Abstract

Agassi, Sztompka, Kincaid and Crothers argue, in various ways, that Merton should not be held responsible for his published views on theory construction, and they provide psychological or strategic explanations for his failure to resolve issues with these views. I argue that this line of defense is unnecessary. A better case for Merton would be that theories in his middle-range sense were a non-technical alternative solution to the problem of spurious correlation. Middle-range theory was not, however, a solution to the problem of diverse approaches. Merton also did not resolve the problems with his account of functionalism and the problems undermine the claim that he had a distinctive “structural” approach all along.

Keywords: Robert K. Merton, middle-range theory, theory construction, spurious correlation, Paul Lazarsfeld.

The most widely read and discussed of Merton’s writings are those collected in Social Theory and Social Structure ([1949, 1957]1968). The book includes his papers on ‘methodology’ broadly construed, which are the most important papers in the collection. Where they can be broken out, they are far more heavily cited than the next most cited paper in the collection, on reference groups, which was an idea from Stouffer’s American Soldier (1949), not a Merton
innovation. The other non-methodology papers in the collection are mostly forgettable. The book in its various forms is cited about 10,000 times in Google Scholar: not included in the book are the two next most cited Merton papers, “Social Structure and Anomie” (1938) and “The Matthew Effect in Science II” ([1968]1973). If citations mean anything, as Merton himself thought they did, these ‘methodology’ papers are Merton’s primary claim to fame (cf. Garfield 1980).

Strangely, each of the commentaries on my discussion of these papers argues, in one way or another, that Merton cannot be held responsible for these influential texts. Sztompka dismisses them as time-bound polemics against Parsons, and points to a paper by Merton and Lazarsfeld on friendship (1964) as representing the real Merton-- a paper cited some 400 times, about 4% of the citations for STSS (2009, 262). Agassi says that Merton is really a defender of a liberal view of science, and Kincaid characterizes what Merton says as “public relations . . .” (2009, 270). Crothers tells us what he thinks Merton really believed, which turns out to be not significantly different from the conventional wisdom of sociologists at the time the texts were written and at variance with what Merton said. That these arguments should even be mounted is remarkable: what other major intellectuals would be defended by the argument that in their most influential texts they did not mean what they said? Merton, in any event, never disowned these texts, except in a limited way in the 1975 article (“Structural analysis in sociology”) I discussed at length. Instead, he reiterated his endorsement of these ideas by producing three editions of STSS (1949, 1957, 1968) and a book, On Theoretical Sociology (1967), that was mostly devoted to these essays and he authorized more than thirteen translations of this material. If Merton didn’t mean what he said, he picked a strange way to show it.
My interpretation did not attempt to save Merton by ignoring all of the problematic scientistic statements he made. This would have shrunk Merton: by changing him from the thinker whose ambitious program for sociology captured the attention of so many in the postwar generation to yet another conventional sociologist who was dubious about Parsons. As the title of the paper suggested, the promise of Merton’s program was to get past the problems of past sociology, with its “many approaches, and few arrivals.” This phrase was a quotation from Merton himself, repeated by Zetterberg as an epigraph ([1954]1963, I). It was the core promise of the whole program. Stzompka grotesquely misreads the title as a condemnation of Merton’s own work, which, Stzompka thinks “clearly implies that there were ‘few arrivals’ in Merton’s work” (2009, 264). The point, of course, was that Merton’s idea of the promise of this new strategy was that it would overcome the situation of “many approaches and few arrivals” characteristic of previous sociology, which he thought he had surpassed.

The central irony of the paper was that Merton was himself reduced, in 1975, to defending his own work as an approach. Kincaid denies that Merton, in 1975, had changed his mind— that he was tolerant of many approaches all along, and simply disagreed with Parsons’ imposition of his own conceptual scheme (2009, 267). Stzompka, in contrast, defends his change of mind as a sign of wisdom. Crothers says that Merton “clearly did change over time . . . and came to more explicitly recognize that there was an irreducible range of sociological viewpoints” (2009, 278). The record amply shows that the Columbia people originally, that is to say in the period of the late nineteen-forties and fifties during which the texts were composed, thought they had something better to offer than tolerance for many approaches, and thought that Merton was explaining what this something was in his middle range theory article (1949). Kincaid implicitly
concedes this when he says that conflict between middle-range theories “is a much more local and manageable affair than that between grand conceptual schemes” (2009, 267-268). This is indeed what they believed. But it turned out to be a false hope—topical areas of the sort that were of concern to Merton, such as crime, are still taught in terms of “many approaches” and the conflicts proved to be no more manageable for being local. The failure of this hope is the larger story line of the paper.

Nevertheless, I argued, there was something important and positive to be salvaged from Merton, and it could only be understood in relation to Lazarsfeld and others in his circle. To be very, very, brief, my positive point about the Merton (and Lazarsfeld) approach was this. Causal modeling as pioneered by Herbert Simon (1952, 1953, 1954b) addressed the problem of spurious correlation by embedding potentially spurious correlations in larger structures of correlations (1954a). Simon devised a method for eliminating some correlations as spurious, based on certain apparently minimal assumptions about non-influence between variables together with assumptions about the non-influence of variables unincluded in the model. The Columbia School knew all about this, and pursued a rival approach, based on the same basic ideas about partialing, but were compelled to assume that when they partialed they were doing so in circumstances that were analogous to controlled experiments. Simon had to make some similar assumptions, but he was able to derive, in places where the Lazarsfeld approach had to assume. This provided the technical core of the Directed Acyclic Graphs (DAGs) discussed in the present philosophical literature. People in the Lazarsfeld shop tried to find a technical fix of their own for the problem of spuriousness, but failed. Merton’s discussion of theory, I argued, provided what can be regarded as a serious alternative response, though not a technical fix, to the problem of
spuriousness: if one has a set of quasi-generalizations that warrant one another, one has some modest grounds of thinking that the correlations that are associated with them are not spurious.

So there is something to the Columbia model, or so I claimed, but not what Merton seemingly intended, namely a general solution to the problem of many approaches that would lead sociology on the path to science. Much of what I showed, of course, was that the project as Merton outlined it was ambiguous and confused, and that Merton did little to resolve these ambiguities and confusions. The way things looked at the time was different: Lazarsfeld and his colleagues had an approach that was different from Simon’s, thought his was a rival to theirs, as indeed it was, and thought they were right, or at least a provided a genuine alternative. Merton had a defense of their general approach in these essays, but not one that was intentionally directed at the specific problem of spuriousness. The Lazarsfeld group tried to find their own technical solution, but failed. In retrospect, however, one can cut through all the ambiguities, confusions, and scientistic posturing, and see that getting a few generalized theoretical claims that both helped to warrant one another and could be associated with correlations supported the idea that the correlations were not spurious. This could not have been seen at the time, in part because the formalism (of “greater the x, greater the y” statements) was unworkable for this purpose and misled them into thinking they were on the road to deductive theory, in part because of the many other confusions I documented, and in part because this issue was mixed up with the question of the merits of contingency tables, which it had nothing to do with.

My positive interpretation was retrospective. But the literature to which I was comparing Merton in my discussion of ambiguities and confusions, including the literature on spuriousness, was not only contemporary, but actively (and in the case of Simon, obsessively) discussed in his
local intellectual world. Nagel, for example, was a colleague, who committed years of teaching effort to a course with Lazarsfeld on The Logic of Social Inquiry, and took Merton and sociology seriously. This seems to have eluded Sztompka, who suggests that Merton is being unfairly held to an arbitrary “reading list” developed in the present by intellectual Lilliputians in remote provinces, or to the standards common after some sort of unnamed twenty-first century revolution in statistics. In fact, the “reading list” which guided me was from the archives: an actual reading list from this course, which Merton himself sometimes participated in.1 Much could be said about this course, and perhaps I should have said more, because its contents belie many of the claims made in the critics’ comments. In the first place, the class was not some sort of rote application of Logical Positivism, nor did it reflect an obsession with laws, as Kincaid’s comments would suggest. Nagel, perhaps surprisingly, dealt mostly with Weber and issues in the philosophy of history. Only one standard positivist text, Hempel on general laws in history (1942), appears on the reading list. Simon’s papers on cause and causal modeling, however, play a prominent role in Lazarsfeld’s contribution to the course.

Sztompka and Crothers suggest that the local context I describe did not exist, that there was no such thing as the Columbia approach to theory construction or a Columbia school, and that, in any case, Merton had nothing to do with it. As a historical claim, this is peculiar. The members of this group self-identified as having a distinctive approach; there is a lot of intellectual and personal commonality. Merton was extensively involved with the training and

educational experience of those who went through this training, and his methodological writings, as I pointed out, were thought of by at least one key figure, Herbert Hyman, to be little more than a defense of the research approach of the Bureau of Applied Social Research (1991, 204). I pointed out that Durkheim’s *Suicide* was used as a methodological exemplar. I noted, though the citation in the article is in error, that James Coleman recalled Merton’s “. . . concern with the process of theory construction in sociology” (1990, 29; emphasis in the original), and his comment that “Merton’s dissection of Durkheim’s *Suicide* in his course on the logic of theory construction showed the inner workings of a master theorist-cum-researcher” (1990, 29). If someone is guilty of constructing an “imagined ‘Columbia Model’,” as Sztompka calls it (2009, 264), it would have to be Coleman and other Columbia products, such as James Price (n.d.), who imagined it and made their non-imaginary careers by adhering to it, or the Columbia faculty, such as Zetterberg, who codified it ([1954]1963, 1962), and the editors of the volumes *The language of social research: A reader in the methodology of social research* (Lazarsfeld and Rosenberg 1955), and *Continuities in the Language of Social Research* (Lazarsfeld et al 1972), who presented it as a coherent, encompassing, approach.

One of the issues discussed in these circles was functionalism, which has a similar and closely connected trajectory to Merton’s own career, ending in incoherence and abandonment. As I show, the issues were already there in the intellectual microclimate of Morningside Heights, and were the subject of a detailed critical discussion by Nagel. Kincaid defends Merton’s functional explanations against Nagel, whom he charges with being overinvested in the positivist notion of scientific law. That is indeed the form of Nagel’s published critique: to ask how a fully nomic version of this kind of explanation would look (Nagel 1956). But the problem with
Merton’s explanations that Nagel noted was about how one completes a functional explanation, and his complaint was that Merton seemed to have no interest in this problem, a problem that arises whether or not one thinks the completion requires a full complement of positivist laws. One way of completing the explanation is to identify feedback mechanisms, making it into a mechanical explanation. In the study of function in biology, the completion is often handed off to evolutionary processes which are themselves causal. How would Merton’s be completed? One answer would be to refer each function to a higher function, the functional character of society itself. But Merton rejected this Parsonsian answer. What was Merton’s answer?

It is not clear that he had any answer at all. Kincaid’s attempt to make sense of Merton’s explanation of political machines exemplifies the difficulty with making these explanations complete and keeping them ‘functional’. Kincaid answers the question of who machines are functional for as follows: “political machines persist (his term) because they satisfy the needs of constituents” (2009, 269). Crothers makes a similar point when he says that bossism is functional for the deprived classes. But these interpretations turn it into a non-functionalist explanation—specifically an interest explanation—that is more readily construed in individualist, rational choice terms: bossism persists because it is a good deal for both sides of the bargain. This is neither a structural nor a functional explanation. The language of function is entirely superfluous.

Making sense of Merton on this point is crucial to assessing the later claim that Merton had a distinctive approach to sociology and was a ‘structuralist’ all along. In a trivial sense that claim has to be true. If he was a structural functionalist, he was a structuralist: structures were the

things functionalism explained. But Merton was a very peculiar functionalist. Elster’s point about the ambiguity of Merton’s use of functional terms (1990, 135), which seem to promise functions but only provide consequences, is echoed by Agassi when he says that the idea of dysfunction “deprives the bold idea of functionalism of all informative content” (2009, 285). Indeed it does. What happened in the course of Merton’s career is that functionalism went out of fashion. Merton, and several of his students, abandoned the functionalist ship. Blau, for example, tried his hand at deductive theory— as an alternative to functionalism (Blau 1970). Critics, of whom I was one, pointed out that these deductive arguments did not work (Turner, 1977). Blau’s defenders, interestingly, argued that these criticisms were off the mark, because Blau really had a hidden functionalist argument, which did work (Donaldson 1985, 66-68). I think that there is at least something to this line of defense: when the Mertonians, and Merton himself, unhooked the functionalist explanatory engine from the structural train, the whole enterprise ground to a halt. Little was heard of this kind of ‘structuralism’ after Blau’s edited tribute volume to Merton, *Approaches to the Study of Social Structure* (1975).

A few minor issues. I did not use Merton’s sociology of science or his discussion of modern science to illuminate his methodological writings, as Agassi and Kincaid thought I should have. Neither did Merton. On the one hand, although he once briefly acknowledged that sociological considerations could explain aspects of the content of science, he himself never made any attempt to connect serendipity or surprising instances to a more general account of science, as Bacon, Whewell, Durkheim, and Polanyi did. Instead, he was careful to insist that social science was at a stage comparable to medieval physics, implying that there were no lessons to be learned from present science.
The original paper contains some errors, which need to be corrected. Coleman, as I have noted above, was miscited. Donald Black, as Crothers correctly observes, was not a Columbian. Mills did not name Merton in *The Sociological Imagination*, as Crothers says, nor did I say he was. But Merton does appear in Mills’ text in a very specific guise: as the “statesman” who carries on a busy traffic between the schools of theory and research ([1959]1961, 110-111). D-A-Gs, as I noted above, derive directly from Simon, not Lazarsfeld, so they don’t help the case for Lazarsfeld’s continued relevance, as Crothers suggests. Neither does the fact that Blau (Blau and Duncan 1967) and Coleman later wrote important quantitative analyses: the casual modeling methods they used in these studies derived from Simon, not Lazarsfeld.

Is there anything to be learned from this exchange? Perhaps two things. The Merton of the methodological essays, written over a relatively short period, is a different creature than the Merton who wrote on science. The other is that the personal reasons for Merton’s public style remain an enigma. I made no attempt to put Merton on the couch. But the temptation seems irresistible. Agassi admits that Merton was evasive, but excuses this as the exercise of the right of establishment figures to ignore the doings of those beneath them. Crothers guesses that Merton “faced some (unconscious) temptation to avoid choosing between stark alternatives but rather to leave issues cloaked in some ambiguity which would allow for maneuver” (2009, 275). Kincaid and Stzompka attribute motives as well. I don’t think these various characterizations match up very well, nor do they fit with much that is known about Merton Perhaps, as I said, the data of the archives will give a more coherent picture. But it is likely to be a very complex one.

References


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