, and other problems, and enable qualitative analysis to attain rigor often associated only with quantification. "Paradigms for sociological analysis are intended to help the sociologist work at his trade" (Merton 1968h:70).²³

As Alejandro Portes shows in Chapter 1 below, the intellectual work of research starts with establishing the phenomenon under study. Merton recognized this, arguing that effective analysis cannot be a response to data as such without prior conceptualization. Conceptualization itself needs to be thoughtful and based on clear understanding of alternatives and their implications. Significant intellectual labor is involved in the task (though it is too often slighted). Merton built on Max Weber's notion of ideal types and his practice of sociological semantics was intended to improve this process.

Only with the phenomena established and a clear grasp of analytic strategies in mind, Merton suggested, could researchers be effective in generating explanatory models that might reveal generalizable features. Generalization is not a matter of facts that hold true without restrictions of scope, but rather of significant explanatory models that can work in different domains. This is at the heart of his idea of theories of the middle range. Science could advance by developing explanations of particular

phenomena, then identifying causal models that could be abstracted from the particular cases, and then studying the extent to which these might operate in other kinds of phenomena. The generalizations might always depend in part on analogies, rather than strict universal identities. But the process would enable analysts to become clearer both about how causal processes worked, and about the commonalities across analytic domains. The strategy anticipates that which has been discussed more recently under the rubric of “mechanisms” (as Tilly notes below). A specific model is transposable into new domains, even where other empirical factors differ. And here is a key way in which research and theory are integrated.

Third, Merton’s work is inadequately understood today because it invites a fragmentary reading in which researchers find pieces they can use—or argue with—in their own subfields and pay little attention to the whole. Each fragment is understood mainly as a contribution to a different explanatory problem in a different subfield. Even those who do draw in deeper and more substantial ways on Merton’s writings typically draw on a subset identified with a particular line of sociological theory and research. Few of those who draw on Merton’s theory of deviance have much awareness of his work in the sociology of science; those who recognize his contributions to the study of bureaucracy may not even know of his work on anomie.

In a certain sense, this is the product of Merton’s own approach. He argued for the integration of sociological theory into research. He accepted the development of specialized research fields as necessary to the maturation of the discipline. And he held that broad theoretical synthesis was at once premature and less helpful than the development of theories of the middle range (Merton 1968h, 1968j, 1968k). Merton presented his work largely in discrete essays. Many of these essays introduced an elegant term or phrase to identify the analytic strategy deployed: “unintended consequences,” “manifest and latent functions,” and indeed “middle-range theories.” The phrases served as mnemonic devices but often came to be remembered by themselves with little connection to Merton’s original argument. Moreover, each of Merton’s essays was crafted as a “whole,” not immediately invoking or demanding a larger framework for its understanding.

One might compare the work of Talcott Parsons. Not only did Parsons write mainly in the form of long books. The phrases he introduced generally derived their meaning from labeling a feature of the larger architecture of his theory. “Pattern variables” is not complete in itself; it demands

explication, a list. It offers an invitation to expositors, interpreters, and even critics. Merton never wrote an exposition of his overall intellectual perspective codified as a “system of thought,” which means that though he was widely known as a theorist, his work is relatively refractory to reading and teaching as “theory.”

Enduring Importance

Nonetheless, as Szrompka suggests, “Merton’s work constitutes a coherent system of thought, not a scattered set of contributions” (1986).²⁴ The effort to develop a more integrated perspective on Merton’s work is well worthwhile and several of the chapters below are helpful in this regard. But the higher stakes are in developing an integrated perspective on sociology. Here the point is less the integration of Merton’s work than the assistance Merton’s work offers in thinking about the ways theory, methods, and research can be better integrated.

Merton’s understanding of scientific work centers on individual scientists who engage in practical problem-solving activity.²⁵ They may conduct experiments or gather field observations, but they do so in ways focused by the attempt to resolve intellectual problems (whether these are raised by difficulties using existing knowledge in practice or by efforts to improve knowledge for its own sake). These problems enable them to choose strategic sites for and approaches to research.²⁶ Their work is guided by broad value commitments (the norms of science) and organized in an institutional structure (which both constrains and rewards), and is in principle cumulative. Merton does not deny that institutional structure may extend to a division of labor that assigns different parts of the overall process to different workers, but neither does he focus on the possibility that this would undermine the craft character of science, including the integration of its different dimensions in the work of individual craft scientists. Merton was himself such a craft scientist and he sought to nurture the same integrated approach in his students.

The development and expansion of sociology as a discipline, however, came with a deficit in integration. While division into subfields might be part of a productive division of labor, this would require a stronger performance than sociology has exhibited in connecting different research domains through theory and mobilizing research to advance theory. Equally,

it would require integrating the selection and improvement of research methods more into a common conversation about theory and research strategy. Too often sociological methods are taught as mere technique. Too often researchers simply deploy the techniques they have mastered as hammers for which all research problems are nails. Too often they master only those techniques currently in fashion, rather than taking seriously the strengths and weaknesses of different methods, analyzing their capacities to disclose and their tendencies to obscure.

Parsons of course thought that his functionalist theory could provide the basis not just for connection but also for holistic synthesis. Merton was delicate in criticism of his friend and sometime teacher. He may have distinguished his approach to middle-range theory in part simply to create a space for his own work that would not involve a direct confrontation. But he also worried both that Parsons's attempt at grand synthesis was premature (his main public criticism), and also that its very holism would maximize its autonomous standing as one among other great theories, but inhibit its role as part of a living, continuously improved integration of theory and research. This seems indeed to have happened. To be sure, for twenty years a great deal of sociological research and analysis was presented in Parsonian—and more generally functionalist—vocabulary. And Parsonian theory was a central target of those who sought to “shift the paradigm” of sociological analysis in the 1960s and '70s. Merton was swept up in the same maelstrom, and his work more fully eclipsed than Parsons's—partly because it wasn't presented as an integrated theory to be arranged in the series of sociological classics or to be attacked from the vantage point of another integrated theory.

To a considerable extent Parsons synthesized previous theoretical work. His functionalist synthesis could inform research but didn't produce a dynamic interaction of research and theorization. What Merton called for was synthesis of ongoing empirical research, including both its explicit and implicit findings and interpretations of their significance. The synthesis would provide guidance in the development of new research projects and constitute a systematic summary of what was known. It would constitute knowledge not as so many particulars but as a set of models for how one or another feature or dimension of social life worked.

When C. Wright Mills famously mocked the sociology of the 1950s as divided between “abstracted empiricism” and “grand theory,” he offered a critique very much in line with Merton's thinking. Mills didn't directly

attack Merton, but he infuriated Merton by his disrespectful tone and by taking Merton's friends Lazarfeld and Parsons as representatives of the two denigrated extremes. Anxious for sociology to gain respect and standing as a science, Merton hardly thought Mills's populist critique productive. But he and Mills shared the sense that the real action was neither in purely theoretical synthesis nor in the accumulation of an ever-larger body of apparently factual information however sophisticated the methods used to construct or present it. Much more than Mills, Merton labored to demonstrate what might lie between the two poles.

Merton's influence—and that of his partnership with Lazarfeld—was expressed partly through Merton's extraordinary teaching and work as a mentor to young researchers. Impressively, many of these attained distinction both as theorists and researchers. Many worked also with diverse methods; for example, such famous “quantitative” sociologists as Peter Blau and James Coleman did some of their most influential research through qualitative fieldwork.

Among Merton's students were such disparate but important sociologists as Peter Blau, James Coleman, Jonathan and Stephen Cole, Lewis Coser, Rose Coser, Alvin Gouldner, Seymour Martin Lipset, and Alice Rossi (as well as several contributors to this volume including Cynthia Epstein, Viviana Zelizer, and Harriet Zuckerman). In the work of all, even those who took up different paths from Merton's own, one can see not only Merton's specific ideas but also the distinctive style of combining theory and research characteristic of Columbia sociology during his time there. Four features were especially important to this approach:

1. The attempt to be theoretically explicit enough, but also modest enough, to produce theoretical sociology that could be continually improved through application and testing in empirical research (including research assessing practical action and historical experience)
2. The use of theoretical analyses to formulate empirical research agendas and analytically useful concepts that would both open up new insight into their immediate objects and enable systematic comparison and identification of general features in the specific cases
3. The development of new lines of inquiry, and where necessary new methods of research, in order to pursue intellectual problems of major significance (rather than merely repeating or refining existing models or generalizations)

4. The attempt—institutionalized as much through the Bureau of Applied Social Research as through the Department of Sociology—to combine deep scholarship and high scientific standards, with attention to important social problems and the effort to inform practical action

The first two of these are relatively familiar, indeed all but canonical and frequently restated in textbooks, though in fact the relationship between theory and research in sociology is tenuous and problematic. The third is equally important, and more often overlooked. It would please Merton, for example, to see the arguments offered in Chapters 4 and 5 by Cynthia Fuchs Epstein and Viviana A. Zelizer for ways in which his work contributes to tackling sociological problems on which he did not focus.

The fourth point is one on which Merton—and indeed Lazarsfeld—were, I think, ambivalent. Merton did not link his politics directly to his sociology. Yet, Merton did enter into public discussions on themes he thought properly informed by scholarly knowledge, such as censorship. Certainly, Merton did major research on topics of public interest and his sociological work informed practical efforts to address social problems, including not least the Supreme Court's Decision in *Brown vs. the Board of Education*. Yet, Merton and Lazarsfeld kept their “applied” work at the Bureau organized as what I have called a parallel professional practice. Applied research paid for studies that could also advance scientific sociology, though that was not usually the object of those who paid. It was important, then, not to let scientific pursuits be reduced to the level of the specific analyses funders sought.

Merton argued repeatedly that scientific knowledge advanced on the basis of concepts, paradigms, and middle-range theories. These could be developed only in analytic work that depended on abstraction from empirical data and indeed abstraction from the immediate particulars in which social issues appeared in everyday life. As it advanced, sociology offered more and better tools for grasping concrete situations and informing policy. As Merton knew from his earliest research, motivations for science could come from outside science, and ideas originating elsewhere could inform science. But scientific cumulation depended on some level of autonomy for science, from politics and public dispute. Merton worried when C. Wright Mills seemed to breach the boundaries and he worried even more in 1968. His model for what has since been called “public soci-

ology” (by his friend and Columbia colleague Herbert Gans) was based on the socially responsible use of professionally mastered expertise.

Merton was strongly drawn to efforts to structure an integrative approach to sociology, one that would strengthen the field inside universities and in public esteem. By the 1960s—even earlier in some cases, including Mills—others thought that this encouraged too much complexity between sociologists and existing social hierarchies and power structures. Many thought this integrative approach dampened conflicts within the field that could be intellectually productive for it. Many would have preferred more open confrontation with social problems. Struggles over these issues were fought out partly at Columbia University, home to C. Wright Mills and Immanuel Wallerstein as well as Merton and Lazarsfeld, and within the ranks of Merton students, which included Alvin Gouldner. Merton's preference for professionalization—and with it integration, codification, and abstraction from immediate issues—seemed stifling to many, not least in the 1960s.

Indeed, Merton's influence was greatest from the era of World War II to the mid-1960s. He had important students later and he published important work later. But in many ways the context in which he had been most effective was in decline. Paul Lazarsfeld first retired and then died in 1976. Merton settled into the role of senior resident sage at the Russell Sage Foundation, putting his set of editorial stamps to work on the papers of visiting fellows rather than graduate students. He continued to write, both for immediate publication and for files of what promise to be an impressive body of posthumous publications.

Interestingly, during the 1960s Merton also ended his “parallel professional practice” of problem-oriented and applied social research organized through the Bureau. In part he had attained a position where he didn't need the resources applied research brought. In part his interests had shifted to renewed engagement in the sociology of science, and to a mixture of projects in theory and intellectual history that he could pursue in the library rather than in the collective research team. One of these was “sociological semantics,” which both Peter Simonson and Harriet Zuckerman address below (in Chapters 10 and 11 respectively). Closely related was the history of ideas that Charles Camic discusses in Chapter 10 and that bore fruit in two remarkable books on the frontiers of literature, history, and sociology: *On the Shoulders of Giants* (Merton 1965) and *The Travels and Adventures of Serendipity* (Merton and Barber 2003).

Merton not only coined but also studied memorable phrases and the patterns of association and evocation in which they were passed on, not least as they informed scholarly reference and the development of reputations. Thus, famously, he traced the phrase, "if I have seen farther it is by standing on the shoulders of giants," through centuries of use. The phrase is most commonly associated with Sir Isaac Newton, though with the widespread success of *On the Shoulders of Giants* Merton must be a very close second. What Merton showed with dazzling erudition and more than a few entertaining digressions was that the aphorism originated with Bernard of Chartres in the twelfth century. This corrected not only those who cited merely Newton but those who credited the phrase to ancient authors, including apparently nonexistent ancient authors, perhaps thinking thereby to accord it greater dignity and impress readers with their Latin references (and here let us not forget the South Philadelphia high school that taught Merton four years of Latin).

Merton's book became famous enough to be known (at least among initiates) by the acronym "OTSOG." This was partly because it was so engagingly written, a scholarly detective story in the form of an extended letter to his friend Bernard Ballyn, a compilation of associations and sometimes improbable connections that invited the allusion to *Tristram Shandy* in the subtitle. But it is also a serious inquiry into the phenomena of scholarly reference and citation, the development of reputations, and the place of science amid humane knowledge.

Merton continued to address the relationship between the first appearances of ideas and the occasions when they begin to have more serious influence, noting how many basic scientific advances were anticipated by "prediscoveries" that failed to change the way scientists thought (Merton 1973c, 1973d). That in turn opened up the question of why this should be, whether in any specific case it was because the "discoverer" lacked stature, or because the context wasn't ready, because a crucial connection wasn't made, or because an empirical or practical test wasn't identified. The role of chance connections—serendipity—in scientific breakthroughs became another enduring focus for Merton's boundless curiosity and careful scholarship.

Merton also advocated for "sociological semantics" as a line of research into how verbal formulations influenced substantive sociological thinking. Explored further by Harriet Zuckerman and Charles Camic in Chapters 11 and 12 below, this builds on the still-neglected insight that conceptualiza-

tions actually matter in scientific work, and are part of how it advances, not merely more or less felicitous summaries of knowledge already established. A concept like "unanticipated consequences," which Merton introduced in a 1936 article, clarifies a phenomenon, making it "visible" for further study. In this case the concept was taken up by a host of researchers across the range of social and behavioral sciences, and it opened up new directions of empirical research as well as theorizing. Moreover, the clear, revealing conceptualization is as much a methodological tool as an element in theory (more precisely, it is both at once). As Peter Simonson shows in his chapter, this understanding is shaped by Merton's own early explorations in the field of rhetoric. Rhetoric also remained an important influence in Merton's sociology of science, as Ragnarvald Kalleberg demonstrates in Chapter 9.

The sociology of science remained the field closest to Merton's heart. He had never entirely abandoned it, but returned to it as his central focus in the 1960s. Thinking it was obvious that science was a social institution of pivotal importance to modern society and that sociology offered crucial resources for its study, he was repeatedly surprised by weak disciplinary interest. This was not merely a personal disappointment for Merton, but, he thought, a danger for the emerging interdisciplinary field of science studies, which needed sociological perspectives. But by a sociological perspective, Merton meant largely an institutional one. And from the 1970s, the sociology of science turned, in large part, away from the study of institutions and toward microsociology of scientific practice. Chapter 6 by Thomas Gieryn situates Merton in the sociology of science, not only historically but also as part of a paradigm for new research.²⁷

Many in the field were critical of Merton's emphasis on the norms of science. This seemed to some apologetic and to others idealistic. In any case they pointed to the frequency with which they saw these norms violated. More generally, structural-functionalism was challenged by a variety of perspectives placing greater stress on self-interest and conflict. Merton's work was often cited as emblematic of the now diverted "mainstream," though this was somewhat ironic, since among leading functionalists, he was particularly attentive to dysfunction, historical change, and conflict. Late in his life he worried that the approach of many in science studies was so relativistic and one-sidedly focused on debunking that it made it hard to see the importance of the relative autonomy of science as a social institution.

Conclusion

In short, Merton was one of the towering figures on whose shoulders contemporary sociology rests. He was without question among the most influential sociologists of the twentieth century. He left behind an extraordinary legacy of sociological publications (and unpublished work), of students who carried on various lines of inquiry he helped to launch, and of others he influenced through collegueship or correspondence.

If Merton's work is less well understood than it should be, this is in fact an opportunity. Revisiting Merton's writings is a source of innumerable insights. Footnotes suggest whole research agendas. Essays still have the capacity to clarify whole fields. Reading Merton is also a reminder of the importance and value of scholarship as such, for even when his arguments are distilled into clear and straightforward prose they reveal foundations in deep and systematic knowledge of previous work (as Alan Sica brings out in Chapter 8).

But even more, there is in reading Merton an opportunity to think anew about how different styles and branches of sociology can better inform each other and strengthen the field as a whole. We can see in Merton's sociology of science not just one more sociological specialty but also one that can help sociology gain capacity for reflection on itself. We should think with Merton about the ways in which theory and research can inform each other, the ways in which middle-range theories can connect empirically disparate subfields, and even the ways in which "applied" research can underwrite scholarly innovation. We needn't always agree with Merton. Even arguments can be productive.

Notes

1. Merton and Lazarsfeld were each politically on the left, particularly in their youth; Lazarsfeld had been an active socialist in Austria. But even in their early Columbia years, their engagements and styles were more professional than political. Robert Lynd, the activist researcher who supported hiring Lazarsfeld partly because he was so impressed with his early study of unemployed workers, repeatedly demanded to know where was his social conscience. Lazarsfeld recalled answering, rather weakly, "Well, that begins after five o'clock" (Smith 1995:150). But the truth was perhaps at least as much that Lazarsfeld's socialist engagements were active in his Vienna milieu

and didn't survive his migration to the United States, though he said he remained a socialist "in my heart." Lazarsfeld also described his social research as "a kind of sublimation of my frustrated political instincts"—a sublimation reinforced by being an immigrant (see Sils 1987). There may have been something of this "sublimation" in Merton's intense professional engagement as well. He was more engaged in social issues than Lazarsfeld, particularly integration. But he channeled most of his engagement through research and efforts to strengthen the discipline of sociology itself, rejecting perhaps both his response to the hostile environment of anti-Semitism and McCarthyism and his conviction that this academic project would matter for public progress. As Smith recounts, Lynd, who had attracted a great deal of attention after the Middletown studies, lost influence and students to Lazarsfeld and Merton precisely because of the stronger professional engagements of the younger men and the greater help they could offer those forging academic careers.

2. By contrast to Merton's famous distinction in "On the History and Systematics of Sociological Theory" (Merton 1967).

3. Composed of his own reflections, the 1994 Charles Horner Haskins Lecture is the most important source for Merton's biography.

4. This was not an altogether amateur or casual undertaking. The boyfriend was Charles H. Hopkins, author of "Ours": *Precarious and Challenges for Ambitious Card Workers*. After Hopkins died in 1948, the Society of American Magicians, Assembly #4, named its annual award in his honor. Merton dedicated *Social Theory and Social Structure* to Hopkins when it was first published the next year.

5. The latter, a pioneering presentation of Durkheim's newly translated *Division of Labor in Society*, was still being reprinted decades later. Sixty years on, Merton recalled the origin of those two first articles. Sorokin had been invited to speak to the Eastern Sociological Conference (a precursor to the Society) on recent French sociology. He couldn't make it and asked Merton to do it in his stead. Not only did the second-year graduate student rise to that challenge, his text was published and also drew the attention of the editor of the *AJS*, Ellsworth Faris, who solicited the second article. Faris himself, as it happened, wrote a dismissive review of Durkheim's book—"naïve, brutish and short" as Merton recalled it—emphasizing the poor empirical source materials from which Durkheim worked. Merton praised the book, trying analytically to bring out its theoretical contributions, but castigated the "infelicitous translation" by one George Simpson, though anxious that it should be clear that this was not the George E. Simpson who had been his undergraduate mentor (See Merton 1994b).

6. Decades on, Merton was at pains to clarify that he went to Harvard to work with Sorokin, having no idea of Parsons's existence: "Parsons had no public identity whatever as a sociologist. He had published just two articles deriving from his dissertation . . . and these had appeared in the *Journal of Political Economy*, a journal it is fair to suppose not much read by undergraduates in sociology bent on deciding where to

do their graduate work. . . . I do no injustice to Pitirim Sorokin's memory by reporting that although we students came to study with the renowned Sorokin, a subset of us stayed to work with the unknown Parsons." (Merton 1980:69)

7. Though Merton was clearly conscious of Parsons's reputation for dismal prose, which he could gently evoke in the quoted passage, he elsewhere went out of his way to praise the "exceptionally clear, direct, and most un-Teutonic English prose" of Parsons's translation of Weber's *The Protestant Ethic* (Merton 1980:69).

8. For a sampling of the extensive commentary over the years, see Cohen 1990.

9. See discussion by Kallberg in Chapter 9 below. Later, of course, Merton would address in more detail the social institutions—including reward systems—that supported this ethos.

10. *A History of the Faculty of Political Science, the Bicentennial History of Columbia University*, published by Columbia University Press, 1955.

11. The influential Harvard physical anthropologist Earnest Hooten was the target of its dominant position in the late 1950s and early 1960s—let alone the struggles of the later 1960s and 1970s—misses the importance of the very different struggles during the 1930s, '40s, and '50s to establish sociology as a leading academic field.

13. Merton's classic "Bureaucratic Structure and Personality" was published in 1940 and reprinted in *Social Theory and Social Structure* (1968d). In addition to a reader in bureaucracy and several other articles, he produced a two-volume report on the use of opinion research and statistics in the AT&T Corporation, *The Role of Social Research in Business Administration*, with E. C. Devereux, Jr (1956).

14. "Social Policy and Social Research on Housing" is the special issue of *The Journal of Social Issues* (1957a) that Merton edited in 1951 with Patricia S. West, Marie Jahoda, and Hanan C. Selvin. This brought into print fragments of a major study by the same authors, *Patterns of Social Life: Explorations in the Sociology of Housing*, which was never formally published and available only in mimeographed form from the Bureau of Applied Social Research. See discussion in Merton 1999. The central intellectual theme of the housing studies was that social structure shapes patterns in social psychological response to factors like racial integration.

15. Merton's studies of integrated housing were cited in the appellant's brief that led to the landmark desegregation ruling, *Brown vs. the Board of Education* (Merton et al. 1957a, 1957b; Merton 1948a). Merton was a signer of the statement "The Effects of Segregation and the Consequences of Desegregation: A Social Science Statement" that Kenneth Clark drafted as a supplement to legal briefs being sent to the Supreme Court (see Clark 1953). See also Merton 1948b.

16. *Mass Persuasion* (1946) is Merton's most sustained treatment. He also coauthored several studies with Paul Lazarfeld, including "Patterns of Influence: A Study of Interpersonal Influence and Communications Behavior in a Local Community" (1949).

17. Merton first published this collection in 1949 and it became a shaping influence on postwar sociology. A revised edition was published in 1957 and then the enlarged edition in 1968.

18. The creation of the Center was the source of one of the few enduring quarrels between Merton and Lazarsfeld. Merton played a leading role in the rejection of Lazarsfeld's plan for a hierarchical teaching organization in favor of an institution in which those chosen for membership would be relatively equal and autonomous as they pursued their own projects within an intellectual community.

19. *Contemporary Social Problems* was the anthropology he edited with Robert A. Nisbet through four editions beginning in 1961. Merton was also for many years a consulting editor for Harcourt and in this role an influence on what was arguably the first great textbook of postwar sociology: Leonard Broom and Philip Selznick, *Sociology: A Text with Adapted Readings* (1963).

20. In Merton 1968g; see esp. 27–28 and 35–37.

21. And if Mills and Gouldner both became leading critics of professional American sociology, neither made Merton a target. Merton regarded Gouldner as among his very best students (even if a difficult person). Merton had brought Mills to Columbia and was often his defender, not least in conflicts with Lazarsfeld. Merton did grow exasperated with Mills and was offended by parts of *The Sociological Imagination* (even though he personally was treated gently).

22. In the later regard, he shared much with the anthropologist Max Gluckman; and more generally, Merton's structural-functionalism reflected not only a distancing from Parsons but an embrace of a perspective widespread in social anthropology.

23. As Merton uses the term, "paradigms" are more limited than the broad structures integrating the state of scientific knowledge in a particular era that are analyzed by Thomas S. Kuhn in *The Structure of Scientific Revolutions* (1962). For Merton, paradigms are systematizations of the analyses developed in particular lines of work. For example, he regarded his accounts of deviant social behavior in "Social Structure and Anomie" (1968c) and of "Manifest and Latent Functions" (1968i) each as paradigms.

24. Sztompska's work is perhaps the best starting point for one seeking an integrated perspective on Merton's work. See also Crothers 1987. Two anthologies are also noteworthy though both (like the present volume) are largely focused on specific contributions and their relationships to subfields. See Coser 1975, especially Sinche-combe's chapter (1–34); and Mongardini and Tabboni 1998.

25. It is worth noting that the exemplars in his historical sociology of science come mainly from the age of heroic amateurs, especially the seventeenth century, not the era of science based in universities (or for that matter industrial or government labs). Scientists like Newton and Kepler did engage simultaneously in empirical research, methodological innovation, and theorization. Of course many of their contemporary works were less heroic and less polymathic. Then as now many contributed empirical

observations without clarity as to how to theorize them, though admission to a body like the Royal Society involved less work-discipline than, say, a postdoctoral fellowship in chemistry today.

26. For the notion of "strategic research site," see Merton 1959. Merton's main development of this notion came in "Multiple Discoveries as Strategic Research Site," first published as part of "Resistance to the Systematic Study of Multiple Discoveries in Science" (1963).

27. There is no single authoritative study of Merton's sociology of science. Indeed, Merton himself never attempted this, and the closest substitute is an edited collection of Merton's work on science with a substantial introduction by Norman Storey, *The Sociology of Science: A special issue of Social Studies of Science* in 2004 is interesting, but also very incomplete. One of the multiple festschriften honoring Merton focuses helpfully on his studies of science (Gieryn 1980). A recent special issue of the *Journal of Classical Sociology* (2007) offers several engagements with Merton's work.

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