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## SOCIOLOGICAL THEORY

ROBERT K. MERTON

### ABSTRACT

The phrase "sociological theory" has been used to refer to at least six types of analysis which differ significantly in their bearings on empirical research. These are methodology, general orientations, conceptual analysis, *postfactum* interpretations, empirical generalizations, and sociological theory. The distinctive limits and functions of each are described and illustrated. A typical case of the incorporation of an empirical generalization into a theoretic system is briefly considered. The conventions of formal derivation and codification are suggested as devices for aiding the integration of theory and empirical research.

The recent history of sociological theory can in large measure be written in terms of an alternation between two contrasting emphases. On the one hand, we observe those sociologists who seek above all to generalize, to find their way as rapidly as possible to the formulation of sociological laws. Tending to assess the significance of sociological work in terms of the scope rather than the demonstrability of generalizations, they eschew the "triviality" of detailed, small-scale observation and seek the grandeur of global summaries. At the other extreme stands a hardy band who do not hunt too closely the implications of their research but who remain confident and assured that what they report is so. To be sure, their reports of facts are verifiable and often verified, but they are somewhat at a loss to relate these facts to one another or even to explain why these, rather than other, observations have been made. For the first group the identifying motto would at times seem to be: "We do not know whether what we say is true, but it is at least significant." And for the radical empiricist the motto may read: "This is demonstrably so, but we cannot indicate its significance."

Whatever the bases of adherence to the one or the other of these camps—different but not necessarily contradictory accountings would be provided by psychologists, sociologists of knowledge, and historians of science—it is abundantly clear that there is no logical basis for their being ranged *against* each other. Generalizations can be tempered, if not with mercy, at least with disciplined

observation; close, detailed observations need not be rendered trivial by avoidance of their theoretical pertinence and implications.

With all this there will doubtless be widespread if, indeed, not unanimous agreement. But this very unanimity suggests that these remarks are platitudinous. If, however, one function of theory is to explore the implications of the seemingly self-evident, it may not be amiss to look into what is entailed by such programmatic statements about the relations of sociological theory and empirical research. In doing so, every effort should be made to avoid dwelling upon illustrations drawn from the "more mature" sciences—such as physics and biology—not because these do not exhibit the logical problems involved but because their very maturity permits these disciplines to deal *fruitfully* with abstractions of a high order to a degree which, it is submitted, is not yet the case with sociology. An indefinitely large number of discussions of scientific method have 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. Ultimately, sociological research must meet the canons of scientific method; immediately, the task is so to express these requirements that they may have more direct bearing on the analytical work which is at present feasible.

The term "sociological theory" has been widely used to refer to the products of sev-

eral related but distinct activities carried on by members of a professional group called sociologists. But since these several types of activity have significantly different bearings upon empirical social research—since they differ in their scientific functions—they should be distinguished for purposes of discussion. Moreover, such discriminations provide a basis for assessing the contributions and limitations characteristic of each of the following six types of work which are often lumped together as comprising sociological theory: (1) methodology; (2) general sociological orientations; (3) analysis of sociological concepts; (4) *post factum* sociological interpretations; (5) empirical generalizations in sociology; and (6) sociological theory.

#### METHODOLOGY

At the outset we should distinguish clearly between sociological theory, which has for its subject matter certain aspects of the interaction of men and is hence substantive, and methodology, or the logic of scientific procedure. The problems of methodology transcend those found in any one discipline, dealing either with those common to groups of disciplines<sup>1</sup> or, in more generalized form, with those common to all scientific inquiry. Methodology is not peculiarly bound up with sociological problems, and, though there is a plenitude of methodological discussions in books and journals of sociology, they are not thereby rendered sociological in character. Sociologists, in company with all others who essay scientific work, must be methodologically wise; they must be aware of the design of investigation, the nature of inference, the requirements of a theoretic system. But such knowledge does not contain or imply the particular *content* of socio-

logical theory. There is, in short, a clear and decisive difference between *knowing how to test* a battery of hypotheses and *knowing the theory* from which to derive hypotheses to be tested.<sup>2</sup> It is my impression that current sociological training is more largely designed to make students understand the first than the second.

As Poincaré observed a half-century ago, sociologists have long been hierophants of methodology, thus, perhaps, diverting talents and energies from the task of building substantive theory. This focus of attention upon the logics of procedure has its patent scientific function, since such inventories serve a critical purpose in guiding and assessing both theoretical and empirical inquiries. It also reflects the growing-pains of an immature discipline. Just as the apprentice who acquires new skills self-consciously examines each element of these skills, in contrast to the master who habitually practices them with seeming indifference to their explicit formulation, so the exponents of a discipline haltingly moving toward scientific status laboriously spell out the logical grounds of their procedure. The slim books on methodology which proliferate in the fields of sociology, economics, and psychology do not find many counterparts among the technical works in the sciences which have long since come of age. Whatever their intellectual function, these methodological writings imply the perspectives of a fledgling discipline, anxiously presenting its credentials for full status in the fraternity of the sciences. But, significantly enough, the instances of adequate scientific method utilized by sociologists for illustrative or expository methods are usually drawn from disciplines other than sociology itself. Twen-

<sup>1</sup> In recent years there have been several volumes which set forth methodological concerns of sociology: Florian Znaniecki, *The Method of Sociology* (New York: Farrar & Rinehart, 1934); R. M. MacIver, *Social Causation* (Boston: Ginn & Co., 1942); G. A. Lundberg, *Foundations of Sociology* (New York: Macmillan Co., 1939); Felix Kaufmann, *Methodology of the Social Sciences* (New York: Oxford University Press, 1944).

<sup>2</sup> However, it should be noted not only that instruments and procedures used in sociological (or other scientific) inquiry must meet methodological criteria but that they also logically presuppose substantive theories. As Pierre Duhem observed in this connection, the instruments as well as the experimental results obtained in science are shot through with specific assumptions and theories of a substantive order (*La Théorie physique* [Paris: Chevalier et Riviere, 1906], p. 278).

tieth-century, not sixteenth-century, physics and chemistry are taken as methodological prototypes or exemplars for twentieth-century sociology, with little explicit recognition that between sociology and these other sciences is a difference of millions of man-hours of sustained scientific research. These comparisons are inevitably programmatic rather than realistic. More appropriate methodological demands would result in a gap between methodological aspiration and actual sociological attainment at once less conspicuous and less invidious.

#### GENERAL SOCIOLOGICAL ORIENTATIONS

Much of what is described in textbooks as sociological theory consists of general orientations toward substantive materials. Such orientations involve broad postulates which indicate *types* of variables which are somehow to be taken into account rather than specifying determinate relationships between particular variables. Indispensable though these orientations are, they provide only the broadest framework for empirical inquiry. This is the case with Durkheim's generic hypothesis, which holds that the "determining cause of a social fact should be sought among the social facts preceding it" and identifies the "social" factor as institutional norms toward which behavior is oriented.<sup>3</sup> Or, again, it is said that "to a certain approximation it is useful to regard society as an integrated system of mutually interrelated and functionally interdependent parts."<sup>4</sup> So, too, the importance of the "humanistic coefficient" in cultural data as expounded by Znaniecki and Sorokin, among others, belongs to this category. Such general orientations may be paraphrased as saying in effect that the investigator ignores

this *order of fact* at his peril. They do not set forth specific hypotheses.

The chief function of these orientations is to provide a general context for inquiry; they facilitate the process of arriving at determinate hypotheses. To take a case in point: Malinowski was led to re-examine the Freudian notion of the Oedipus complex on the basis of a general sociological orientation, which viewed sentiment formation as patterned by social structure. This generic view clearly underlay his exploration of a specific "psychological" complex in its relation to a system of status relationships in a society differing in structure from that of western Europe. The *specific* hypotheses which he utilized in this inquiry were all congruent with the generic orientation but were not prescribed by it. Otherwise put, the general orientation indicated the relevance of *some* structural variables, but there still remained the task of ferreting out the particular variables to be included.

Though such general theoretic outlooks have a more inclusive and profound effect on the development of scientific inquiry than do specific hypotheses—they constitute the matrix from which, in the words of Maurice Arthus, "new hypotheses follow one another in breathless succession and a harvest of facts follow closely the blossoming of these hypotheses"—though this is the case, they constitute only the point of departure for the theorist. It is his task to develop specific, interrelated hypotheses by reformulating empirical generalizations in the light of these generic orientations.

It should be noted, furthermore, that the growing contributions of sociological theory to its sister-disciplines lie more in the realm of general sociological orientations than in that of specific confirmed hypotheses. The development of "social history," of institutional economics, and the importation of sociological perspectives into psycho-analytic theory involve recognition of the sociological dimensions of the data rather than incorporation of specific confirmed theories. Social scientists have been led to detect sociological gaps in the application

<sup>3</sup> Émile Durkheim, *The Rules of Sociological Method* (Chicago: University of Chicago Press, 1938), p. 110; *L'Éducation morale* (Paris: Félix Alcan, 1925), pp. 9-45, *passim*.

<sup>4</sup> Conrad M. Arensberg and Solon Kimball, *Family and Community in Ireland* (Cambridge: Harvard University Press, 1940), p. xxvi.

of their theory to concrete social behavior. They do not so often exhibit "sociological naïveté" in their interpretations. The economist, the political scientist, and the psychologist have increasingly come to recognize that what they have systematically taken as given, as data, may be sociologically problematical. But this receptivity to a sociological outlook is often dissipated by the paucity of adequately *tested specific theories* of, say, the determinants of human wants or of the social processes involved in the distribution and exercise of social power. Pressures deriving from the respective theoretic gaps of the several social sciences may serve, in time, to bring about an increasing formulation of specific and systematic sociological theories appropriate to the problems implied by these gaps. General orientations do not suffice. Presumably this is the context for the complaint voiced by an economist:

[The economist always seeks to refer his analysis of a problem] back to some "datum," that is to say, to something which is extra-economic. This something may be apparently very remote from the problem which was first taken up, for the chains of economic causation are often very long. But he always wants to hand over the problem in the end to some sociologist or other—if there is a sociologist waiting for him. Very often there isn't.<sup>5</sup>

#### ANALYSIS OF SOCIOLOGICAL CONCEPTS

It is at times held that theory is comprised of concepts, an assertion which, being incomplete, is neither true nor false but vague. To be sure, "conceptual analysis," which is confined to the specification and clarification of key concepts, is an indispensable phase of theoretic work. But an array of concepts—status, role, *Gemeinschaft*, social interaction, social distance, *anomie*—does not constitute theory, though it may enter into a theoretic system. It may be conjectured that, in so far as an antitheoretic bias occurs among sociologists, it is in protest against those who identify theory with

clarification of definitions, who mistakenly take the part for the whole of theoretic analysis. It is only when such concepts are interrelated in the form of a scheme that a theory begins to emerge. Concepts, then, constitute the definitions (or prescriptions) of what is to be observed; they are the variables between which empirical relationships are to be sought. When propositions stating such relationships are logically interrelated, a theory has been instituted.

The choice of concepts guiding the collection and analysis of data is, of course, crucial to empirical inquiry. For, to state an important truism, if concepts are selected such that no relationships between them obtain, the research will be sterile, no matter how meticulous the subsequent observations and inferences. The importance of this truism lies in its implication that truly trial-and-error procedures in empirical inquiry are likely to be comparatively unfruitful, since the number of variables which are not significantly connected is indefinitely large.

It is, then, one function of conceptual clarification to make explicit the character of the data subsumed under a given concept.<sup>6</sup> It thus serves to reduce the likelihood of spurious empirical findings couched in terms of the given concepts. Thus, Sutherland's re-examination of the received concept of "crime" provides an instructive instance of how such clarification induces a revision of hypotheses concerning the data organized

<sup>6</sup> As Schumpeter remarks about the role of "analytic apparatus": "If we are to speak about price levels and to devise methods of measuring them, we must know what a price level is. If we are to observe demand, we must have a precise concept of its elasticity. If we speak about productivity of labor, we must know what propositions hold true about total product per man-hour and what other propositions hold true about the partial differential coefficient of total product with respect to man-hours. No hypotheses enter into such concepts, which simply embody methods of description and measurement, nor into the propositions defining their relations (so-called theorems), and yet their framing is the chief task of theory, in economics as elsewhere. This is what we mean by *tools of analysis*" (Joseph A. Schumpeter, *Business Cycles* [New York: McGraw-Hill Book Co., 1939], I, 31).

<sup>5</sup> J. R. Hicks, "Economic Theory and the Social Sciences," *The Social Sciences: Their Relations in Theory and in Teaching* (London: Le Play Press, 1936), p. 135. (Italics mine.)

in terms of the concept.<sup>7</sup> He demonstrates an equivocation implicit in criminological theories which seek to account for the fact that there is a much higher rate of crime, as "officially measured," in the lower than in the upper social classes. These crime "data" (organized in terms of a particular operational concept or measure of crime) have led to a series of hypotheses which view poverty, "slum conditions," feeble-mindedness, and other characteristics held to be highly associated with low-class status as the "causes" of criminal behavior. Once the concept of crime is clarified to refer to the violation of criminal law and is thus extended to include "white-collar criminality" in business and the professions—violations which are less often reflected in official crime statistics than are lower-class violations—the presumptive high association between low social status and crime may no longer obtain. We need not pursue Sutherland's analysis further to detect the function of conceptual clarification in this instance. It provides for a *reconstruction of data* by indicating more precisely just what they include and what they exclude. In doing so, it leads to a liquidation of hypotheses set up to account for spurious data by questioning the assumptions on which the initial statistical data were based. By hanging a question mark on an implicit assumption underlying the research definition of crime—the assumption that violations of the criminal code by members of the several social classes are representatively registered in the official statistics—this conceptual clarification had direct implications for a nucleus of theories.

In similar fashion conceptual analysis may often resolve apparent antinomies in empirical findings by indicating that such contradictions are more apparent than real. This familiar phrase refers, in part, to the fact that initially crudely defined concepts have tacitly included significantly different elements so that data organized in terms of these concepts differ materially and thus ex-

hibit apparently contradictory tendencies.<sup>8</sup> The function of conceptual analysis in this instance is to maximize the likelihood of the comparability, in significant respects, of data which are to be included in a research.

The instance drawn from Sutherland merely illustrates the more general fact that in research, as in less disciplined activities, our conceptual language tends to fix our perceptions and, derivatively, our thought and behavior. The concept defines the situation, and the research worker responds accordingly. Explicit conceptual analysis aids him to recognize to what he is responding and which (possibly significant) elements he is ignoring. The findings of Whorf on this matter are, with appropriate modifications, applicable to empirical research.<sup>9</sup> He found that behavior was oriented toward linguistic or conceptual meanings connoted by the terms applied to a given situation. Thus, in the presence of objects which are conceptually described as "gasoline drums," behavior will tend modally toward a given type: great care will be exercised. But when people are confronted with what are called "empty gasoline drums," behavior is different: it is careless, with little control over smoking and the disposition of cigarette stubs. Yet the "empty" drums are the more hazardous, since they contain explosive vapor. Response is not to the physical but to the conceptualized situation. The concept "empty" is here used equivocally: as a synonym for "null and void, negative, inert," and as a term applied to physical situations without regard to such "irrelevancies" as vapor and liquid vestiges in the container. The situation is conceptualized in the second

<sup>8</sup> Elaborate formulations of this type of analysis are to be found in Corrado Gini, *Prime linee di patologia economica* (Milan: Giuffrè, 1935); for a brief discussion see C. Gini, "Un tentativo di armonizzare teorie disparate e osservazioni contrastanti nel campo dei fenomeni sociali," *Rivista di politica economica*, XII (1935), 1-23.

<sup>9</sup> B. L. Whorf, "Relation of Habitual Thought and Behavior to Language," in L. Spier, A. I. Halliwell, and S. S. Newman (eds.), *Language, Culture, and Personality* (Menasha: Sapir Memorial Fund Publication, 1941), pp. 75-93.

<sup>7</sup> Edwin H. Sutherland, "White-Collar Criminality," *American Sociological Review*, V (1940), 1-12.

sense, and the concept is then responded to in the first sense, with the result that "empty" gasoline drums become the occasion for fires. Clarification of just what "empty" means in the given universe of discourse would have a profound effect on behavior. This case may serve as a paradigm of the functional effect of conceptual clarification upon research behavior: it makes clear just what the research worker is doing when he deals with conceptualized data. He draws different consequences for empirical research as his conceptual apparatus changes.

A further task of conceptual analysis is to institute observable indices of the social data with which empirical research is concerned. Early efforts in this direction were manifest in the works of Durkheim (and constitute one of his most significant contributions to sociology). Though his formalized conceptions along these lines do not approach the sophistication of more recent formulations, he was patently utilizing "intervening variables," as lately described by Tolman and Hull, and seeking to establish indices for these variables.<sup>10</sup> The problem, as far as it need be stated for our immediate purposes, consists in devising indices of unobservables or symbolic constructs (e.g., social cohesion)—indices which are theoretically supportable. Conceptual analysis thus enters as one basis for an initial and periodic critical appraisal of the extent to which assumed signs and symbols are an adequate index of the social substratum. Such analysis suggests clues for determining

whether in fact the index (or measuring instrument) proves adequate to the occasion.<sup>11</sup>

#### "POST FACTUM" SOCIOLOGICAL INTERPRETATIONS

It is often the case in empirical social research that data are collected and then subjected to interpretative comment. This procedure in which the observations are at hand and the interpretations are subsequently applied to the data has the logical structure of clinical inquiry. The observations may be case-history or statistical in character. The defining characteristic of this procedure is the introduction of an interpretation *after* the observations have been made rather than the empirical testing of a predesignated hypothesis. The implicit assumption is that a body of generalized propositions has been so fully established that it can be appropriately applied to the data in hand.

Such *post factum* explanations, designed to "explain" given observations, differ in logical function from speciously similar procedures where the observational materials are utilized in order to *derive* fresh hypotheses to be confirmed by *new* observations.

A disarming characteristic of this procedure is that the explanations are indeed consistent with the given set of observations. This is scarcely surprising, inasmuch as only those *post factum* hypotheses are selected which do accord with these observations. If the basic assumption holds—namely, that the *post factum* interpretation utilizes abundantly confirmed theories—then this type of explanation indeed "shoots arrowy light into the dark chaos of materials." But if, as is more often the case in sociological inter-

<sup>10</sup> Durkheim's basic formulation, variously repeated in each of his monographs, reads as follows: "It is necessary . . . to substitute for the internal fact which escapes us an external fact that symbolizes it and to study the former through the latter" (see his *Rules of Sociological Method*, chap. ii; *Le Suicide* [Paris: F. Alcan, 1930], p. 356; and *Division du travail social* [Paris: F. Alcan, 1932], pp. 22 ff.). Most detailed consideration of Durkheim's views on social indices is provided by Harry Alpert, *Émile Durkheim and His Sociology* (New York: Columbia University Press, 1939), pp. 120 ff. On the general problem see C. L. Hull, "The Problem of Intervening Variables in Molar Behavior Theory," *Psychological Review*, L (1943), 273-91.

<sup>11</sup> Among the many functions of conceptual analysis at this point is that of instituting inquiry into the question of whether or not the index is "neutral" to its environment. By searching out the assumptions underlying the selection (and validation for a given population) of observables as indices (e.g., religious affiliation, an attitude scale), conceptual analysis initiates appropriate tests of the possibility that the "index" has become dissociated from its substratum. For a clear statement of this point see Louis Guttman, "A Basis for Scaling Qualitative Data," *American Sociological Review*, IX (1944), 139-50, esp. 149-50.

pretation, the *post factum* hypotheses are also *ad hoc* or, at the least, have but a slight degree of prior confirmation, then such "precocious explanations," as H. S. Sullivan called them, produce a spurious sense of adequacy at the expense of instigating further inquiry.

*Post factum* explanations remain at the level of *plausibility* (low evidential value) rather than leading to "compelling evidence" (a high degree of confirmation). Plausibility, in distinction to compelling evidence, is found when an interpretation is consistent with one set of data (which typically has, indeed, given rise to the decision to utilize one, rather than another, interpretation). It also implies that alternative interpretations equally consistent with these data have not been systematically explored and that inferences drawn from the interpretation have not been tested by new observations.

The logical fallacy underlying the *post factum* explanation rests in the fact that there is available a variety of crude hypotheses, each with some measure of confirmation but designed to account for quite contradictory sets of affairs. The method of *post factum* explanation does not lend itself to nullifiability, if only because it is so completely flexible. For example, it may be reported that "the unemployed tend to read fewer books than they did previously." This is "explained" by the hypothesis that anxiety increases as a consequence of unemployment and, therefore, that any activity requiring concentration, such as reading, becomes difficult. This type of accounting is plausible, since there is some evidence that increased anxiety *may* occur in such situations and since a state of morbid preoccupation does interfere with organized activity. If, however, it is now reported that the original data were erroneous and it is a fact that "the unemployed read more than previously" a new *post factum* explanation can at once be invoked. The explanation now holds that the unemployed have more leisure or that they engage in activity intended to increase

their personal skills. Consequently, they read more than previously. Thus, whatever the observations, a new interpretation can be found to "fit the facts."<sup>12</sup> This example may be sufficient to indicate that such reconstructions serve only as illustrations and not as tests. It is this logical inadequacy of the *post factum* construction which led Peirce to observe:

It is of the essence of induction that the consequence of the theory should be drawn first in regard to the unknown, or virtually unknown, result of experiment; and that this should virtually be only ascertained afterward. For if we look over the phenomena to find agreements with the theory, it is a mere question of ingenuity and industry how many we shall find.<sup>13</sup>

These reconstructions typically by-pass an explicit formulation of the conditions under which the hypotheses will be found to hold true. In order to meet this logical requirement, such interpretations would necessarily be predictive rather than postdictive.

As a case in point, we may note the frequency with which Blumer asserts that the Thomas-Znaniecki analyses of documents "merely seem to be plausible."<sup>14</sup> The basis for "plausibility" rests in the consistency between the interpretation and the data; the absence of compelling evidence stems from the failure to provide distinctive tests of the interpretations apart from their consistency with the initial observations. The analysis is fitted to the facts, and there is no indication of just which data would be taken to contravene the interpretations. As a consequence, the documentary evidence

<sup>12</sup> The pertinent data have not been assembled. But, on the plausibility of the second interpretation, see Douglas Waples, *People and Print: Social Aspects of Reading in the Depression* (Chicago: University of Chicago Press, 1937), p. 198.

<sup>13</sup> Charles Sanders Peirce, *Collected Papers*, ed. Charles Hartshorne and Paul Weiss (Cambridge: Harvard University Press, 1932), II, 496.

<sup>14</sup> Herbert Blumer, *An Appraisal of Thomas and Znaniecki's "The Polish Peasant in Europe and America"* (New York: Social Service Research Council, 1939), p. 38; see also *ibid.*, pp. 39, 44, 46, 49, 50, 75.



merely illustrates rather than tests the theory.<sup>15</sup>

#### EMPIRICAL GENERALIZATIONS IN SOCIOLOGY

Not infrequently it is said that the object of sociological theory is to arrive at statements of social uniformities. This is an elliptical assertion and hence requires clarification. For there are two types of statements of sociological uniformities which differ significantly in their bearing on theory. The first of these is the empirical generalization: an isolated proposition summarizing observed uniformities of relationships between two or more variables.<sup>16</sup> The sociological literature abounds with such generalizations which have not been assimilated to sociological theory. Thus, Engel's "laws" of consumption may be cited as examples. So, too, the Halbwachs finding that laborers spend more per adult unit for food than white-collar employees of the same income class.<sup>17</sup> Such generalizations may be of greater or less precision, but this does not affect their logical place in the structure of inquiry. The Groves-Ogburn finding, for a sample of American cities, that "cities with a larger percentage engaged in manufacturing also have, on the average, slightly larger percentages of young persons married" has been expressed in an equation indicating

the degree of this relationship. Although propositions of this order are essential in empirical social research, a miscellany of such propositions only provides the raw materials for sociology as a discipline. The theoretic task, and the orientation of empirical research toward theory, first begins when the bearing of such uniformities on a set of interrelated propositions is tentatively established. The notion of directed research implies that, in part,<sup>18</sup> empirical inquiry is so organized that if and when empirical uniformities are discovered, they have direct consequences for a theoretic system. In so far as the research is directed, the rationale of findings is set forth before the findings are obtained.

#### SOCIOLOGICAL THEORY

The second type of sociological generalization, the so-called "scientific law," differs from the foregoing inasmuch as it is a statement of invariance *derivable* from a theory. The paucity of such laws in the sociological field perhaps reflects the prevailing bifurcation of theory and empirical research. Despite the many volumes dealing with the history of sociological theory and despite

<sup>15</sup> It is difficult to see on what grounds Blumer asserts that these interpretations cannot be mere cases of illustration of a theory. His comment that the materials "acquire significance and understanding that they did not have" would apply to *post factum* explanations generally.

<sup>16</sup> This usage of the term "empirical" is common, as Dewey notes. In this context, "empirical" means that the subject-matter of a given proposition which has existential inference, represents merely a set of uniform conjunctions of traits repeatedly observed to exist, without any understanding of *why* the conjunction occurs; without a theory which states its rationale" (John Dewey, *Logic: The Theory of Inquiry* [New York: Henry Holt & Co., 1938], p. 193).

<sup>17</sup> See a considerable collection of such uniformities summarized by C. C. Zimmerman, *Consumption and Standards of Living* (New York: D. Van Nostrand Co., 1936), pp. 51 ff.

<sup>18</sup> "In part," if only because it stultifies the possibilities of obtaining fertile new findings to confine researches *wholly* to the test of predetermined hypotheses. "Hunches" originating in the course of the inquiry which may not have immediately obvious implications for a broader theoretic system may eventuate in the discovery of empirical uniformities which can later be incorporated into a theory. For example, in the sociology of political behavior, it has been recently established that the larger the number of social cross-pressures to which voters are subjected, the less interest they exhibit in a presidential election (P. F. Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice* [New York: Duell, Sloan & Pearce, 1944], pp. 56-64). This finding, which was wholly unanticipated when the research was first formulated, may well initiate new lines of systematic inquiry into political behavior, even though it is not yet integrated into a generalized theory. Fruitful empirical research not only tests theoretically derived hypotheses; it also originates new hypotheses. This might be termed the "serendipity" component of research, i.e., the discovery, by chance or sagacity, of valid results which were not sought for.

the plethora of empirical investigations, sociologists (including the writer) may discuss the logical criteria of sociological laws without citing a single instance which fully satisfies these criteria.<sup>19</sup>

Approximations to these criteria are not entirely wanting. To exhibit the relations of empirical generalizations to theory and to set forth the functions of theory, it may be useful to examine a familiar case in which such generalizations were incorporated into a body of substantive theory. Thus, it has long been established as a statistical uniformity that in a variety of populations, Catholics had a lower suicide rate than Protestants.<sup>20</sup> In this form the uniformity posed a theoretical problem. It merely constituted an empirical regularity which would become significant for theory only if it could be derived from a set of other propositions, a task which Durkheim set himself. If we restate his theoretic assumptions in formal fashion, the paradigm of his theoretic analysis becomes clear:

1. Social cohesion provides psychic support to group members subjected to acute stresses and anxieties.
2. Suicide rates are functions of *unrelieved* anxieties and stresses to which persons are subjected.
3. Catholics have greater social cohesion than Protestants.
4. Therefore, lower suicide rates should be anticipated among Catholics than among Protestants.<sup>21</sup>

<sup>19</sup> E.g., see the discussion by George A. Lundberg, "The Concept of Law in the Social Sciences," *Philosophy of Science*, V (1938), 189-203, which affirms the possibility of such laws without including any case in point. The book by K. D. Har, *Social Laws* (Chapel Hill: University of North Carolina Press, 1930), does not fulfil the promise implicit in the title. A panel of social scientists discussing the possibility of obtaining social laws finds it difficult to instance cases (Blumer, *op. cit.*, pp. 142-50).

<sup>20</sup> It need hardly be said that this statement assumes that education, income, nationality, rural-urban residence, and other factors which might render this finding spurious have been held constant.

<sup>21</sup> We need not examine further aspects of this illustration, e.g., (1) the extent to which we have adequately stated the premises implicit in Durkheim's

This case serves to locate the place of empirical generalizations in relation to theory and to illustrate the several functions of theory.

1. It indicates that theoretic pertinence is not inherently present or absent in empirical generalizations but appears when the generalization is conceptualized in abstractions of higher order (Catholicism—social cohesion—relieved anxieties—suicide rate) which are embodied in more general statements of relationships.<sup>22</sup> What was initially taken as an isolated uniformity is restated as a relation, not between religious affiliation and behavior, but between groups with certain conceptualized attributes (social cohesion) and the behavior. The *scope* of the original empirical finding is considerably extended, and several seemingly disparate uniformities are seen to be interrelated (thus differentials in suicide rates between married and single persons can be derived from the same theory).

2. Once having established the theoretic pertinence of a uniformity by deriving it from a set of interrelated propositions, we provide for the *cumulation* both of theory and of research findings. The differentials-in-suicide-rate uniformities add confirmation to the set of propositions from which they—and other uniformities—have been

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interpretation; (2) the supplementary theoretic analysis which would take these premises not as given but as problematic; (3) the grounds on which the potentially infinite regression of theoretic interpretations is halted at one rather than another point; (4) the problems involved in the introduction of such intervening variables as social cohesion which are not directly measured; (5) the extent to which the premises have been empirically confirmed; (6) the comparatively low order of abstraction represented by this illustration; and (7) the fact that Durkheim derived several empirical generalizations from this same set of hypotheses.

<sup>22</sup> Thorstein Veblen has put this with typical cogency: "All this may seem like taking pains about trivialities. But the data with which any scientific inquiry has to do are trivialities in some other bearing than that one in which they are of account" (*The Place of Science in Modern Civilization* [New York: Viking Press, 1932], p. 42).

derived. This is a major function of *systematic theory*.

3. Whereas the empirical uniformity did not lend itself to the drawing of diverse consequences, the reformulation gives rise to various consequences in fields of conduct quite remote from that of suicidal behavior. For example, inquiries into obsessive behavior, morbid preoccupations, and other maladaptive behavior have found these to be related to inadequacies of group cohesion.<sup>23</sup> The conversion of empirical uniformities into theoretic statements thus increases the *fruitfulness* of research through the successive exploration of implications.

4. By providing a rationale, the theory introduces a *ground for prediction* which is more secure than mere empirical extrapolation from previously observed trends. Thus, should independent measures indicate a decrease of social cohesion among Catholics, the theorist would predict a tendency toward increased rates of suicide in this group. The atheoretic empiricist would have no alternative, however, but to predict on the basis of extrapolation.

5. The foregoing list of functions presupposes one further attribute of theory which is not altogether true of the Durkheim formulation and which gives rise to a general problem that has peculiarly beset sociological theory, at least, up to the present. If theory is to be productive, it must be sufficiently *precise* to be *determinate*. Precision is an integral element of the criterion of *testability*. The prevailing pressure toward the utilization of statistical data in sociology, whenever possible, to control and test theoretic inferences has a justifiable basis, when we consider the logical place of precision in disciplined inquiry.

The more precise the inferences (predictions) which can be drawn from a theory,

the less the likelihood of *alternative* hypotheses which will be adequate to these predictions. In other words, precise predictions and data serve to reduce the *empirical* bearing upon research of the *logical* fallacy of affirming the consequent.<sup>24</sup> It is well known that verified predictions derived from a theory do not "prove" or "demonstrate" that theory; they merely supply a measure of confirmation, for it is always possible that alternative hypotheses drawn from different theoretic systems can also account for the predicted phenomena.<sup>25</sup> But those theories which admit of precise predictions confirmed by observation take on strategic importance since they provide an initial basis for choice between competing hypotheses. In other words, precision enhances the likelihood of approximating a "crucial" observation or experiment.

The internal coherence of a theory has much the same function, for if a variety of empirically confirmed consequences are drawn from one theoretic system, this reduces the likelihood that competing theories can adequately account for the same data. The integrated theory sustains a larger measure of confirmation than is the case with distinct and unrelated hypotheses, thus accumulating a greater weight of evidence.

<sup>24</sup> The paradigm of "proof through prediction" is, of course, logically fallacious:

If *A* (hypothesis), then *B* (prediction).

*B* is observed.

Therefore, *A* is true.

This is not overdisturbing for scientific research, inasmuch as other than formal criteria are involved.

<sup>25</sup> As a case in point, consider that different theorists had predicted war and internecine conflict on a large scale at the present time. Sorokin and some Marxists, for example, set forth this prediction on the basis of quite distinct theoretic systems. The actual outbreak of large-scale conflicts does not in itself enable us to choose between these schemes of analysis, if only because the observed fact is consistent with both. Only if the predictions had been so *specified*, had been so precise, that the actual occurrences coincided with the one prediction and not with the other, would a determinate test have been instituted.

<sup>23</sup> See, e.g., Elton Mayo, *Human Problems of an Industrial Civilization* (New York: Macmillan Co., 1933), p. 113 *et passim*. The theoretical framework utilized in the studies of industrial morale by Whitehead, Roethlisberger, and Dickson stemmed appreciably from the Durkheim formulations, as the authors testify.

Both pressures—toward precision and logical coherence—can lead to unproductive activity, particularly in the social sciences. Any procedure can be abused as well as used. A premature insistence on precision at all costs may sterilize imaginative hypotheses. It may lead to a reformulation of the scientific problem in order to permit measurement with, at times, the result that the subsequent materials do not bear on the initial problem in hand.<sup>26</sup> In the search for precision, care must be taken to see that significant problems are not thus inadvertently blotted from view. Similarly, the pressure for logical consistency has at times invited logomachy and sterile “theorizing,” inasmuch as the assumptions contained in the system of analysis are so far removed from empirical referents or involve such high abstractions as not to permit of empirical inquiry.<sup>27</sup> But the warrant for these criteria of inquiry is not vitiated by such abuses.

#### FORMAL DERIVATIONS AND CODIFICATION

This inevitably superficial account has, at the very least, pointed to the need for a closer connection between theory and empirical research. The prevailing division of the two is manifested in marked *discontinuities* of empirical research, on the one hand, and systematic theorizing unsustained by empirical test, on the other. There are conspicuously few instances of consecutive research which have cumulatively investigated a succession of hypotheses derived from a given theory. Rather, there tends to be a marked dispersion of empirical inquiries, oriented toward a concrete field of human behavior, but lacking a central theoretic orientation. The plethora of discrete

empirical generalizations and of *post factum* interpretations reflect this pattern of research. The large bulk of general orientations and conceptual analyses, as distinct from sets of interrelated hypotheses, in turn reflect the tendency to separate “theoretic activity” from empirical research. It is a commonplace that continuity, rather than dispersion, can be achieved only if our empirical studies are theory-oriented and if our theory is empirically confirmable. However, it is possible to go beyond such affirmations and to suggest certain conventions for sociological research which might well facilitate this process. These conventions may be termed “formalized derivation” and “codification.”<sup>28</sup>

Both in the design and in the reporting of empirical research, it might be made a definite convention that hypotheses and, whenever possible, the theoretic grounds (assumptions and postulates) of these hypotheses be explicitly set forth. The report of data would be in terms of their immediate pertinence for the hypotheses and, derivatively, the underlying theory. Attention should be called specifically to the introduction of interpretative variables other than those entailed in the original formulation of hypotheses and the bearing of these upon the theory should be indicated. *Post factum* interpretations which will inevitably arise when new and unexpected relationships are discovered should be so stated that the direction of further probative research becomes evident. The conclusions of the research might well include not only a statement of the findings with respect to the initial hypotheses but, when this is in point, an indication of the order of observations needed to test anew the further implications of the investigation. Formal derivation of this character has had a salutary effect in psychology and economics, leading, in the

<sup>26</sup> Stuart A. Rice comments on this tendency in public opinion research (see *Eleven Twenty-six: A Decade of Social Science Research*, ed. Louis Wirth [Chicago: University of Chicago Press, 1940], p. 167).

<sup>27</sup> It is this practice to which E. Ronald Walker refers, in the field of economics, as “theoretic blight” (*From Economic Theory to Policy* [Chicago: University of Chicago Press, 1943], chap. iv).

<sup>28</sup> To be sure, these conventions are deduction and induction, respectively. Our sole interest at this point is to translate these logical procedures into terms appropriate to current sociological theory and research.

one case, to sequential experiments<sup>29</sup> and, in the other, to an articulated series of investigations. One consequence of such formalization is that it serves as a control over the introduction of unrelated, undisciplined, and diffuse interpretations. It does not impose upon the reader the task of ferreting out the relations between the interpretations embodied in the text.<sup>30</sup> Above all, it prepares the way for consecutive and cumulative research rather than a buckshot array of dispersed investigations.

The correlative process which seems called for is that which Lazarsfeld terms "codification." Whereas formal derivation focuses our attention upon the implications of a theory, codification seeks to systematize available empirical generalizations in *apparently different* spheres of behavior. Rather than permitting such "separate" empirical findings to lie fallow or to be referred to distinctive areas of behavior, the deliberate attempt to institute relevant provisional hypotheses promises to extend ex-

isting theory, subject to further empirical inquiry. Thus, an abundance of empirical findings in such fields as propaganda and public opinion, reactions to unemployment, and family responses to crises suggest that when persons are confronted with an "objective stimulus-pattern" which would be expected to elicit responses counter to their "initial predispositions," their actual behavior can be more successfully predicted on the basis of predispositions than of the stimulus-pattern. This is implied by "boomerang effects" in propaganda,<sup>31</sup> by findings on adjustive and maladjustive responses to unemployment,<sup>32</sup> and by research on the stability of families confronted with severe reductions in income.<sup>33</sup> A codified formulation, even as crude as this, gives rise to theoretic problems which would be readily overlooked if the several empirical findings were not re-examined within a single context. It is submitted that codification, as a procedure complementing the formal derivation of hypotheses to be tested, will facilitate the co-development of viable sociological theory and pertinent empirical research.

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<sup>29</sup> The work of Clark Hull and associates is pre-eminent in this respect (see, e.g., Hull, *Principles of Behavior* [New York: D. Appleton-Century Co., 1943]). See also comparable efforts toward formalization in the writings of Kurt Lewin (e.g., Kurt Lewin, Ronald Lippitt, and S. K. Escalona, *Studies in Topological and Vector Psychology I* ["University of Iowa Studies in Child Welfare," Vol. XVI (Iowa City, 1940)], pp. 9-42).

<sup>30</sup> A book such as John Dollard's *Caste and Class in a Southern Town* teems with suggestiveness, but it is an enormous task for the reader to work out explicitly the theoretic problems which are being attacked, the interpretive variables, and the implicit assumptions of the interpretations. Yet all this needs to be done if a sequence of studies building upon Dollard's work is proposed.

<sup>31</sup> Paul F. Lazarsfeld and Robert K. Merton, "Studies in Radio and Film Propaganda," *Transactions of the New York Academy of Sciences, Series II*, VI (1943), 58-79.

<sup>32</sup> O. M. Hall, "Attitudes and Unemployment," *Archives of Psychology*, No. 165 (March, 1934); E. W. Bakke, *The Unemployed Worker* (New Haven: Yale University Press, 1940).

<sup>33</sup> Mirra Komarovsky, *The Unemployed Man and His Family* (New York: Dryden Press, 1940); R. C. Angell, *The Family Encounters the Depression* (New York: Charles Scribner's Sons, 1936).