Many Approaches, but Few Arrivals: Merton and the Columbia Model of Theory Construction

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Robert Merton's essays on theories of the middle range and his essays on functional explanation and the structural approach are among the most influential in the history of sociology. But their import is a puzzle. He explicitly allied himself with some of the most extreme scientistic formalists and contributed to and endorsed the Columbia model of theory construction. But Merton never responded to criticisms by Ernest Nagel of his arguments, or acknowledged the rivalry between Lazarsfeld and Herbert Simon, rarely cited the philosophical and methodological literature, and responded to critics with ambiguous concessions, leaving the Mertonian legacy profoundly ambiguous.

Key words: Robert Merton, Paul Lazarsfeld, Theory Construction, Middle Range Theory, Causal Modeling, Émile Durkheim

We may have many concepts but few confirmed theories; many points of view, but few theorems; many approaches, but few arrivals. Perhaps a shift in emphasis would be all to the good. (Merton quoted in Zetterberg [1954] 1963, I).

Robert Merton wrote a number of influential papers on the general methodology[1] of sociology, which deal with basic questions about the nature of explanation, the nature of theory, the relevance of empirical evidence to theory, and the prospects for future theoretical development. These papers are, for Merton, what the *Rules of Sociological Method* (1895) was for Émile Durkheim, or the essays in the *Wissenschaftslehre* (1922) were for Max Weber. Merton's status as a classic depends in part on the claim that, like those classics, he had a coherent and distinctive vision of sociological knowledge. Ferdinand Tönnies and Alexis de Tocqueville, in contrast, do not quite make the grade because they had no such vision, or at least did not articulate it in a classic text. Karl Mannheim does not because his attempted solution to the problem of perspectives is fatally flawed, and in the end collapses into a position originally worked out by Weber. The most destructive methodological critique of Mannheim, which argued exactly this point, was written by Merton himself (1968, 543-562). Parsons is on the cusp because his methodological vision, on display in the opening chapters of *The Structure of Social Action* (1937) and in his commentaries on Durkheim and Weber in the same text, now seems to have yielded nothing of substance. The Promised Land, once arrived at, looked so unlike what was promised that the promises of systematic social theory have themselves been discredited. Merton, in contrast, seems to have been a better prophet, whose writings had a better fit with what sociology could in fact achieve. He was also acutely conscious of the problem of arrivals, of getting results, as the epigraph of this article shows. The idea that sociology needed to go beyond "approaches" or perspectives to "arrivals" or empirical facts was at the core of his methodological vision. To the extent that sociology produced such knowledge, Merton's vision is vindicated. But understanding the vision is not, as we will see, an easy matter.

Merton’s papers, and especially the essay on theories of the middle range (1949, 39-72), also are of interest in the history of sociology apart from this somewhat absurd game of ranking sociologists as classics. “The Columbia Model of Theory Construction,” to which Merton had a close if ambiguous relation, provided, for a time, the most influential answer to the question of how theory related to empirical research in American sociology. Many sociologists in the middle of the twentieth century embraced the model and understood their own scholarship as contributing to sociology by developing middle range theory. As a kind of conventional wisdom, the essays influenced the form and the content of what was done in sociology for decades even apart from those who were part of the Columbia experience. The term “theories of the middle range” is still referred to by sociologists and even more so by applied social scientists. And the concept has articulate defenders (e.g., Pawson 2000, 2005) who argue that the concept is relevant to the present– at least to the present of applied social science. Methodological thinkers and theorists as diverse as Barney Glaser and Donald Black generated their own viewpoints out of their Columbia experiences, and the Columbia model provided a template for the scholarship many Columbia graduates (cf. Price 2003).

Understood as documents in the history of methodology, however, Merton’s papers are somewhat mysterious. They resist the kind of historical analysis that historians of methodology ordinarily perform. These include such standard approaches in the history of ideas as looking at reception history, identifying sources and relevant teachers, and locating the work in the larger development of thought in a given area, as well as some approaches particularly relevant to methodology: looking at the specific contemporary scientific content to which the work refers, to the author’s own scientific work, to the close scientific allies of the writer, and to the specifically philosophical reading and personal context of the thinker. These kinds of analyses can be performed quite successfully on Durkheim and Weber's methodological writings, as the extensive, not to say endless, literature on them shows. In what follows, I will show what the application of standard methods of analysis in the history of methodology and the asking of the usual questions yields in the case of these essays. They produce something—and sometimes quite interesting results. But they do not produce the kind of clarity that is characteristically produced by inquiries into methodological writings—and indeed, that the conventions of methodological writing, which is in the end a form of philosophical writing, force from their authors. Instead, asking the usual questions makes the texts, their intent, and their meaning more enigmatic.[2] The difficulties in getting a convincing reading out of Merton's writings in this area are well known. Jon Elster, after attempting to make sense of Merton's uses of the term 'function" in his writings on science, and trying to determine whether Merton means anything more than "consequences," threw up his hands:

> I have to report that after many readings and re-readings I still do not know. Perhaps Merton has been playing the oldest game of all, that of suggesting the strong claim without actually sticking his neck out and embracing it explicitly? (1990, 135)

We encounter this same problem throughout the texts, as we will see. But the ambiguities do have a pattern, and the core ambiguities in his methodological writings are identifiable.

Merton in Intellectual Context: The Notion of "Conceptual Schemes"

One kind of question that is ordinarily asked of methodological writings is contextual: Why is the text written at all? Whom is it written against? The usual answer is that the text is an attempt to contribute to some ongoing discussion, usually with identifiable discussion partners. Great texts change the terms of the discussion, and their greatness can be understood by contrasting them to the terms and conventions of the context in which they were written. Contextualism flourished as an approach to history because the context of texts retrospectively regarded as great had been forgotten, so that novelties were attributed to thinkers that were not novelties at all, but part of the conventions in terms of which, and against which, the texts were written (Jones 1977; Skinner 1969, 1970, 1972, 1974).

In the case of Merton, we seem to have a particularly straightforward answer to this contextual question. The target is Parsons. The texts are not overtly polemical, but the target is clearly the idea of analytical sociological theory understood as the construction of a conceptual scheme. The case against Parsons reflects Merton's experiences in empirical research with Paul Lazarsfeld, and for the most part uses examples from the BASR (Bureau of Applied Social Research) tradition. But aside from promoting the research products of the BASR tradition as useful and relevant to theory—something that even Parsons himself would not have denied—what does this case assert?

"The Bearing of Empirical Research upon the Development of Social Theory" (1948b) provides a typology of ways that empirical research alters our sociological understanding and supports revisions of our "theoretical" ideas other than either the top-down Parsons one or the hypothesis testing model. Research, Merton argued, had additional functions. The "serendipity pattern" involved the appearance of anomalous results that indicated that there was something wrong with the way the problem was formulated. Research also “reformulates,” by which Merton means that it exerts pressure for the elaboration of conceptual schemes. Research also “deflects,” by which Merton means that it redirects theoretical interest by producing new kinds of data through the use of new methods. And research “clarifies,” meaning that it forces theorists to explain themselves in ways that can be turned into researchable projects (Merton 1948b, 157-58).

The text exhibits a typical intellectual strategy of Merton’s. He assumed (or takes it that the reader accepts) a standard view of the subject, and did not reject it, but instead qualified it by introducing elements—in this case the four additional functions of empirical research—that appear to be contrary to, and different in kind from, the standard view. But the additions neither radicalize nor replace the standard view. Nor do they reject it. Merton was later to call some examples of this strategy “ambivalence.” A simple example is the fact that we depend on expert medical advice; the ironic qualification of this core fact is the secondary fact that we also may resent the advice. Hence we are ambivalent. Donald Levine has suggested that Merton was himself ambivalent about the idea of autonomous theory (2006). This strategy, or actual ambivalence, makes his methodological writings especially puzzling. Rather than revising the standard view to correct it in light of the qualifications, or to extend it to cover the qualifications, he leaves both the standard view and the qualifications intact.

The confusions to which this strategy give rise can be seen in connection with a term used by Parsons that Merton also uses in his methodological essays, and does not reject, but qualifies: “conceptual schemes.” In this essay, it appears in connection with the idea that “new data exert pressure for the elaboration of a conceptual scheme” (1948b, 162). The reasoning seems simple, and it is based on a set of cases: “fresh empirical facts . . . led Malinowski to incorporate new elements into a theory of magic” (1948b, 162). A BASR study of Kate Smith’s war bonds campaign showed that people didn’t like salesmanship, feigned personal concern, and so forth, but liked Kate Smith. Why? The interviews revealed that they believed Kate Smith to be sincere because her actions—which appeared to be self-sacrificing and to stress her voice—proved her sincerity and devotion to the task. In each case, the research suggested “new variables to be incorporated into a specific theory” (1948b, 165). This seems innocuous, on the surface. There is no discussion of the meaning of the term conceptual scheme. Moreover, this central issue is incidental to the announced motivation for the paper, as found in the opening passages, to claim that the hypothetico-deductive model tends to “distort” the actualities of research (1948b, 157). But the paper says little about this. His own additions merely correct what this model “exaggerates” and “minimizes” (1948b, 157).

Making sense of these passages is illustrative of the difficulties of making sense of these texts. As Merton knew well, the term “conceptual scheme” was a commonplace of Harvard discussions of science in the period in which Merton was a student, especially by L. J. Henderson, who had a large role in the formation of Harvard sociology, and Alfred North Whitehead. It continued to be used extensively during the period of these papers, especially by Harvard’s influential President, James Bryant Conant, in his widely read books, *On Understanding Science* (1947), *Science and Common Sense* (1951), in his later lectures at Columbia: “Science and Technology in the Last Decade,” “The Changing Scientific Scene, 1900-1950,” “Science and Human Conduct,” and “Science and Spiritual Values” (collected in *Modern Science and Modern Man* 1952), and in his casebooks on conceptual change in science

(1957), which Merton himself taught from, and Thomas Kuhn both contributed to and taught from. Kuhn was Conant's protégé, and these texts contain virtually everything that is found in *The Structure of Scientific Revolutions* (1962) other than the term "paradigm" itself, which replaced conceptual schemes. As it was originally employed by Henderson and articulated by Whitehead, the term referred to the constitutive assumptions and concepts that were presupposed by the scientific theories of an era, which changed at a pace that was much slower than mere theories and were far removed from empirical data. Henderson thought of the conceptual schemes of medicine, for example, as having held on for centuries. Revisions of conceptual schemes were revolutionary, but revolutions were rare. The roots of this conception were neo-Kantian, as Merton knew very well from his reading of the historians of science Alexandre Koyré and Pierre Duhem, both of whom he quoted.

The relation between conceptual schemes and data could be thought of in two ways: Conant thought of this in terms of a continuum, on which the empirical content gradually diminished as one rose from empirical generalizations and hypotheses to theories and then to conceptual schemes. But a more radical interpretation was that the data themselves were constituted by these non-empirical assumptions, and thus could not undermine them directly. This was the option Kuhn ultimately used to radicalize Conant and make the problematic of radical conceptual change famous. But it is also present in the paraphrase from Duhem that Merton gives: that "the instruments as well as the experimental results obtained in science are shot through with specific assumptions and theories of a substantive order" (1945, 463n2).

Parsons drew a different lesson from Henderson in the 1930s: that to be a science required that a discipline *have* a conceptual scheme. Without it, theories and empirical generalizations were impossible to bring together into a larger whole, and progress was impossible. So he set himself the heroic task of constructing and identifying in its past thinkers the conceptual scheme of sociology and understanding its place in relation to other conceptual schemes, notably that of economics. In the context of the time, this made sense. German sociologists, influenced directly by the neo-Kantian model of science that understood a science to be a hierarchically arranged scheme of concepts that logically integrated a domain of fact, manufactured such schemes endlessly: Georg Simmel had done so, as did Leopold von Wiese, Alfred Vierkandt, and so on. Parsons thought that a univocal common scheme that could be constructed or derived from an analysis of the history of the field was an intelligible response to this proliferation of schemes, however over optimistic it ultimately turned out to be.

So what was Merton's response to all this? He expresses "strong agreement with Parsons' view that the day of rival schools of sociological theory, each purveying its system of doctrine in the marketplace of sociological opinion or engaging in open academic warfare with its enemies, has come to a well-deserved close" (1948a, 164). So it would seem that he agrees with Parsons on the need for a single conceptual scheme. But he then says that "a shift in emphasis would be all to the good"– away from concentrating solely on schemes and toward "special theories," provided that "there is a *pervasive* concern with consolidating the special theories into a more general set of mutually consistent propositions" (1948a, 166). Merton characterizes this as a "policy" for the allocation of the scarce resources of "the men who practice sociological theory" that would enable the replacement of "many approaches" with a "few conclusions" and "many points of view" with a "few theorems" (1948a, 166). This is the core idea of middle range theory he later articulates. But how does it relate to the problem of conceptual schemes?

An answer of sorts is to be found in “The Bearing of Empirical Research on Sociological Theory.” As we have seen, the term “conceptual scheme” figured in his discussion of Malinowski’s theory of magic (1948b, 162) and the term “general orientations” in his discussion of the Kate Smith war bond drive (1948b, 163), but the terms, as he used them, relate only very loosely to these established usages. Neither Malinowski’s theory of magic nor the vague ideas about mass persuasion that motivated the BASR study of the war bond drive are conceptual schemes in the grand sense the Harvard tradition had in mind. Moreover, the supposed special role of the data in forcing a change in conceptual schemes does not seem very special. As Merton recounts it, Malinowski noticed that the Trobrianders did not use magic within the lagoon, where their fishing methods were reliable, but only for those situations where the methods were not reliable. He then generalized this clue into a theory about the relation of magic to “the fortuitous and uncontrollable” (1948b, 163). The argument supports, in a vague way, skepticism about the project of constructing conceptual schemes, inasmuch as it hints that there is always something that a preconstructed conceptual scheme will fail to anticipate. But, speaking at all strictly, these examples are irrelevant to conceptual schemes understood in the core sense, or in Parsons’ sense.

There are many more examples of this odd use of language. The terms formalization and derivation, as we will see, lose their original meanings as well. This makes identifying Merton’s views difficult. What is his view of the Harvard idea of conceptual schemes? Does he reject it? One would think that he does, despite his quotation from Duhem, if he thinks that the role of conceptual schemes somehow disappears when we construct “special theories.” But does he reject Parsons’ idea that each science needs a developed conceptual scheme? Is the argument merely a “policy” claim about how to make sociology advance as a science? If so, how exactly is the policy supposed to work? Is it really the case that concentrating on special theories would produce conclusions and theorems that were free from the problem of multiple approaches? This is important to what follows, because it is precisely this argument that he abandoned in 1975. Similarly, is his argument that the standard hypothesis testing model of social research misrepresents what actually happens in research a methodological disagreement with this model, or merely helpful practical commentary? Does he have, as he sometimes implied, as we will see in connection with his allusions to abduction, a different model? More generally, was Merton using these essays to provide a road map for the development of sociology– a strategy– or was he describing a model of what sort of knowledge was *possible* for sociology, that is to say a distinctive methodological viewpoint to be installed in place of the received views?

Abduction and Codification

Contextualists also ask what is novel in a formulation or theory, and making sense of the critique of Parsons would be made much easier if there were something that Merton claimed to do or endorse that was different, on a methodological rather than policy level, than what Parsons endorsed. And here we seem to get some results: a body of theoretical practices, which are associated with the Columbia approach and endorsed by Merton, that add to the standard view of hypothesis testing and generation. Whether he meant to treat it as a distinctive method or not, Merton had a term for it: codification. The activity of codification, moreover, plays a large role in the Columbia model, and in the thinking of those who were influenced by the Columbia model. In the practice of Lazarsfeld and his co-workers, the term “transfer” is used to describe the practice, which they thought of as distinctive to their methodological approach, of systematically searching for ways to extend and apply explanations that were developed in a context to new settings.[3]

The standard view, enshrined in the distinction between the context of discovery and the context of justification, is that there is no method for inventing explanatory hypotheses, but that doing so is the essence of scientific discovery itself. The idea of generalizing from narrow scope empirical results to a more general theoretical claim falls outside the standard model of hypothesis testing. If there were a "method" here, it would be a genuinely distinctive addition to standard textbook methodology. Merton, who credited Lazarsfeld for the idea, combines it with the idea of transfer, as when he says that "codification seeks to systematize available empirical generalizations in *apparently different* spheres of behavior" (1949, 155). In this case, Merton, unusually, also identified a philosophical source, or at least a precursor, for the idea, suggesting that it involved elements of abduction, a concept of Charles S. Pierce. Merton understood abduction as "the initiation and entertaining of a hypothesis as a step in inference," rather than merely collecting new instances, and notes that Pierce "had long before noticed the strategic role of the surprising fact" (1948b, 158n5).

Abduction is itself a murky notion. Today it is often understood as a form of "inference to the best explanation," in line with Pierce's own idea that abduction involved creating new rules to explain observations. But what Merton meant is less clear than even Pierce's usages. The ambiguity becomes apparent when we consider the more explicit account given by one of the witnesses and participants in Columbia theory construction, Hans Zetterberg. Like Merton, Zetterberg gives an example rather than a theoretical account. He begins with some findings:

college students at Bennington who were elected as worthy representatives of the school agreed more with the liberal values of the college community than others. Through a series of steps of, to put it impolitely, jacking up the findings to higher levels of abstraction, it became “the more favorable evaluations rank and file members receive in a group, the more their ideas converge with those of other group members” (1954, 24). This example fits better with Pierce, and captures the notion that abduction, unlike deduction or induction, adds novel conceptual elements. But aside from the addition of new conceptual elements, all Zetterberg is describing is the familiar pattern of subsuming less broad generalizations under more broad generalizations, that is to say deductive explanation itself. The addition of new, more general, conceptual elements is the task of theory. But there is no procedure for doing this– it requires the creative activity of the theorist. Yet Merton seems to think that he has, in discussing such things as the serendipity pattern, added something to the standard picture. The question is “what?” Are these merely observations of a sociological kind about what actually happens in social research, in contrast to textbook images of “science?” Or do they change our understanding of the nature of sociological knowledge?

One answer to this question would come from noticing that the same kind of elevation of explanations of observational findings to a higher level of abstraction is recognizably the core idea of Barney Glaser’s “grounded theory,” which adds to it the confused idea that concepts “emerge from the data.”[4] This otherwise unintelligible phrase would make sense if “the data” were the naturally given and already conceptualized social world into which the social research enters. And this interpretation fits nicely with Merton’s examples, which describe the experience of the researcher striving to construct and test statistical or ethnographic hypotheses on social data. The researcher often finds that the hypotheses don't work, and finds out why, not by merely inventing new hypotheses and revising the theory to fit the negative results, but by going back to the social world itself for the reasons. This way of framing the issue suggests that the "data" are not, as in the hypothesis testing model, merely constituted for us, but talk back in sometimes surprising ways. If we merely test our hypotheses as stated, we miss out on this fact-- that the subjects of our research are subjected to further interviews in which they sometimes can give us reasons for their behavior that were not part of our original hypotheses. Merton describes just such a case under the heading of the serendipity pattern; he gives the example of the parents who fallaciously believed that there were more adolescents available for baby-sitting in their new suburban home than had been available in the city, an error resulting from the fact that they knew intimately more people in the suburbs and that therefore more adolescents existed socially for them (1948b, 159).

But this just adds to the confusion over Merton's point. One reaction to this talking-back feature of social research would be this: acknowledging that there is a genuine, accessible, inter-subjective world of reasoning and interpreting agents to go back to amounts to acknowledging the ontological and theoretical primacy of this world and of these intersubjective realities in sociological explanation. Any methodological construction of this world into a set of questions with scalable answers on a survey researcher's interview schedule is an imposition on the underlying intersubjective reality, necessarily a distortion, and in all likelihood a travesty of it. But Merton draws no such conclusion, nor does he even seem to grant that there is a problem of this sort. Nevertheless, the whole account of the bearing of empirical research on theory points to such a conclusion. These issues were well known in the history of sociological methodology, and not merely a concern of the cross-town phenomenologist Alfred Schutz, whose existence Merton barely acknowledged.[5] They are central to Weber's admonitions in "'Objectivity' in Social Science and Social Policy"(1904) that such things as medieval world views were ideal-types that had an inherently problematic relation to the underlying reality of a middle ages full of individuals with different views of the world.

Merton could have availed himself of Lazarsfeld's dismissal of ethnomethodological critics of survey research, which held that the issues that they raised were well known to survey researchers and they dealt with them successfully on the basis of that knowledge. But given the actualities of such things as questionnaire construction, this response amounted to saying "we know that our results are approximations and misrepresentations of the underlying reality– but we don't really care about the underlying reality." This is fine for an applied researcher, who can sell usable approximations. But it is odd, to say the least, for a scientist, much less one who has, like Merton, made a point about the fact that questionnaires, as in the Kate Smith survey, did not dig deeply enough. Similarly, Merton, could, quite justifiably, have argued that obsessing with this ultimate reality was itself a dead end, and that the way forward was through proposing and refining theories that were, necessarily, approximations. And one can take his continued commitment to the strategy of middle range theory as a practical endorsement of this claim. But then we are faced with another ambiguity. What is the status of these approximations? Are they stepping stones to less approximate theories, and if so, how? Or are they the best that we can expect out of sociology? Or are approximations of the sort represented by BASR research all we can hope for?

Among the Classics

The mystery deepens and clarifies if we approach these texts in another standard way: by comparing them to the positions and arguments of the actual classic figures in the history of sociology. Here again we have a mystery about what Merton knew or read. It is evident that Merton's knowledge of the writings of Weber and Durkheim, and especially of Georg Simmel, whose ideas he employed extensively, was exceptional. Moreover, his knowledge of the prehistory of the ideas that he used was extraordinarily extensive. His early paper on unintended consequences exhibits this mastery (1936), and he expressed the desire, never fulfilled, to write a full history of this idea. But there is, even in this case, a striking neglect of both the philosophical literatures on these topics and the philosophical discussions in the classics themselves.

What he doesn't say is nevertheless revealing. Weber, in his methodological writings, is the pre-eminent theorist of the problem of viewpoints. For Weber, the very content of the social sciences is constituted in the first instance not by the sciences themselves, but by the culture, a culture that changes thus changing the content of the social sciences. Commitment to a cultural viewpoint is a precondition of social science. There is no autonomous scientific standpoint—a social science modeled on astronomy would be meaningless to us—the questions we want social science to answer originate in our culture, and the special social sciences reflect subsets of these concerns. For Weber, in short, "many approaches" is the condition of social science—they have eternal youth, and can come to conclusions only within the limits of the pregiven diversity of culturally given and subsetted starting points. Even Simmel, a more traditional neo-Kantian who held to the idea of neo-Kantian philosophy of science that to be a science one needed a uniquely valid hierarchical order of concepts which integrated the entire domain—an idea that reappears of course in Parsons—recognized that in the social sciences this was no longer possible, since viewpoints motivated by class interests and other political differences already had produced a diversity of approaches that no attempt at synthesis could overcome.

Merton seems to echo these concerns, with his many approaches slogan. And he shows his awareness of the issue of perspectives as it appears in Weber in his discussion of Mannheim (1968, 556-62), where he argued that Mannheim reverted to a Weberian view of the subject in his later writings (1968, 560n25). But he never addressed either Simmel or Weber on these subjects. Simmel's key methodological essay (1917, 3-25) is never cited. And Weber's methodological writings are directly cited only in the 1930s, in the context of issues about science. So we not only do not have Merton's reflections on these essays, we have no record of his response. German sociological system building—the irreducible diversity of frameworks provided by von Wiese, Vierkandt, and so forth—must have seemed to him to be a fate to be avoided. But he did not ask, as Simmel did, why it occurred, or address Weber's elaborate answer to this question.

Dialogue with the classics with respect to methodological questions, in short, was not Merton's style. Perhaps the excitement of the 1940s, especially the idea that sociology had made a new start, so that Whitehead's slogan "a science which hesitates to forget its founders is lost," had application to these problems. But there is an exception of sorts to the lack of dialogue with the classics on these questions. Merton was, like the Columbia school generally, enamored of Durkheim's *Suicide* (1930). James Coleman recalled the intensity and instructiveness of Merton's detailed interrogation of this text in his graduate seminar as one of the high points of his education in sociology (Merton et al 1979). And doubtless Merton thought, as Durkheim himself did, that the problem of diversity of approaches could be resolved by concentration on secure empirical results— “conclusions” juxtaposed to the “approaches” in the Merton quote as epigraph of this chapter. But here again there is a puzzle. Durkheim’s own account of what he was doing, his own methodological text, *The Rules of Sociological Method* (1895), was never a major concern for the Columbia school. And it is far from clear what they thought they had substituted for it.

The treatment of Durkheim provides a fascinating subplot of its own. The text was scrutinized at length at Columbia, discussed not only by theorists, but also, in a sophisticated way, by the survey statisticians in the department. Herbert Hyman, who started his career with Rensis Likert and Samuel A. Stouffer, but became a key figure at Columbia in the 1950s, devoted two chapters in his *Survey Design and Analysis* (1955) to the problem of interpreting the analysis of *Suicide*. Hyman understood the key peculiarity of the book: that Durkheim eliminates hypotheses—climate, latitude, and so forth—that have good correlations with suicide, in favor of a different “social” set of hypotheses that appear initially to be grounded in exactly the same kind of evidence. If we use the standards common in sociology at that time and the present, we would conclude that Durkheim is simply inconsistent. His hypotheses are no more confirmed than the ones he rejects. Hyman notes the problem, but tries to avoid the implication:

> The student may have noted one strange feature in the design followed by Durkheim. After completing all these *indirect* tests whose sole purpose was the negation of all possible explanations other than social causes, he then proceeded to make direct tests of

> the influence of social factors. One ponders this fact. (Hyman 1955, 230-1, italics in the original)

The terms “indirect” and “direct” allow him to make Durkheim consistent, but only partially. The “indirect” test is claimed to be a normal feature of the logic of science, and Hyman cites both Mill as explicated in Cohen and Nagel’s standard textbook (1934)[6] and Hans Zetterberg’s *Theory and Verification in Sociology* of 1954. The method, Hyman admits, is essentially unknown in survey research. The only example he could cite was a Scottish migration study, which eliminated factors as possible causes, by showing that there were no consistent differences in effects within Scotland, leading to the conclusion that the remaining effects were the result of the “a global social movement related to Scottish rural life as such” (1955, 232). Durkheim, presumably, used the indirect method to eliminate the non-social causes, forcing us to accept the sole alternative, namely his list of social causes.

This explanation is itself strange, for two reasons. One is that it is asymmetric: by Hyman’s logic, if Durkheim had started with the social causes, he could have just as easily eliminated them first, making one of the other explanations the remaining one that needed to be accepted on the basis of the direct evidence, which, if the statements are understood as statistical correlations, was every bit as good as the evidence for the claims Durkheim favored. The second reason is that Durkheim specifically disclaims the application of the method of residues and all the other methods involving indirect proof, on the grounds that the complexity of the social world makes them impossible to use, and embraces another as appropriate for sociology: the method of concomitant variations.[7] But his use of this method makes no sense if it is understood to apply to mere statistical correlations. It only makes sense if his argument is as follows: the non-social variables correlate, but imperfectly, with the suicide rate, and are therefore not genuinely causal; certain social causes, however, correlate perfectly, and therefore are the genuine causes. The correct answer is that Durkheim's standard is different from the standard common in sociology. For him the issue is this: his correlations are perfect (or nearly perfect, with explanations of the imperfections) laws. Every step up in the magnitude doing the explaining produces a step up in the suicide rate. There are *no* exceptions, at least no falsifying ones (though one may object that his treatment of the actual exceptions is somewhat ad hoc). In short, for Durkheim, unlike for Hyman, these are real laws of science: true generalizations.

The fact that Columbia sociology made a hash of their iconic classical study is revealing, especially of their methods of working. It would be understandable that Hyman never bothered to read *The Rules of Sociological Method* (1895) where Durkheim explains his differences with Mill. But Hannan Selvin, the Columbia Ph.D. who later published a series of similarly erroneous interpretations (1958, 1965, 1975, 1985), seems to have missed these key passages as well. Merton, it seems, did not engage with the *Rules* on its own terms either, or he would have noticed that the explanation being taught to Columbia students was in error. But this insouciance about the actual methodological texts—Durkheim, Weber, and Simmel alike, not to mention Mill, is itself important to understanding how they proceeded. Columbia sociology was willing to make do with their own, homemade version of the answers to these classical methodological problems, or as they must have seen it, the correct, up-to-date, and relevant to contemporary sociology versions, and saw no need to grapple with the thorny problems of the tradition itself.

Zetterberg: Spokesperson for the Columbia Model?

So what was the homemade version? And what made it historically distinctive? These also turn out to be difficult questions to get a clear answer to. Hyman's reference to Hans Zetterberg points to another standard approach to the history of methodology: look at the explications and applications of the method provided by the students and followers of the major figures. And this approach in turn points to still another standard method for understanding the history of methodology: look at the formal structures used by the methodologist. The problem of formal structure may seem arcane, but it is at the heart of the issue. Merton called for systematic sociological theory. "Systematic" would, at first blush, seem to imply the existence of logical relations of some kind between the elements of the theory. He even used the term "formalization," though as we will see, this usage was part of a set of usages that were nonstandard and ambiguous. For Parsons, the logical relations were those of the formal structure of exhaustive classifications, the logic was the logic of typologies, and the constructions were understood as transitional. Parsons held out the hope of a future in which sociologists would state their theories in terms of differential equations. Merton rejected the typological or conceptual scheme-building approach, though his rejection, as we have seen, was framed in such a way that it is unclear whether there is any sort of methodological principle involved. This is a common occurrence in Merton's methodological writings. Here and elsewhere Merton tends to appeal to the actual state of empirical research and to pronounce the methodological strategies he is critical of as irrelevant to them, but without explaining what the larger point of the kind of research he favored was, or where it was leading. This was a sore point with his critics, notably C. Wright

Mills, who made it the point in *The Sociological Imagination* (1959), and thus cemented it in the minds of the 1960's generation.

The actual state of research of the sort approved of by Columbia sociology during this period is probably best represented by the notorious inventory of empirical findings produced by Bernard Berelson (a former librarian and major participant in research at the Bureau of Applied Social Research at Columbia) and Gary Steiner, published as *Human Behavior: An Inventory of Scientific Findings* (1964). This book was a collection of statistical findings in behavioral science that were more or less robust. It represented, to many people, the failed promise of the revolution of the postwar redirection of the social disciplines under the label "behavioral science." Merton himself had, of course, been one of the proponents of this revolution, and defended it in an important report to the then newly established Ford Foundation (Foundation Report 1949), which subsequently invested heavily in it. From the point of view of the disappointed and the critics, the revolution amounted to nothing more than a bunch of unrelated and theoretically insignificant studies. What were missing were any logical or systematic connections between the contents of the inventory—and none were on the horizon.

The idea of middle range theory may be understood as a response to this situation as it was developing in the early postwar period, and as a defense of the value of this research, at least in terms of its future utility to a certain kind of theory. But to understand it as a response requires an answer to the question of what sort of systematization or logical relation between the findings it promises. If we consider the problem to be one of logical form, the Columbia model did have an answer to the question of what form a theory should take: statements of the form "the greater the x, the greater the y." This is the form that Merton used in one instance to explicate Durkheim.

But it is the only place he used this form. His students used it more frequently. Lewis Coser, for example, in *The Functions of Social Conflict* (1956) begins chapters with a translation from Simmel, comments on it, and closes with a summary which "formalizes" it in the language used at Columbia. Many of these summaries have a resemblance to "greater the x, greater the y" statements such as this one:

> Greater participation in the group and greater personality involvement of the members provide greater opportunity to engage in intense conflicting behavior and hence more violent reactions against disloyalty (Coser 1956, 72).

The statement illustrates a problem with the Columbia argot. Although statements in this form may be useful for deriving empirical tests—one need only provide operational definitions for the terms, collect data, and run a cross-tabulation or correlation to see whether there is such a relationship—they are not useful for the purpose of systematization, if this is supposed to involve logical relations between theoretical claims.

The standard meaning of "formalization," as distinct from the Merton-Lazarsfeld usage, is this: a logical relationship in a formalized theory is one that holds solely by virtue of the form of the statements in the theory, rather than by some feature of their meaning. Coser's statements are not in a form that permits us to draw inferences about other statements—about their consistency, for example—on the basis of form. Although some resemble "greater the x, greater the y," statements, others do not, such as "Conflict serves to establish and maintain the identity and boundaries of a group" (1956, 38). This is a simple case, but it is unclear how it could be formalized into a standard notation. Other statements are more complex:

> . . . antagonism is usually involved as an element in intimate relationships. Converging and diverging motivations may be so commingled in the actual relationship that they may be separated only for classificatory or analytical purposes, while the relationship has a unitary character *sui generis*. (1956, 64)

We need to have some complex intuitive feel for the subtleties of the language to make a reasonable guess as to how such statements relate to one another.

Yet there is a ready, and reasonable, Mertonian response to such concerns. Merton noted that there were many discussions of method that “set forth the logical prerequisites of scientific theory, but, it would seem, they have often done so on such a high level of abstraction that the prospect of translating these precepts into current sociological research becomes utopian” (1949, 140). However, he said, “approximations to these criteria are not entirely wanting” (1949, 150), and while he warned that “pressures—toward precision and logical coherence—can lead to unproductive activity,” he concluded that “the warrant for such criteria is not vitiated” by their abuse (1949, 153). He also argued that applying the criteria appropriate to contemporary or nineteenth century natural science to social science was a mistake, not because the criteria were “wrong,” but because the present social sciences were at a stage of development similar to that of early modern science.

But Merton’s use of this form raises a red flag, warning of potential confusion. As we have seen, “the greater the x, the greater the y” meant something different to Durkheim, who saw it as deterministic, than it did to Hyman, who read it as indicating statistical association. What did it mean to Merton, or among Columbia sociologists generally? Did they sometimes mean "x is correlated with y" and sometimes something stronger? Or is there no coherent answer to the question of what the statements mean? One finds no answer to this question in Merton. But there is an answer, to be found in Hans Zetterberg's *Theory and Verification in Sociology,* a text which Merton himself cites with approval, along with Berger, Zeldich and Anderson (1968, 60-61). Zetterberg's text was one of the most read and reviled texts of its time. It went through several editions after its original publication in 1954. It is cited by Columbia sociologists, and Zetterberg was employed at Columbia for virtually all of his American career, as Instructor 1953-54 in the School of General Studies, Assistant Professor 1954-57 in the Graduate School, and Associate Professor 1957-64 in the Graduate School.[8] The relation between this text and Merton's own views is obscure—as is the relation between Merton's views and the Columbia theory construction model generally. But examining the text at least shows where the issues lie.

Zetterberg's book is not an application of Logical Positivism to sociology. Though he makes one reference to the philosophical literature of the time, he distances himself from it. It is, rather, an attempt to epitomize the Columbia model on its own terms. Zetterberg tells his own story as one who learned the rigorous methods of sociology inspired by George Lundberg and F. Stuart Chapin, but came to regard most sociological research of this type as wanting in significance. At Columbia he discovered what he characterized as Merton's side of an argument with Lazarsfeld over the way to advance social science, which Zetterberg interpreted as an argument for "theory." In short, the book is the work of a convert. The book begins with the same quotation from Merton which I have used as the epigraph for this chapter. The problem of the status and the meaning of theories of the middle range define Zetterberg's enterprise. Middle range theories are partial. But "on the horizon of sociological thought looms the problem of integrating these theories into a more inclusive whole" (1954, 2), that is to say the problem Merton called consolidation.

We may begin with Zetterberg's answer to the question of the meaning of "the greater the x, the greater the y" statements. Zetterberg concedes that sociology has few, if any deterministic, invariant explanations. So he translated results that are correlational or involve categorical analysis with increasing magnitudes into statements about increasing probabilities, thus making the y's into probabilities. The example he gives comes from the *American Soldier* (1949). Privates who fell into the group "low conformist" in relation to Army norms had a lower percentage of promotions than those in the next higher group with respect to conformity, and this low conformist group had a lower percentage than the highest group (1954, 37-8). In this case, the method is directly from Durkheim: the relation is perfect, but it involves only three categories– not a very stringent test of the proposition that the probability of promotion increases with the degree of conformity, but at least a relevant one. If the proposition was meant seriously as a stochastic proposition, the fact of the probability of promotion would be strictly predicted by the (measured, but with error) fact of conformity, and there would be a quantitative law relating the two. And in this case, it would be formalizable in a way that allowed it to be related to other theoretical propositions so that something could be derived from them together. Unfortunately it would also be false: individual conformity is not an exact predictor of the individual probability of promotion. At best, it is a predictor of the probability that the members of a class of privates who got the same score on the conformity test would be promoted. That is something, to be sure, namely a correlation. Durkheim, in contrast, had a reason to call his results a law: he thought he was measuring properties of collectivities when he examined suicide rates. So his regularities were genuine, and true-- though there was some trickery in making this come out correctly.

Zetterberg's solution to the problem falls between Durkheim and Hyman. He treated "greater the x, greater the y" statements as statements about increasing probabilities, and then reasoned that such statements were weakly confirmable by ordinary statistical tests of the null hypothesis.[9] This allowed him to treat the conformity-promotion link as a successfully confirmed proposition, despite the fact that all the evidence he provided is the result of a Chi-square test of the null hypothesis. He then added an important step: he argued that sets of similar propositions that can be logically related to one another are more highly confirmed as a whole as a result of adding the propositions together in a unit. His reasoning is that because a proposition which is deduced carries the same probability as the proposition from which it is deduced, every time an implication is confirmed, the result is akin to the result of replication. So confirming ten related propositions is as good as confirming the first proposition ten times (1954, 75). This

> is the explanation of why it is comparatively hard to confirm a proposition but comparatively easy to confirm a theory. We can readily give modest empirical support to any of our propositions, but this is usually not enough to have much confidence in any one, if taken by itself. But a theory can coordinate these modest supports into high support for its postulates. (1954, 76)

The problem with this account is that the propositions actually aren't true—in the literal sense necessary for them to be related deductively. The test for the literal truth of a proposition to the effect that each increase in conformity produces a higher level of probability of promotion is much more stringent than beating the null hypothesis on a Chi-square. If the proposition were true, it would be a universal generalization about this relationship, with no exceptions for any increase in conformity. Consequently, the "modest support" that they get from Chi-squares is not modest so much as irrelevant. The strategy of testing a null hypothesis about the relation of x and y and drawing the conclusion that a theoretical statement in universal form about the relation between x and y would work only if there is a logical relation of disjunction between the hypothesis of no difference, that when x rises, y never rises, and the universal claim that when x rises, y always rises, i.e., that there are no other possibilities but these two. But of course this is not the case. Indeed, the whole procedure of testing null hypotheses and drawing "theoretical" conclusions from them that characterized this era of "behavioral science" represents, as Kurt Danziger put it a "travesty of the scientific method " (1990, 154).

But there is way out of this problem other than disregarding basic logic: we can claim that these very weak kinds of confirmation are highly preliminary, and merely point to the kinds of laws that might one day be refined out of this very crude material. The picture is a compelling one. It suggests that sociology can advance by theoretically integrating very modest findings of the Berelson and Steiner type, thus producing theories that are better confirmed than the propositions that make them up. We might argue that the deficiencies of the theories are the result of their crudity, and that down the road they can be made into something less crude and closer to literal truth, and gradually consolidated with other theories. This was Merton's strategy. But there is a question as to whether he actually believed this strategy could work.

Part of the problem of determining what Merton actually believed can be seen by considering his puzzling relation to the whole business of formalism and formalization. It seems as though he remained personally aloof from it, while praising those who applied his ideas about middle range theory by constructing formalizations. In the preface to the Polish edition of *Social Theory and Social Structure* (1982) he praised the Polish sociologist Andrzej Malewski, a philosophically trained formal logician, whose main works were attempts to provide axiomatic individualist versions of Marxism. And this praise was not an aberration or a courtesy citation for his Polish audience. In his 1975 "Structural Analysis in Sociology" essay, he again cites Malewski, by then long dead (1975, 33). But in this same paper, he speaks of the chastening experience of the social sciences (excepting economics and psychology) with the ideal of "tight-knit theoretical systems" (1975, 42).[10] Another bit of evidence comes from Merton's remarks on what we might call the end-game problem. An advocate of systematic theory can move the day of reckoning off into the distant future, but it is not an option to be vague forever about what systematization means. The ideal of axiomatization is the usual option, and it is clearly present, if not prominent, in Merton, and it is never rejected outright. There is no other reason to praise Malewski, and Merton didn't repudiate Peter Blau's efforts in this direction, or disown any of the more exuberant followers of the Columbia model in the theory construction tradition– including Zetterberg himself. Moreover, if there is an alternative to some sort of axiomatic systematization as the end game of systematic social theory in Merton, it is never articulated.

Yet there is an interpretation that doesn't require an answer to the end-game question and still makes sense of the idea of systematic middle range theory as something with added cognitive value over mere empirical generalizations, apart from the possible value of such theories as a preliminary to a brighter future. A simpler approach would be to forget about formalization or deductive theory, and treat the “greater the x, greater the y” statements in a way that fits with the kinds of facts that these sociologists were dealing with, that is, treat them as simple statements about correlations or statistical association, or tables where categories have some degree of predictive power. These relationships do not strongly warrant anything about other relationships between variables, and do not allow for “deduction” except in the case where the correlations are very high. This was the point of an influential 1964 article by Costner and Leik, “Deductions from ‘Axiomatic Theory’,” which showed graphically how high the correlations between an A and B and a B and C needed to be in order to guarantee even the sign of the relation between A and C.

Although the fact of statistical association has little deductive significance, it does warrant something. Sets of such relations, conceptually related to one another, do have greater epistemic weight than single tables or correlations taken alone: in this sense Zetterberg was right. Moreover, the presence of a correlation between A and B and B and C does increase the probability of one between A and C, even if there is only a narrow band of joint correlations that guarantees it. If the situation of sociology is a matter of dealing with weak relationships--especially, as is normally the case, weak relationships which are confounded with many other weak relationships—constructing theories which use these weak warrants to link the parts is better as a way of showing that the relationship has the kind of causal reality being attributed to it than merely inventorying weak relationships in the manner of Berelson and Steiner. There is some degree of support, even if it is a modest one, that the related empirical claims give each other in addition to the weak empirical support of the correlational evidence for the individual claim. Zetterberg understood why this support was needed: confounding meant that there were always alternative hypotheses that were difficult to exclude. This was an important issue for other reasons that do not surface in Merton's writings, but were central to Lazarsfeld, and indeed to the whole Columbia project, and these reasons give the idea of middle range theory an important point. It provides an alternative approach to the problem of confounding. Relationships that can be understood in terms of a "theory" which has some weakly confirmed propositions that weakly warrant the proposition in question are better supported than those which are supported only by the "fact" of potential causal influence (which is the case with confounders). The relevance of this becomes clear only when we put it in the context of a major methodological dispute to which Merton was only an incidental party, but which involved Lazarsfeld.

The Lazarsfeld-Simon Problem

The great methodological writings in social science, including Mill's *System of Logic* Book VI (1843), William Jevons' *Principles of Science* (1874), Weber's *Wissenschaftslehre* (1922), and Durkheim's *Reglès* (1895), were written or revised at the same time as these writers were producing major works of social science. A common approach to these texts is to use the substantive work to illuminate the methodological writings, and to do the reverse as well. During the period of his essays on methodology, Merton of course was intensely engaged in empirical research, or in collaborating in empirical research, and he makes the links himself. His writings reflect his experiences after arriving at Columbia and collaborating with Lazarsfeld. The relationship between them was complex. Merton often substituted for Lazarsfeld in classes, and in terms of teaching, they presented a more or less unified front. In this respect, they were both signed onto the Columbia model of theory construction. But Lazarsfeld's characteristic concerns, and his own intellectual contributions, as he understood them, were distinct from Merton's.

One feature of Lazarsfeld's career is particularly relevant to the issues we have just touched on: the problems of confounding and the question of what middle range theory was an alternative to. Lazarsfeld understood his own methodological work in terms of a rivalry with Herbert Simon. Simon's paper "On the Definition of the Causal Relation" (1952), which is intelligible only in relation to Simon's "Causal Ordering and Identifiability" (1953) to which it refers, was taught from as early as 1953 in the joint course that Lazarsfeld taught with Ernest Nagel, with expositions both by Nagel and Lazarsfeld[11]. Simon's "Spurious Correlation: A Causal Interpretation" (1954) acknowledges Lazarsfeld and refers to the "elaboration" model, Lazarsfeld's alternative. This important—and defining—rivalry between Lazarsfeld and Simon has a great deal to do both with the argument for middle range theory and the subsequent history of American sociology, as we will see. But its relation to Merton himself, at least from the point of view of the available material in print, is a puzzle. Merton never mentions Simon, except in the context of organization theory, ignores the body of methods that grew up after Simon's breakthrough papers, and says little if anything about the problem of confounding, the genuineness of causes, and related issues.

What was Merton's relation to the Lazarsfeldian project and how did this relation appear to others at the time? What did he understand? What did he subscribe to? Because of the way theory and methods were presented in the graduate program, students tended to see the Columbia way as a more or less coherent whole. People in the Lazarsfeld camp tended to see Merton as an advocate of the Lazarsfeldian program rather than as a contributor. Herbert Hyman comments that

> Merton had extracted scientific by-products from specific studies, demonstrating only, although persuasively, that particular applied research projects could also have scientific value. However, in 1948 he published the first of a series of essays making an eloquent general argument for the value of applied research in the development of sociological theory, documenting it with examples from Bureau projects and other learned references. A revised version appeared in his *Social Theory and Social Structure* (1949, 1957, 1968), the message thus being disseminated over the years in the English and many foreign language editions of the text. (Hyman 1991, 204)

Lazarsfeld had little or no interest in theory, and no interest in (and perhaps no understanding of) formalization in the normal sense of the term. The young Columbia trained philosopher of science Patrick Suppes had a modest relationship with Lazarsfeld and sent him drafts of his writings on axiomatic measurement theory; Lazarsfeld seems not to have responded, or perhaps even read, these drafts. When Lazarsfeld presented a mathematical model which he called a "theory" at a 1959 World Congress of Sociology, he was reproved by Suppes, who noted that it was "not a theory in the ordinary acceptation of the term" (International Sociological Association 1961, 350-51). But this lack of interest was belied by the course he ran for years jointly with Nagel[12], and a parallel discussion group primarily for faculty which he also sponsored, which considered the larger issues of “methodology,” including philosophical issues. And the texts which he and his collaborator produced on the *Language of Social Research* (1955) reflected this: the term “language” itself signified an ambition akin to logical positivism to define the terms of the domain of social research.

Merton was not a major participant in these efforts. But neither was he absent from them. Not only was he a scheduled participant in Lazarsfeld’s class with Nagel, in one memorable case he faced off of against Nagel, who had produced a passage from Parsons which Nagel criticized as gibberish. Merton responded by providing an elegant defense and explication of Parsons.[13] Lazarsfeld would not have done this, or have been able to do this. Yet there are puzzles here. In one of his oral histories Lazarsfeld comments that middle range theory was a misleading term, since Merton himself didn’t believe that there was anything beyond middle range theory– beyond what the term seemed to promise or allude to. Lazarsfeld certainly did not believe there was anything beyond middle range theory. Did Merton have the same views, or were his polite concessions in print to Parsons’ project sincere? More to the point, what did Merton himself understand or think about the issues that concerned Lazarsfeld? Did the division of labor in their partnership reflect their shared views of issues of theory and method, or was it more a matter of mutual usefulness with little overlap of concerns? Their early contact, before either of them was at Columbia, involved a shared concern over the objectivity of survey measurement. But the pattern of their later interaction was not so much one of concerns on which they exchanged views, and worked out a common viewpoint, but different concerns, which fit together without conflict because their identities and focus were different.

Lazarsfeld’s concerns were, however, relevant in an important way to the idea of middle range theory, and to its ultimate fate. The model for the material on which middle range theorists would work was the kind of studies done by Lazarsfeld: the kinds of single survey projects that developed measures or classifications to match concepts that were thought to express the important aspects of the situation, which usually could be presented in two by two tables and reasoned about in terms of the model called "elaboration" at Columbia. Elaboration amounted to adding a classification by a confounder to a one variable relationship to see whether the apparent relationship—a treatment effect, for example—disappeared or was reversed. The reasoning was based on what later became known as Simpson's paradox, and was, as a matter of statistics, without novelty. The core reasoning was taken from the standard statistics textbook of the first half of the twentieth century, widely known by the name of the authors, Yule and Kendall, where it was called partialling (1937). Complex applications of this type of reasoning to causal situations occurred in the 1920s in sociology, so the reasoning itself was not an innovation even in sociology. The point of the elaboration model was to determine whether or not the original zero-order relation held up or disappeared when a control variable with a suspected confounder was introduced. If the control variable was thought to be a causal antecedent to the other variables, it "explained" the relationship, meaning the observed relation was the result of the relation of each the variables with the antecedent variable, and not on the causal relations between them, a relationship which now could be regarded as spurious. The term "interpretation" was used when the control variable was assumed to intervene or be a causal intermediary between the original two variables or categories.

The key term is "assume." The holy grail of this kind of analysis was to get the statistics, rather than our prior ideas about what causes what, to determine what the actual causal structure was.[14] Lazarsfeld's model had been the experiment, and he had to assume that the survey was the equivalent of an experiment: that the relevant variables had been included and that all the possible causal relationships were understood. But the assumption that the uncontrolled survey situation resembled a controlled experiment was fatally vulnerable. As Hyman admitted,

> the reader may feel somewhat uneasy because of the dilemma, noted several times in this discussion, that there are no systematic criteria for differentiating developmental sequences or configurations from instances of spuriousness. We have been unable to establish formal criteria up to this time. The writer and Dr. Hans Zetterberg are working on one approach which if ultimately fruitful will be reported in the literature. (1955, 263n25)

The results never appeared. And the issue remained a burr under Lazarsfeld's saddle.[15]

Lazarsfeld knew very well that Herbert Simon had made a major breakthrough in resolving this problem. Simon's paper on the subject of causal ordering was in the reading list of the course he taught with Nagel, and the subject of two weeks of class work. Both Nagel and Lazarsfeld presented the work, on separate days[16]. What Simon had done was to provide an account of how to derive the order of relations between variables, rather than to simply assume them. Of course, these derivations needed assumptions too. But the assumptions were less onerous, and less needed to be assumed. Simon's strategy relied on minimal causal knowledge– essentially knowledge that something could not be the cause of something else (for example because it preceded it in time). This was a fundamentally different style than Lazarsfeld's. It was less reliant on “theory” even in the informal sense of this term as it was used in sociology. The assumptions it needed could be, in the best circumstances at least, limited to banal background knowledge that no one contested. Although the methods could be used with invented measures conceptualized for the purpose of testing hypotheses, it could also be used to produce determinate results with ordinary data collected for other purposes.

Lazarsfeld saw Simon as a threat—indeed, he was terrified of him, as is shown in an episode following the publication of Hyman’s *Survey Analysis*. Hyman had presented the method of elaboration in terms of examples from Stouffer’s *American Soldier* (1949). When the book appeared, Lazarsfeld, who identified with the project as a statement of the BASR point of view, was upset at a comment made by Hyman about the strangeness of the procedure of ordering causes that could not be time-ordered by treating the more general facts as prior. This was an idea of Lazarsfeld’s, and it was a way of reducing the arbitrariness of the practice of elaboration. Lazarsfeld thought Hyman’s presentation of this idea as “strange” was catastrophic, and wrote a long memo explaining why, and suggesting that a mimeographed rewrite be put out to “the one or two score of people who might be willing and able to follow such a discussion.” The person he did not want to notice was Simon. “I have been jittery since the appearance of the book that someone like Herb Simon will sit down and do the job I have sketched in this memorandum. This would be embarrassing indeed.”[17] Lazarsfeld’s obsession with Simon continued for years. In the 1960s, he commissioned Terry Clark to write a paper comparing the elaboration model to the Simon-derived structural equation models that were beginning to appear in sociology, with the aim of identifying something that the Lazarsfeld approach could claim to do better.[18] Simon won this argument, by showing that spuriousness could be understood as an aspect of the problem of causal ordering itself– and thus made into the kind of problem that could be settled with data rather than by assumption (Simon 1954, 8). But the fact that causal ordering could be derived, in at least some cases, rather than having to be assumed, was a dramatic change. The fact that Simon continued to need to make assumptions led Lazarsfeld to emphasize this feature of causal analysis (Simon 1979).

At the time the larger significance of this struggle over methodology was not clear beyond a small group, as Lazarsfeld's reference to the score or so people who would understand the issues with Hyman's book makes clear. Merton was probably not among the number in the know. But in 1975, after the dust settled, Merton's student Lewis Coser produced a remarkable presidential address to the American Sociological Association (1975), attacking both ethnomethodology and the Wisconsin school, which had applied developed forms of Simon's methods to one of the classic problems of American sociology: status attainment. The problem of status attainment originated in the world wide concern at the end of the nineteenth century over the problem of rural depletion: the problem of the quality of the population left as a result of the mass migrations to cities of the late nineteenth century. Study of this problem turned into study of the problem of what traits distinguished those who left from those who stayed, and eventually into the problem of which traits made people upwardly mobile. Coser correctly understood that the methods used were a profound threat to the Columbia model. They did not rely on "theory" in the same way. Basic background knowledge was sufficient to warrant the assumptions. They did not use the analogy with experiment to generate Berelson and Steiner type findings that could be aggregated into middle range theories, nor did they invent new concepts to measure.

The Problem of Functionalism

Merton did not engage with these methodological issues himself. But he was forced to deal with the most dramatic "theoretical" issue raised by the 1960s, the demise of functionalism. This had a personal element. Students in the 1960s, inspired by C. Wright Mills' *The Sociological Imagination* (1959) and a revulsion against the panglossianism and aridity of Parsons, demonized functionalism and positivism. Merton was well aware of the sea change, which included the apostasy of his own student Alvin Gouldner, and he responded to it in an essay published in 1975, "Structural Analysis in Sociology," which retrospectively reinterpreted his own commitments in such a way as to differentiate them from the functionalism that was routinely denounced. This essay contains one of his few explicit discussions of philosophy of science and its relevance to sociology, though the point is characteristically oblique.

To understand the later essay it is essential to understand the two earlier essays which it reinterprets. The first was Merton's classic essay on "The Unanticipated Consequences of Purposive Social Action" (1936), a tour de force of intellectual history which took up an idea prominent in Weber but with, as Merton shows, many precursors and affinities. The second was his paper of 1949, "Manifest and Latent Functions" that introduced his famous analysis of the functionality of political machines (1949, 73-138).[19] The Logical Positivists had taken an interest in functional explanation in sociology during this period, and produced a series of major papers on the subject, applying their analysis to sociology, and in one case specifically to Merton. The results were revealing.

The philosopher who was closest to Columbia sociology was Ernest Nagel, perhaps the most ubiquitous philosopher of science of his generation. As we have seen, for many years he co-taught a course with Lazarsfeld. Merton cited Nagel's essay on functional analysis (1956) in the later editions of *Social Theory and Social Structure* among the later works that extended his ideas, explaining that Nagel had "formalized" Merton's "paradigm . . . in terms of an abstract set of notations designed to make explicit how its various parts are related to elements of the functional approach in biology" (Merton 1949, 138). But Nagel's paper, read carefully, is not a validation of Merton's ideas. Instead, it shows them to be unworkable. Nagel's point is that to be entitled to make the kinds of claims functionalists routinely made, they needed a great deal of theory in the form of laws. A complete explanation would require a theoretical account and determinate laws describing the set of equilibrium states that would count as satisfying the notion of functional for a given system. Not only is none of this to be found in Merton, there is no discussion of what such theories would amount to or how one would get them. Nor was Nagel impressed with the latent-manifest distinction. He pointed out that "unless 'subjective aim in view'" has some special explanatory status for him, "Merton's distinction between manifest and latent functions is vacuous, and all functions fall under the head of 'latent functions'," (1956, 271). And he observed, in line with his argument that completing a functionalist explanation required a full theory that specified such things as the determinants of the states of equilibrium, that Merton was interested "primarily in the preliminary stage of functional analysis, rather than the completed outcome of such an inquiry" (1956, 263).

The point of these criticisms is this: with teleological explanations there is no halfway house, no way to use them without being committed to the whole machinery of ends being explained in terms of other ends. This was a point made in one of the papers read in the Nagel-

Lazarsfeld class[20]: Hempel's paper on types (1952), which made the point that even the teleological abstractions of the economic theory of rational action were nonexplanatory unless they could be grounded in a general theory of action that was explanatory. In the case of Merton, the issue was completion as well. To talk about, for example, the Boss system in municipal politics as functional is necessarily to be committed to questions about what the Boss system is functional for, what feedback mechanisms assure the functionality of the Boss system, and questions about how these mechanisms select for optimality or maximal equilibrium among possible functional solutions. These are, as Elster was later to reiterate, precisely the questions Merton does not answer. They are also why Nagel dismisses his interests as primarily in the preliminary stages of explanation. If Merton had attempted to go on to complete the analysis, he would have needed to construct a theory of the larger systems in which the Boss system is embedded. This is the path that leads to full blown functional or systems doctrines. In short, functionalism and middle range theory, if middle range theory is to be understood as an explanatory end point, are logically incompatible projects.

The 1975 essay is Merton's indirect response to these and other issues raised by the 1960's onslaught. In it, Merton reinterprets his own views in the light of the tumult of the '60s and the rise of post-positivist philosophy of science, reframing his position as a paradigm-like perspective called structural analysis to differentiate it from structural functionalism. The defense of this perspective as a distinctive approach is the theme of the volume in which it appears, and was a theme of later defenses of Merton (cf. Clark et al 1990). From the point of view of the history of methodology, this is a remarkable capitulation. The promise of middle-range theory was to free us from the problem of many approaches and few arrivals. By concentrating on empirical generalizations based on BASR type survey results, abducting from them and transferring our theoretical understanding of them to other contexts, then theoretically systematizing these results, and consolidating one theory with another, we would get conclusions, or arrivals, free of the tyranny of different approaches which arose from the ambition of sociologists to construct divergent frames of reference. Now we are told that structural analysis is itself an approach among others, and that the development of this approach is the core to Merton's claim to originality and his decisive difference from functionalism, and that theoretical pluralism– which he had confidently proclaimed the end of in 1948– was a good thing, if it didn't collapse into subjectivity and anarchism.

To make this concession was simply to bow to the reality of what had transpired in every major substantive area of sociology: each was characterized not by coherent middle range results but by multiple rival approaches. Deviance, for example, which Merton himself treated as a poster child for the promise of middle range theory and the pointlessness of comprehensive theory, had, among its "many approaches," in addition to his own influential ideas about *anomie*, well-established perspectives like labeling theory, differential association theory, power theories, and so forth. So retreating to the idea of having an approach was to give up on the core motivation for middle range theory. Yet the argument retains a key ambiguity. We are told that theoretical pluralism is, within limits, a good thing, but we are also told that linking up the ideas of structural analysis with "complementary ideas in other paradigms" will help "continue to make modest theoretical consolidations toward the ultimate and still very remote ideal of a unified comprehensive theory" (1975, 52). The "arrival," it appears, is still the ultimate goal, it is merely more remote.

But this written down version of Merton's approach turned out to be distinctly problematic from the point of view of explanation. Merton was able to point to his many appeals to concepts that qualified functionalism– to his uses of "dysfunction," his notion of ambivalence, and his discussion of the Matthew effect, which runs contrary to the core meritocratic nature of science– as evidence of his commitment to a "structural" approach. And he was also able to show the affinities of his language with that of Marxism and its notion of contradiction. But Marxism is straightforwardly a form of historical teleology, which is entitled to the notion of contradiction because it has an idea of what would count as a future resolution of the contradiction. What entitles Merton to his usages? And what are the usages? He uses the term "generate" to characterize how social structures produce, for example, differing rates of deviance, and how they generate change within themselves (1975, 35). One way they produce change is through "strains, conflict, and contradictions" which amplify "dysfunctional consequences" (1975, 36). The conflicts themselves are generated by structure as a result of the fact that social structures differentiate into arrays of statuses, strata, organizations, and so on "which have their own and therefore potentially conflicting as well as common interests and values" (1975, 35).

There is a recognizable pattern of explanation here: social structure does something that produces side-effects that partially conflict with the main things it does. But the pattern is functionalist: the "doing" makes sense only as a functional notion, and the "dysfunctional consequences" make sense only if we have a functional notion to start with. But to use the notion of function, as Nagel and Hempel pointed out in the readings for the Nagel-Lazarsfeld course of the 1950s, and Jon Elster was to point out later, commits the user to the rest of the story: a fullfledged account of the ends and the mechanisms that produce a directional relation to those ends. The structural approach was parasitic on the functionalism it disowned.

Regretting Nothing?

Merton lived through the implosion of functionalism, the end of Logical Positivism, the supersession of the model of statistical analysis that his concept of middle range theory depended on, and the failure of the behavioral science model he had promoted in the post war period. What was his response? And how did his contemporaries respond? One close contemporary was Edward Shils, whose career has many eerie parallels to Merton's: both came from a Jewish milieu in Philadelphia in which education and professional aspiration was the norm, both had formative intellectual and to some extent personal intellectual interaction with Mannheim and Parsons, both read classical social theory obsessively, both studied science, and both were enthusiastic participants in the efflorescence of "behavioral science" in the post war period, the time of Merton's original methodological essays. Shils wrote a somewhat parallel but much less well-known essay in the same period (1949). Shils, however, disowned his former enthusiasms. He wrote in 1980 that

> when I look back on my writings of the beginnings of this period, I am mortified by what appears now as callowness and the will to believe. I must acknowledge that for at least five years after the end of World War II, I thought that knowledge gained through the methods and theories of the social sciences might develop sufficiently so that it would

> contribute to the wisdom of the public and the governments of western societies. (1980, 21)

Merton never published such a statement. The concessions made in “Structural Analysis in Sociology” in 1975 were serious, but far from an expression of mortification.

Instead, Merton tried to salvage what he could from the original vision by the device of pushing the realization of the dream of a unified theory off into the remote future. By this time he had lived for twenty years with Nagel’s critique, without responding and without acknowledging its content. He emphasized his differences with the more naive forms of functionalism, without ridding his thought of the functionalist vocabulary that provided its explanatory power. He took no notice of the revolution in statistical analysis that had marginalized Lazarsfeld. If he had understood the methodological writings as promoting a tactical bet on a strategy for the development of sociology, he would have known that the bet had gone bad. But he did not abandon it, professing instead to hope that it would eventually pay out. Our estimate of Merton as a methodological thinker comes down to the question of whether the bet is, after all this, still credible, or ever was credible.

But as always, there are ambiguities. Merton should have known by the middle 1950s, from Nagel and from the Simon papers discussed in the Lazarsfeld -Nagel class, that the positions he had taken in *Social Theory and Social Structure* were untenable, that the Lazarsfeld model for dealing with causation was superseded, and that the demands of functional explanation were such that “middle range theory” using functional concepts, including dysfunctions and strains, were merely, as Jerry Fodor has recently put it, attempts to “take out loans” on a more inclusive sort of functionalism on the assumption that they could be repaid "sooner or later" (Fodor 2007, 4). In other words, he should have known that this was a bad bet from a very early point. But for whatever reason, Merton never acknowledged any of the issues. Was it that Merton simply believed that the products of the Columbia model made sense and were of value in spite of all of this? Perhaps. One thing is clear, however. None of this had anything to do with the sixties, or with rebellious students. The figures who provided the case against Merton's position were established members of the academy with careers that stretched back to the 1920s and 30s. Merton, for whatever reason failed to respond. The mystery is why. Perhaps the answer is his faith in the ultimate vindication of his position. But it is still a mystery as to why he believed, if he in fact did.

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Notes

[1]. The are collected in the first section of *Social Theory and Social Structure* (1968), and include his essays "On Sociological Theories of the Middle Range" (1949, 39-72), "Manifest and Latent Functions"(1949, 73-138), "The Bearing of Sociological Theory on Social Research" (1949, 139-155), and "The Bearing of Empirical Research on Sociological Theory" (1948b, 156-171). Among his other methodological papers should perhaps be included his earlier comment (1948a) on Talcott Parsons' "The Position of Sociological Theory" (Parsons 1948), which are repeatedly cited in *Social Theory and Social Structure*, and his much later paper on structural explanation (1975).

[2]. It should be added-- a point that will be discussed further below-- that perhaps these enigmas are the result of lack of data. Merton's papers, as of the writing of this paper, are not yet available, and it is possible that they will provide answers to these questions. If that is the case, this paper can be read as a set of problems that the papers need to solve or questions that they need to answer.

[3]. The term "transfer" appears as a term of art in memos about the *Uses of Sociology* (Lazersfeld et al 1967) (Ernest Nagel, Teaching Materials: Logic of Social Research 1964, Box 20, *Nagel Collected Papers* 1930-1988, Rare Book and Manuscript Library Columbia University). The reason this was thought to be distinctive was that correlational sociology in the tradition represented by William F. Ogburn treated correlations which were suitably partialled and ordered as explanatory on their own, did not think there was a need for additional theoretical backing, and did not think applying the results in other contexts added value or explanatory power to the original analysis, and more generally treated theoretical interpretation as non-objective: Ogburn famously likened the interpretation of a scattergram to the interpretation of an

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editorial cartoon. This is an important distinction in the subsequent literature (cf. Glymour 1983, for a current defense of the idea that generalization beyond a limited setting is irrelevant to making causal inferences within the setting).

[4]. The passage in Pierce from which this comes makes this point in a backhanded way:

> Long before I first classed abduction as an inference it was recognized by logicians that the operation of adopting an explanatory hypothesis– which is just what abduction is– was subject to certain conditions. Namely, the hypothesis cannot be admitted, even as a hypothesis, unless it be supposed that it would account for the facts or some of them. The form of inference, therefore is this:
>
> The surprising fact, C, is observed;
>
> But if A were true, C would be a matter of course,
>
> Hence, there is reason to suspect A is true.
>
> Thus, A cannot be abductively conjectured until its entire content is already present in the premiss, "If A were true, C would be a matter of course." (Pierce Papers 1934, Vol. V, 5.189, p.117)

The point is that the content of A cannot be gotten out of C, but needs to go beyond but imply C. This is the exact opposite of the idea that concepts can emerge from the data: instead, new concepts must be brought to the facts which account for them.

[5] . The sole reference seems to be in a comment on the neglect of the contribution of Dorothy Swaine Thomas, in which Merton notes that Schutz and Peter McHugh fail to mention her as the co-author of a paper on multiple discoveries (1973, 447n19).

[6] The reference to indirect tests is presumably to the method of residues, but he might also have had in mind the other eliminative methods, the methods of difference and the combined method of agreement and difference.

[7]. This is discussed in Durkheim 1982: 151; cf. Turner 1996, 368.

[8]. Zetterberg was also widely reviled as a person, as a result of his two year reign as Chair of the Sociology Department at Ohio State, during which, in one of the defining moments of the 1960s in American Sociology, he initiated a dramatic purge and expulsion of sociologists, mostly young and some tenured.

[9]. The literal meaning of even this probabilistic construal of “the greater the x, the greater the y,” if it is a genuine “empirical generalization,” would be that the pattern of probabilities would hold for every subset of the group about which the generalization was made, something that is obviously going to be false. The statistical tests involve the idea of a (mythical) universe from which actual data is a sample, and makes the probability statements into claims about this universe, and the tests about whether the sample might be from this universe. Why does this matter? Because explanation involves the implications of the general claims. But nothing specific about any officer or even set of actual officers is directly implied by claims about the universe. So these claims aren't going to “explain” in a strict sense, and any given set of actual data is going to be consistent with an indefinitely large array of claims about the universe.

[10]. Merton's repeated references to Clark Hull as a model are even more telling. Hull, under the influence of the philosopher Kenneth Spence, concocted a system of postulates involving the notion of structure and function. Spence– not unlike, as we will see, Merton's own philosophical interlocutor Ernest Nagel– rejected Hull's axiomatizing efforts as nonsensical.

[11]. Ernest Nagel, Teaching Materials: Logic of Social Inquiry1953, Box 20, *Nagel Collected Papers* 1930-1988, Rare Book and Manuscript Library Columbia University.

[12]. Ernest Nagel, Teaching Materials: Logic of Social Inquiry, Box 20, *Nagel Collected Papers* 1930-1988, Rare Book and Manuscript Library Columbia University.

[13]. The event was described to me by the late Howard Smokler, a philosophy of science student at Columbia who was a student in the class.

[14]. The core underlying causal idea here is the idea of net effects, which has a long history and is even acknowledged by Durkheim (Turner 1997, 29). I disagree with Andrew Abbot's interesting account of the rise of causal modeling in sociology (1998) because he seems to think that the various sayings of sociological methodologists about "forces" and the like represent some sort of nascent metaphysics that is worth criticizing. As Judea Pearl notes in *Causality*, "the causal relationship 'not affected by' is . . . the *only* causal notion that has found a place in statistics textbooks" (2000, 139n, italics in the original). The core idea that motivated Simon, as I have suggested here, is the idea of making the statistics alone determine causality or non-spuriousness. The question is whether this is possible– or whether some very minimal sorts of causal information must be added– has motivated the literature ever since his breakthrough paper. Pearl describes the technical issues with the many attempts to produce a statistical test for

confounding, the strategy on which they have converged, and the near-success but ultimate failure of purely statistical solutions to this problem (2000, 182-89). Ironically, Pearl's solution harkens back to the solution given by Giddings (Turner 2007, 17).

[15]. As late as 1963, he was assigning term papers on the problems, conflicts, and inadequacies of Hyman's two key chapters on spuriousness (Terry N. Clark, A terminological and conceptual analysis of the central elements of chapters VI and VII in *Survey Design and Analysis*, Lazersfeld Papers, Columbia University).

[16]. Ernest Nagel, Teaching Materials: Logic of Social Inquiry, 1953, Box 20, *Nagel Collected Papers* 1930-1988, Rare Book and Manuscript Library Columbia University.

[17]. Memorandum, Lazarsfeld to Hyman and Zetterberg, May 14, 1956, Box 20, *Nagel Collected Papers* (1930-1988), Rare Book and Manuscript Library Columbia University.

[18]. Terry N. Clark Methods File Part 1; Series III Additional Files, Box 1960-74, Simon Method, *Lazarsfeld Papers*, Rare Book and Manuscript Library Columbia University. Lazarsfeld also realized that his legacy was at stake here. It is revealing that when Clark proposed a chapter to his dissertation that reflected the historical background of the other (and ultimately winning) side of this issue, Lazarsfeld demanded that it be taken out, with the comment "you are writing my history, not Phil Hauser's" (Clark 1998, 304). Hauser was the product of the Ogburn correlational tradition that reached back to Giddings and Pearson and led, via its econometric side, to Simon's breakthrough.

[19]. The inspiration for the example of political machines appears to have been a book which fascinated Lazarsfeld, an autobiography and apologia by a political boss– itself an anomalous

event– entitled *You're the Boss* (Flynn 1947). Public Opinion Series 4, Box 1, Bryce, James, *Lazersfeld Papers*, Rare Book and Manuscript Library Columbia University.

[20]. Ernest Nagel, Teaching Materials: Logic of Social Inquiry, 1953, Box 20, *Nagel Collected Papers* 1930-1988, Rare Book and Manuscript Library Columbia University.