This article discusses causal analysis as a paradigm for explanation in sociology. It begins with a detailed analysis of causality statements in Durkheim's Le suicide. *It then discusses the history of causality assumptions in sociological writing since the 1930s, with brief remarks about the related discipline of econometrics. The author locates the origins of causal argument in a generation of brilliant and brash young sociologists with a model and a mission and then briefly considers the history of causality concepts in modern philosophy. The article closes with reflections on the problems created for sociology by the presumption that causal accounting is the epitome of explanation within the discipline. It is argued that sociology should spend more effort on (and should better reward) descriptive work.*

The Causal Devolution

ANDREW ABBOTT
University of Chicago

When we are all dead and forgotten, some scholar will sit in a quiet office at the end of a weary afternoon and gaze in perplexity at his notes on the sociological journals of the 1960s, 1970s, and 1980s. He wonders just what it was that sociologists believed. He writes,

> The people who called themselves sociologists believed that society looked the way it did because social forces and properties did things to other social forces and properties. Sometimes the forces and properties were individual characteristics like race and gender, sometimes they were truly social properties like population density or social disorganization. Sociologists called these forces and properties "variables." Hypothesizing which of these variables affected which others was called "causal analysis." The relation between variables (what these

---

AUTHOR'S NOTE: *I would like to thank the Warden and Fellows of Nuffield College in the University of Oxford for the award of a Norman Chester Fellowship, during whose tenure I drafted this article. John Goldthorpe, Robert Sampson, and two referees provided helpful comments. I would also like to thank audiences at the Institutt for Sosiologi at the University of Oslo and the Departments of Statistics and Sociology at the University of Washington for stimulating comments. The article was written for the latter university's series of Lectures on Causality in the Social Sciences, in honor of Herbert Costner.*

SOCIOLOGICAL METHODS & RESEARCH, Vol. 27 No. 2, November 1998 148-181

> sociologists called the "model") was taken as forcible, determining. In this view, narratives of human actions might provide "mechanisms" that justified proposing a model, but what made social science *science* was the discovery of these "causal relationships."

Can this really have been what sociologists thought? He wonders . . .

Luckily, of course, this future moment has not arrived. We ourselves understand all the hidden things we mean by the idea of causality, the things that are not in fact apparent in our articles. We know quite well, for example, that the causal view just described is not the view of the classical social theorists. We know that this version of causalism is rather the vernacular social theory implicit in the methods courses that we all take within a year of arriving at graduate school, a set of things we come to take for granted when we apply standard empirical methods.

Initially, of course, we all remember the caveats. Regression relationships, as our instructors told us and as we tell our students, are the mere entails of real social action. Action is reality. But familiarity and practice send the caveats packing. Economics articles, it is true, invoke formal action theories before settling into the comfortable technology of linear modeling. But in the other empirical social sciences, an article's "theory" comprises only a few narratives of "possible mechanisms." Either way, action and contingency disappear into the magician's hat of variable-based causality, where they hide during the analysis, only to be reproduced with a flourish in the article's closing paragraphs.

As a result, our empirical work would not be read by someone like my future historian as grounded in the immediate social reality of action, the way it seems grounded to us. To him, we live within a view of social reality that we ourselves do not really believe. Our theoretical hearts are one place, our empirical heads another.

In considering this disjunction, I begin by examining a classical discussion of causality in sociology, that of Emile Durkheim's *Suicide*. I then review the history of causality in sociological methodology and focus on the practical aims behind the movement to causal analysis in the 1950s and 1960s. Contextualizing those aims leads me to the philosophical literature on causality and explanation. Breaking the usually assumed link between causality and explanation, I then return to sociology and recommendations for the future of methodology.

I should say a word about tone. Some readers of this article have felt it to be overargued. There are many neighborhoods in the city of sociology, they say, enough room for causality and anticausality; there is no need to crush causal argument. Others—causalists—have a simpler reaction; we knew all this already.

In the years I have engaged in methodological debates, I have grown used to both arguments: One thinks you have said too much, another that you have said nothing at all. To the "many mansions" argument, I respond that philosophically careless research practice is common in most of sociology's neighborhoods, from historical sociology to medical sociology to stratification research. I have nothing but admiration for the creative audacity that many years ago brought causalism into sociology in the first place. But that an unthinking causalism today pervades our journals and limits our research is an empirical fact. Of course there is room for many kinds of good work. But there should be no room for thoughtless work.

To the argument that we know all this already, there is no response. Home truths are always old truths. That is no reason to avoid saying them, particularly if research practice ignores them. In any case, I intend this piece to be not anticausal but reflective; to stand back from something that has become our common sense and ask why it has become so.

CAUSALITY IN THE CLASSICS: DURKHEIM

I begin with Durkheim. Not that his direct influence on images of causality was great. Untranslated until 1951, *Le suicide* (1897) assumed paradigmatic status only as the causal revolution gained momentum in the following decade. But, by now, three generations of sociologists have taken *Le suicide* as a sacred text, and the book's great clarity makes both its virtues and its vices singularly accessible. It raises, at one point or another, nearly every important problem in the theory of causality.

In *Le suicide*, Durkheim insists from the start that causality embodies a kind of "forcing"—a determination—that is like the causality of classical mechanics. Thus, in the preface,

> When each people is seen to have its own suicide-rate . . .; when it appears that . . . marriage, divorce, the family, religious society, the army, etc., affect it in accordance with definite laws . . . these states and institutions will no longer be regarded as simply characterless, ineffective ideological instruments. Rather they will be felt to be real, living, active forces, which, because of the way they determine the individual, prove their independence of him. (1951:38-39)

Causality here means "living, active forces." It means determination. It means necessary and sufficient reason. Although Durkheim will later rely on more indirect types of causation, the ideal of social causality that he expresses at the outset is almost mechanical.

This mechanical image pervades Durkheim's writing on causality. His argument against cause by imitation rests essentially on a rejection of "action at a distance," the bugbear of classical physics. In the theory of imitation, he says,

> A cough, a dance-motion, a homicidal impulse may be transferred from one person to another even though there is only chance and temporary contact between them. *They need have no intellectual or moral community between them nor exchange services, nor even speak the same language, nor are they more related after the transfer than before.* (1951:123; emphasis added)

Imitation is thus rejected as a causal mechanism because it rests on a cognitive connection that is incapable of transmitting real influence between individuals; that is, because it constitutes action at a (social) distance. For Durkheim, it is the *sharing* of something—in particular norms—that enables the passage of true causal force from one actor to another. Norms are an ether that is necessary to explain how social causality moves across seeming voids.

But Durkheim's approach at other times reflects less 19th-century physics than 19th-century medicine. Doctors then separated the causes of diseases into three layers: predisposing causes, precipitating (or "exciting") causes, and anatomical causes. Predisposing causes made people differentially likely to acquire certain diseases; certain climates were thought to affect lung diseases, for example, and excessive work was thought conducive to insanity. By contrast, precipitating causes "tripped the switch" in some of these predisposed people, thereby starting a disease. Alcoholism might trigger epilepsy, or "disappointed affection" might trigger mania. Anatomical causes then produced the final common pathways in disease; they were the physical lesions that created the symptoms.

Much of Durkheim's analysis of the social origins of suicide fits the predisposing cause model, and indeed Durkheim used that very word to specify his model:

> Chaque société est predisposée à fournir un contingent determiné des morts volòntaires. Cette predisposition peut donc être le sujet d'une étude speciale et qui ressortit à la sociologie. (1897:15)[1]

Here, we seem to follow the doctors' model precisely; social forces make certain things more likely, whereas individual, contingent processes take care of the actual outcomes. But Durkheim has something more in mind, as we see in his discussion of heredity, the predisposing cause par excellence of late-19th-century medicine:

> When suicide is said to be hereditary is it meant merely that the children of suicides by inheriting their parents' disposition are inclined in like circumstances to behave like them? In this sense the proposition is incontestable but without bearing, for then it is not suicide that is hereditary; what is transmitted is simply a certain general temperament which, in a given case, may predispose persons to the act but without forcing them, and is therefore not a sufficient explanation of their determination. (1951:93)

Durkheim emphatically rejects heredity as a cause of suicide, and precisely for its merely predispositional effects. The social causes that Durkheim will later introduce will be seen not as predisposing but as precipitating causes, by which Durkheim clearly means forces of determination, forces of joint sufficiency and necessity.

At times, Durkheim even seems to argue that social causes are alternatives to individual ones. Hence,

> No description, however good, of particular cases will ever tell us which ones have sociological character. If one wants to know the several tributaries of suicide as a collective phenomenon one must regard it in its collective form, that is, through statistical data, from the start. (1951:148)

But in the last analysis, his approach does make the social causes a general framework within which individual forces exercise specific effects. Thus, he says that

> [records of presumptive motives of suicides] apparently show us the immediate antecedents of different suicides; and is it not good methodology for understanding the phenomenon we are studying to seek first its *nearest causes*, and then retrace our steps further in the series of phenomena if it appears needful? (1951:148; emphasis added)

The immediate motives—which Durkheim is at pains to dismiss as what we would call intervening variables—thus simply ring changes on tendencies already established by larger forces.

Durkheim seems then to take a hybrid approach, a model of causality associated less with the physical or medical sciences than with the social sciences themselves. This is the model we social scientists are raised on, in which social forces directly determine underlying parameters, and individual cases then vary around them in response to local causality. We might call this the analysis of variance (ANOVA) model of causality, after the principal method embodying it. It is easy to point to passages in which Durkheim takes this ANOVA approach. Thus,

> Certainly many of the individual conditions [i.e., causes] are not general enough to affect the relation between the total number of voluntary deaths and the population. They may perhaps make this or that separate individual kill himself, but not give society as a whole a greater or lesser tendency to suicide. (1951:51)

Durkheim explicitly separates the two "levels" of causality:

> We do not accordingly intend to make as nearly complete an inventory as possible of all the conditions affecting the origin of individual suicides, but merely to examine those on which the definite fact that

> we have called the social suicide rate depends. The two questions are obviously quite distinct, whatever relation may nevertheless exist between them. . . . The [sociologist] studies the causes capable of affecting not separate individuals but the group. (1951:51)

The fundamental difference between this position and the predisposing/precipitating model is that Durkheim, like other social scientists, wants to think of the social causes as determining, necessitous forces, not just as general probabilistic drifts. The general/particular causal distinction is made, but the location of force is changed. Whereas the 19th-century doctors often found the higher level (predisposing causes) unchangeable and hence of merely academic interest, Durkheim's interest lies in precisely those higher level (in his case, social) causes. For him, it is the immediate causes that are uninteresting.

It helps that data on these immediate causes—the so-called motives or immediate stimuli for suicide—are notoriously poor. Nonetheless, Durkheim takes the time to reject them. First, he presents statistics showing that overall suicide rates have risen sharply while these motives have not changed much at all. Then, he presents statistics showing motives to be roughly the same for farmers and liberal professionals, when "actually the forces impelling the farm laborer and the cultivated man of the city to suicide are widely different" (1951:151). He puts the matter bluntly:

> The reasons ascribed for suicide, therefore, or those to which the suicide himself ascribes his act, are usually only apparent causes. . . . They may be said to indicate the individual's weak points, where the outside current bearing the impulse to self-destruction most easily finds introduction. But they are no part of this current itself and consequently cannot help us to understand it. (1951:148)

Here, Durkheim explicitly turns the predisposing/precipitating model on its head. Individual factors are now the predisposing ones; social-level factors are the exciting, effective, forceful causes.

The overall design of the great monograph that embodies this conceptualization of causality is familiar. After rejections of nonsocial arguments, Durkheim briefly considers what we would now call a descriptive or typological analysis—classifying suicides into categories and then analyzing them category by category. But here he finds too many diversities, too much missing data, too many errors in recording motivation. Better to reject this material altogether and pursue a purely social, causal analysis of suicide. In a central passage, he professes his faith:

> Only in so far as the effective causes differ can there be different types of suicide. For each to have its own nature, it must also have special conditions of existence. The same antecedent or group of antecedents cannot sometimes produce one result and sometimes another, for, if so, the difference of the second from the first would itself be without cause, which would contradict the principle of causality. Every proved specific difference between causes therefore implies a similar difference between effects. Consequently, we shall be able to determine the social types of suicide by classifying them not directly by their preliminary described characteristics, but by the causes which produce them. (1951:146-47)[2]

This "classification by causes" governs the body of Book II, which closes with a final summary of the ANOVA view of causality:

> Such are the general characteristics of suicide, that is, those which result directly from social causes. Individualized in particular cases, they are complicated by various nuances depending on the personal temperament of the victim and the special circumstances in which he finds himself. But beneath the variety of combinations thus produced, these fundamental forms are always discoverable. (1951:294)

One can easily see why this book became the paradigmatic text of modern causal analysis, despite Durkheim's primitive statistical techniques. It is self-consciously scientific. It invokes the concept of causality. It combines the classical physicists' mechanism with the doctors' differentiation of local and global cause. It makes causality both universal and unique. And it lays out a model of causality that prefigures, almost word for word, the analysis of variance as set forth more than three decades later by Ronald Fisher and his statistical colleagues.

But we must hesitate before accepting this facile judgment, for it reads the present into the past. If we ask what *Durkheim* thought he was doing with all this talk of causality—talk that for us seems the very blazon of contemporary quantitative sociology—the answer is that Durkheim saw himself in a battle with immanent evolutionists like Spencer and Comte, scholars who saw in the course of events the mere working out of even grander and more universal forces than Durkheim's social powers. *The Rules of the Sociological Method* ([1895] 1964) makes it very clear that Durkheim took up the cudgel of causality in the name of contingency and variation, in the name of the particular against the universal. Odd as it may seem today, he thought he was urging the importance of real history, of actual events. Thus, what seems to us now like the urtext of the sociological causalism of the last 40 years was in fact seen by its author as a manifesto in favor of studying contingent social action over chronicling a unified, transhistorical development.

CAUSALITY AND EMPIRICISM IN SOCIOLOGY

As noted before, Durkheim's present symbolic importance belies what was in fact a negligible historical effect on social science's images of causality. The synthesis of causal analysis with quantitative methods so evident in *Le suicide* in fact became widespread only in the 1950s, around the time *Le suicide* was translated into English and republished by the Free Press. It was only then that the accidental affinity between Durkheim's theories and Fisher's mathematics made *Le suicide* the bible of causal analysis, repeatedly cited by Lazarsfeld (Lazarsfeld and Rosenberg 1955), Stinchcombe (1968), and other developers of modern sociological methodology. The new synthesis of causalism and quantitative analysis in fact arrived by a quite different route.

Quantitative analysis was old in sociology, indeed in the social sciences more broadly, by the 1950s. But early quantitative analysis was neither causal nor inferential in the modern sense of those terms. The revolution of Fisherian statistics swept through the field of statistics itself only in the late 1920s. At that time, quantitative analysis in sociology took various forms—social trends under Ogburn, ecological analysis under the Chicago school, social distance scales under Bogardus—but none of these invoked the new Fisherian orthodoxy, although they did employ the correlational methods of the preceding generation of statisticians.

The new sociological statistics of the 1930s had three broad origins: biometrics, psychometrics, and econometrics. The biometric avenue is both the simplest and the most important. It was the biometricians who created modern inferential statistics between 1890 and 1930. Galton, Pearson, Fisher, Wright, and their colleagues invented correlation coefficients, regression methods, and path analysis. They also devised sampling theory and hypothesis testing, with its presumption of probabilistic reference models.[3]

In the early days, the biometric revolution was somewhat anticausal. The elder Pearson, for example, reduced causality to invariant succession in his celebrated *Grammar of Science* ([1892] 1937: 102-14). To be sure, within the experimental designs characteristic of early biological research, the distinction between causality and association tended to blur. By isolating treatments from the influences of extraneous (we would say spurious) factors, experiments permitted a direct causal inference between treatment change and observed results. But much early statistical work—from that on intelligence to that on inheritance—was in fact conducted in nonexperimental situations and without any real theories of mechanism. It therefore tended to de-emphasize causality and causal explanation. In particular, the main goal of the agricultural work was operational—how best to improve crop yield. Knowledge of mechanism was tangential to such work, which was effectively evaluation research without any theoretical pretensions.

The study of intelligence led to another set of statistical developments, this time within psychology. There, Thorndike, Spearman, and Thurstone developed scale and factor analysis. The psychometricians were even less causally oriented than the biometricians. The psychometricians' factor analysis reduced complex data to simple forms in order to reconcile quantitative data with intuitive categories. It ignored causality altogether. But this was hardly surprising. The major theoretical problem to which factor analytic studies were addressed was the debate over the faculties of the mind—the long-standing concern about whether there were separate "organs" of memory, desire, intellection, and so on. This was not a causal but rather a descriptive problem.

Econometrics provided a third entrance for statistics into social science. The earliest econometricians were fairly explicit associationalists. Yule (1912) discussed "causation of pauperism" in the following operational terms in his *Introduction to the Theory of Statistics*:

> When we say, in fact, that any one variable is a factor of pauperism, we mean that changes in that variable are accompanied by changes in the percentage of the population in receipt of relief, either in the same or the reverse direction. (P. 192)

Yule's text contained no index reference for causality or causation. It was only with the problem of reciprocal influence of time series that causal issues became central in econometrics. Writers like Slutsky, Frisch, and Tinbergen all spoke of their work as involving causes, but in all three cases causation essentially reduced to uniform association, as it did in the influential program document of Haavelmo (1944) on probability in econometrics. Issues of causation proper (issues going beyond association) arose in econometrics as questions of reversibility and direction, although lagged paths—characteristic from Tinbergen onward—took care of many such problems. Some economists urged the separation of a theoretical level from a statistical one (as Keynes did in criticizing Tinbergen; see Morgan 1990:121 ff), arguing that causal ideas could come only from theory. Others believed causality could be analyzed purely at the data level; the problem of specifying direction was in fact Wold's reason (from the late 1940s onward) for preferring the recursively structured path analytic paradigm (Morgan 1990:255). In the main, however, defining cause simply as uniform association was standard throughout the econometric literature, particularly in its simultaneous equations branch. Econometricians tended to use the language of cause more often than did the other new statisticians, but by it they meant only relationship or association.[4]

Thus, the three main strands by which the new statistics emerged and entered the social sciences were somewhat mixed in their attitudes toward cause. To the extent that the biometricians and econometricians talked about causality, it was identified with invariant relationships or associations. For their part, the psychometricians did not care much about causality at all. This muted role for causality had also been characteristic throughout the quantitative sociology of the 1920s and 1930s. William Fielding Ogburn said that his Ph.D. thesis was "not a study to determine causes, but it is hoped that it may be used as a basis for a study of causes" (cited in Bernert 1983:237).

In sociology, it was by contrast the old-style qualitative theorists who wanted to talk of causality. Thus, MacIver's *Social Causation* (1942) attacked mathematicization precisely for losing sight of causality in a haze of associations. Much of MacIver's anger was directed toward what he called the "mathematical limbo" of the more extreme versions of logical positivism: for example, Morris Cohen's claim that "mathematical and logical relations form the intelligible substance of things" (MacIver 1942:49, 53). The extreme positivists in sociology—George Lundberg being the most vociferous—had argued that the concept of causality was anthropomorphic and "theological"; only association could be observed, never force or compulsion. Indeed, the program of the early socio-logical-postivists included not only this extreme anticausalism but an equally extreme operationalism that they had taken from the physicist Percy Bridgman (MacIver 1942:157).

The concept of causality with which MacIver attacked these positivists was in fact closer to our concept of explanation than of causality. Causal assessment, he said, means seeking answers to the question "Why does something or some regularity happen?" Like Aristotle, he gave a number of different kinds of why questions, suggesting a number of generic types of causes. It is plain that the word *causality* meant for him something fundamentally different than it did for Cohen, Lundberg, and the like. Nor was MacIver the only such voice in sociology. In his principal methodological work (1934), Znaniecki saw determined, causal processes as one of three basic types of social processes (the others being ontogenetic and phylogenetic processes) and made the Whewellian argument that causes could be discovered by analytic induction. Thus, World War II found qualitative, nonstatistical sociologists talking about causality and action, whereas quantitative, statistical sociologists focused mainly on association and were skeptical of causation. Yet, after the war, the language of causality quietly drifted back into quantitative sociology. Causality reappeared as part of Lazarsfeld's gentling of the harshly scientistic paradigm associated with the social physics of Lundberg, Dodd, Zipf, and others. The Lazarsfeld and Rosenberg (1955) reader on *The Language of Social Research* established the modern concept of methodology (not least by consciously choosing that word; Lazarsfeld and Rosenberg 1955:4) and made the investigation of causes central to that methodological process, citing MacIver alongside Lundberg, Dodd, and Durkheim, and indeed using MacIver's term "causal assessment."

But the old skepticism remained. In *The Language of Social Research's* main section on multivariate analysis, the analyst is said to be pursuing "explanation," not causal assessment. Explanation here means discovering general regularities, whereas causal assessment means "applying available knowledge to the understanding of a specific case, be it a person or a collective" (1955:387). Lazarsfeld and Rosenberg gave examples of such causal assessment in a section on empirical analysis of action, focusing on a favorite example, the process of purchasing a good. "Any bit of action," they tell us, "is determined on the one hand by the total make-up of the person at the moment, and on the other hand, by the total situation in which he finds himself" (1955:393). In this formulation—which could easily have come from Herbert Blumer's symbolic interactionism—Lazarsfeld and Rosenberg view causality as a way of understanding and explaining particular action in particular settings.

The Lazarsfeld/Rosenberg skepticism about causality was echoed in Stouffer's (1957) comprehensive review of quantitative methods. With surprising prescience, Stouffer noted the potential of game theory, decision theory, and stochastic processes for the future of sociology. He also covered recent developments in sampling, item scaling, experimental design, and official statistics. Multiple regression, in its infancy as a sociological method, rated a brief mention before much longer sections on factor analysis and significance statistics. But causality simply was not there. Even in Zetterberg's (1954) sketch of the scientific paradigm of sociology, causality played a very quiet second fiddle to explanation. Zetterberg's emphasis on axiomatic theory and deductive theory testing clearly derived from the logical account of science found in Cohen and Nagel (1934) and Hempel (1942). Like Stouffer, he was deeply concerned with what we might call the semantics of research: the separation of theoretical and operational variables, the validity of definitions, and the reliability of measures. Causality—a matter of syntax—was mentioned only briefly, in the discussion of longitudinal designs, and in fact was derided as a term "on the common sense level" (Zetterberg 1954:60).

But there were distinct changes in the second and third editions of Zetterberg's scientific manifesto. By 1965, he was remarking that "if the 1950's were particularly hospitable to taxonomies and descriptive studies, the 1960's seems more hospitable to theories and verificational studies" (p. 29). He introduced an extensive section on propositions in sociology, in which cause figures centrally:

> When we know or assume the direction in which the variates influence each other, we can designate one as a *determinant* (cause or independent variable) and the other as a *result* (effect or dependent variable). (P. 64)

This statement seemed to follow the econometricians' acceptance of causality as simply invariant relation interpreted by a theoretical judgment about direction. But Zetterberg (1965) included a lengthy analysis of varieties of linkage between determinants and results, discussing reversible and irreversible relations, deterministic and stochastic relations, sequential and coextensive relations, sufficient and contingent relations, and necessary and substitutable relations.

Sociology seemed to have passed a watershed. By the 1972 edition of the Lazarsfeld and Rosenberg reader, the whole picture had changed. The lead article of the book's section on multivariate analysis was a Hirschi and Selvin chapter on principles of causal analysis. The whole section on empirical analysis of action had disappeared (along with any coverage of qualitative research). Denzin's (1970) reader on *Sociological Methods*, although reprinting Turner's 1953 elaboration of Znaniecki's analytical induction, reprinted another chapter from Hirschi and Selvin (1967), an attack on false criteria of delinquency. This chapter treated the associationalist conception as the only tenable possibility for a concept of causation but assigned to association a quality of forcing or determination.[5]

The watershed, of course, was the implementation of path analysis. Path analysis had a long but subterranean history. Invented by Wright in the 1910s (the first full published exposition was Wright 1921), it was applied by Wright to problems in population genetics and econometrics. Although Wright used it widely and was followed by a few others in population genetics, path analysis remained generally unknown until the 1950s. The great statistician Tukey (1954) wondered "why I had not known about it before" (p. 35), and even as late as 1972 Goldberger argued that "statisticians have generally ignored Wright's work" (p. 988).

Although Wright had worked in economics and had provided path analyses for his father's influential 1927 book on tariffs, economists too more or less ignored his work. Indeed, his Chicago friend and economist colleague Henry Schultz knew of the methods but did not use them (Morgan 1990:178; Goldberger 1972). As a result, economists routinely reinvented methods Wright had already developed (e.g., Tinbergen's "arrow scheme" of 1936; see Morgan 1990:118). It was Wold who around 1950 began explicitly to reintroduce path analysis to economics. Simon (1953, 1954) (another member of the Cowles Commission circle at Chicago in the early 1950s) also supported path analysis, arguing that previously unreflective causality concepts in economics (e.g., in Haavelmo's celebrated 1944 article) should be placed on a firm philosophical foundation (1953:66 n. 6). It is noteworthy that causality in this work was still defined in a very limited fashion. Wold (1964) defined it in rigidly operational terms, completely separating it from ideas of determination and forcing in his concept of a "generalized stimulus-response definition of causation" (p. 274). Simon (1953) likewise defined causal ordering in pure set-theoretic terms as the partial ordering of minimal subsets of variables.

Path analysis entered sociology through Duncan and Blalock. In 1964, Blalock's *Causal Inferences in Non-Experimental Research* looked back to Wold and Simon. Reversing the positions of the 1940s and 1950s, Blalock treated causality as intrinsically linked with quantitative analysis and with the analysis of general, not particular, phenomena. The opening pages of his book tell us just how dominant the causal model had become and how it had been redefined, losing the connotations of action that had been associated with it in Durkheim, MacIver, and even the early Lazarsfeld. There is no mention whatever of action, actors, or intentions. Causation is literally called a "forcing" (p. 8) and is something more than mere constant conjunction or sequence. Like Durkheim, Blalock regards "causal laws" as deterministic in the terms of classical physics (p. 17) but clouded by error, by which he means unspecified causes, individual variability, and so on. By the word *theory*, he means representations of reality in linear transformations. It is clear, in fact, that the philosophical prologue derives logically from the statistical argument that follows it.

Duncan's (1966) use of path analysis did not invoke an explicit concept of cause as determination. He argued for "causal diagrams" that must be "isomorphic with the algebraic and statistical properties of the postulated system of variables" (p. 3), suggesting that the algebraic properties came first. Indeed, he quoted from Wright passages that prestated the celebrated critique Freedman (1987) would later give of misused path analysis:

> "The method of path coefficients is not intended to accomplish the impossible task of deducing causal relations from the values of the correlation coefficients. . . . The method depends on the combination of knowledge of the degrees of correlation amoung the variables in a system with such knowledge as may be possessed of the causal relations." (Duncan 1966:15, quoting Wright 1934:193; 1960:444)

The Blalock (1964) book and Duncan article proved to be citation classics in the early 1970s. Blalock's book and Duncan's article received a total of 115 and 76 citations, respectively, in the quinquennium 1971-1975. Between them, they launched a fleet of path diagrams.

Whatever Duncan's technical impact, his cautious use of causality was eclipsed in the literature by the more determinative concept of Blalock. The new emphasis on the concept of causality hinged on a redefinition; the term *causality* could become important again in part because it had a new meaning. Causality was now seen as a property of mathematical and statistical propositions rather than a property of reality, a fact clear in Blalock's phrasing of discussions of causality specifically in terms of equations and conditional probability, a phrasing that came directly from Simon (1953, 1954; see also 1952) and indeed echoed the long-standing associationalist definition of causality in econometrics.

The shift also paralleled earlier trends in the philosophy of science. The logical positivists had sought to escape from the traditional epistemological problems of empiricism by redefining virtually all the central concepts of science, making them descriptions of scientific language rather than of empirical reality. Causality was one of several terms so treated. Thus, in Cohen and Nagel's (1934) immensely influential *Introduction to Logic and the Scientific Method*, causality was defined as simply a kind of statement—a statement labeling an invariant relationship. Which specific aspect of this invariance was of interest would vary with the theoretical interests of the investigator, a point Blalock (1964:18) was to emphasize as well. Ayer (1946) took the same view in *Language, Truth and Logic*, holding that "every general proposition of the form 'C causes E' is equivalent to a proposition of the form 'whenever C, then E' " (p. 55). The move toward the Durkheimian model of causality was thus justified not on the ontological grounds Durkheim had used but on general philosophical grounds. The new model reinstated the idea of causality but only by making it a predicate of discourse, not reality. At the same time, however, Blalock also quietly reinstated the Durkheimian concept of cause as forceful and determining, and his lead was followed by a generation of students unconcerned with the philosophical niceties that had restrained Duncan and many of the econometricians.

A summary makes this development more clear. For Durkheim, causality meant envisioning social reality as governed by real actions rather than by grand immanent forces. Causes were the local (although to our thinking still emergently social) forces determining these actions. For the early statisticians, causality had seemed of relatively little concern; they were interested in description or outcome analysis rather than in mechanisms. If anything, causality for them meant uniform association. For the logical positivists, causality in the lay sense was an anthropomorphic bugbear to be purged from real science. They were militant supporters of associationism and would accept the concept of causality only if it meant nothing more than association. For MacIver and other nonstatisticians, by contrast, causality seemed the essential heart of explanation in human affairs. Indeed, what they meant by causality was what the others meant by explanation. Even for the early Lazarsfeld, causality meant something about understanding particular human actions. But the new causalism of Blalock and others accepted the causality concept of the logical positivists (causality as a predicate of statements rather than reality and as a concept not referring to action) at the same time reinvesting it with both the quality of emergentism and the character of forcing or determination, both of which are present in Durkheim, although qualified by his sense of causality as tied up with action. As a result, there emerged the full-blown ANOVA concept of causality. Ironically, philosophers are today reading the causal modeling literature for insight into "the nature and structure of probabilistic causation" (Irzik and Meyer 1987).[6]

I need not tell the reader that this view of cause is hegemonic in American sociology, although not, interestingly enough, in psychology or market research. We have ended up believing that social reality is determined in the main by certain general forces, and that these generalities are then specified by combinations of forces, and further limited by various aspects of "individuality," which in this sense is best understood as idiosyncratic higher order interaction. Although we are, as I noted at the outset, very careful to tell ourselves and our students that this is really only the mathematical framework, in practice, a surprising number of sociologists believe, for all intents and purposes, that this is the way the social world itself operates.

My discussion so far offers no account for why the Blalock/Duncan generation took up causal analysis in the form it did and with the vigor it did. There seem to have been a number of reasons for this adoption.

One of these was a real belief in "science" as a stance for sociology. Thus, although Duncan regarded himself as a student of Blumer as much as of Ogburn, it was Blumer's insistence on rigor and science that appealed most strongly to him.[7] Blalock's similar faith in science speaks in page after page of his writing. To be sure, scientism was in the air. Logical positivism was triumphant in philosophy, and science had, in many people's eyes, won World War II. In social science, too, scientific stances seemed particularly strong. Polls were becoming universal. Market research was booming, both in the strongly quantitative Lazarsfeldian variant and in the softer, psychologistic style of Lloyd Warner and Burleigh Gardner. And scientism as an ideology went beyond a simple methodological stance. It was a general commitment, as evidenced by the arrival of double-blind reviewing in sociology journals in the mid-1950s.

With respect to the new methodologies, this belief in science was very much a young man's game. In 1955, Duncan was 34, Blalock and

Coleman 29, Goodman 27. Lazarsfeld was the grand old man at 54, dedicating the 1955 reader to "Charles Glock and his 'young turks' at the Bureau of Applied Social Research." Of course, sociology as a whole was young at the time because of its rapid expansion after the war. But there was a very generational flavor to the causal revolution. Duncan's famous public attack on Warner's "unscientific" categories, written when he was 29 and coauthored with his graduate student colleague Harold Pfautz, was as much a young person's attack on the establishment as it was a blow in a quantitative/qualitative fight (Pfautz and Duncan 1950). Blalock first published his text *Social Statistics* at 34. Coleman's (1964) capaciously quixotic book on mathematical sociology came at 38.

The youthfulness of the new causalism suggests another reason for its adoption, one for which I have a strong hunch but scant evidence. Mullins (1973) saw the new causalists as Cromwellian puritans. But I think, by contrast, that the leaders of the new causalism were a little like Hilary climbing Everest. They did it "because it was there." The methods had been worked out by others. They could be quickly borrowed and applied. Trying them out would be easy. Who knew what might result? The young turks did it, in short, for fun.

We get this "let's try it out" feeling again and again in Coleman's (1964) *Introduction to Mathematical Sociology*, as we do even at times in *The American Occupational Structure* (Blau and Duncan 1967). Duncan was explicit in that book about the extreme assumptions necessary for the analysis, but repeatedly urged the reader to bear with him while he tried something out to see what could be learned. The memoirs one reads of Columbia in the early days of the Bureau of Applied Social Research all suggest a similar mood of experimentation—often contemptuous of outsiders but lightheaded and giddy and energetic as only young people with a new truth can be.

To be sure, this try-it-out attitude was combined with the scientific puritanism Mullins so disliked. More consequentially, it was also combined with extraordinary exclusivity, for few senior sociologists could follow the mathematics necessary to undertake causal analysis on their own, and computerized commodity statistics were not yet available. As a result, the young causalists were rulers of a roost no one else could attack at an age when in more typical careers they would have been chafing for a decade or more under their elders' tutelage. It is little wonder that as the years passed they began to take their own creation with such remorseless seriousness.

This seriousness was passed on to their students, who had not had the experience of inventing causalism and therefore could not know at first hand its contingent, historical character. Rather, they learned causalism in methods courses as the way science was done, and, within a few years, statistical packages would prevent their acquiring that tacit knowledge and sense of respect for the methods that comes with having worked them by hand. Duncan passed on to his students neither his desire for what he once called "a properly relativistic sociology" nor the comprehensive vision of social life that he had gotten from Blumer. He finished his career decrying nearly everything that people had done in following him (Duncan 1984). The causalists won all the battles but lost the war.[8]

What was the war about? It was about providing a compelling and interesting account of social life. Causalism was a tactical avenue to that larger strategic aim. The great mistake of the causalists, in Duncan's eyes, was to have mistaken the tactical victories of "doing the science right" for the strategic victory of getting a better account of social life. To put it into the familiar terms of causalism itself, the mistake was to have taken the indicator for the concept. This losing sight of the main objective is what I mean by the title of this article—the causal devolution.

But if the mistake was to believe that a fairly narrow causality concept was the only way to undertake the explanation of social life, what are the alternatives? Here, it is helpful to turn to the philosophical literature.

PHILOSOPHY, CAUSALITY, AND EXPLANATION

The ANOVA view of causality common in social science in fact bears little relation to developments in the analysis of causality by philosophers. The classical analyses are those of Aristotle and Hume. With characteristic good sense, Aristotle noted that people meant a variety of things by "cause." Today, each of his four causes supports a major theoretical strand in social science. Material causes are studied by demographers, who believe that the explanations of social phenomena lie in the different qualities of the human materials going into them. Formal causes are studied by structuralists, who see in networks and patterns the determining shapes of human affairs. Final causes are studied by functionalists, with their interest in the purposes and ends of action. And efficient causes are the focus for choice modelers, who seek the final pathways by which action is determined. In most empirical social science, the first three (at least) are mixed together, although recently the last is entering the mix almost as often. Aristotle's notion of cause is thus a broad one, covering most of our activity as social scientists. Like MacIver's notion in the current century, his concept comes closer to our idea of explanation than to our idea of cause.

The Humean analysis is more specific. As is well known, Hume directly attacked the notion of causality as a "forcing" of things to happen and as a necessary relationship in the real world. For Hume, causality was a simple matter of invariable sequence or constant conjunction. Cause and effect had to be both adjacent and temporally successive, and the relation between them had to be constant. But the necessity of it was purely in the mind of the beholder, for necessity could not be directly perceived. As the logical positivists were later to argue, causation denoted a kind of statement rather than a kind of relationship between things. Even Kant could rescue causality from Hume's attack only by making it one of the categories of the pure reason, an a priori aspect of knowing. On this point, Durkheim—and with him most social, indeed, most natural, scientists—completely ignored the Western philosophical tradition, for he took forcing and determination as central to his concept of cause.

Since Hume, the modern philosophy of causality has divided over a number of issues. Central among them are the following:

1. Singular versus multiple causes
2. Necessity versus sufficiency as the logical mode of causality
3. Rational action versus mechanical determination as paradigms for causality
4. Causality as simultaneous versus causality as sequential
5. Determinant versus probabilistic causality.

I cannot review this literature—vast, controversial, and inconclusive—in so brief a space. But it is important to note that the social science notion of causality embodies one particular position within these inconclusive debates. It has generally made determination rather than action paradigmatic and has always presumed sequentiality: Cause must precede effect. Problems 1, 2, and 5 have been resolved by a probabilistic version of the position of Mackie (1974) on plural causality. Mackie identified causality with what he called INUS conditions: "insufficient but nonredundant parts of an unnecessary but sufficient condition." The Mackie concept more or less justifies our common practice of apportioning causality in pieces: telling the public—or at least as they hear it—that criminality is 65 percent due to heredity, or intelligence 35 percent due to nurture, or whatever. The fact that the causes are insufficient in themselves covers us if they do not work separately. The fact that they are nonredundant parts of something covers our obvious assumption of plural causality. That they add up to something that is sufficient rather than necessary gets us out of directional difficulties. (For a review, see Marini and Singer 1988.)

The philosophical literature, however, remains deeply divided on most issues of causality. Indeed, the greatest modern review of the practical problem of causality—Hart and Honore's (1985) *Causation in the Law*—is deliberately, maddeningly catholic in its conceptions of cause. But the book does make it clear that from a legal and practical point of view, the question of causality always arises as part of the question of explanation. And about explanation there is a separate and equally complex philosophical literature.

The modern philosophy of explanation rests on Hempel's celebrated argument—set forth at the high water mark of logical positivism in 1942—that explanation of particular events always takes the form of a syllogism whose major premise is a "covering law" and whose minor premise is an assertion that a particular situation meets the hypothesis conditions of that law. Thus, in Durkheim's case, sociology provided the theoretical major premise whereby certain social conditions entail certain necessary consequences: lack of social integration causes high suicide rates. The minor premise was the demonstration that a particular case fit the hypothesis of the major premise: Saxony lacked strict religion and hence lacked social integration. The syllogism then produced the inescapable conclusion that Saxony had a high suicide rate. Thus, sociology "explained" that suicide rate.

Over the years, Hempel's argument has drawn considerable opposition. From the outset, Popper ([1943] 1962, chap. 25) argued that all social covering laws are trivial, the classic example being that "sane persons as a rule act more or less rationally" (2:265). For Popper, to invoke such a law was simply to restate the problem, not to explain anything. All the real explaining was done by side conditions that specified which covering laws hold, that is, which of a set of plural causes were doing the explaining and which were simply assumed in setting up the problem.

But the larger response to Hempel came from philosophers of history, who proposed a completely alternative view of explanation, based on narrative. There were three general versions. The first was the "understanding" model of Collingwood (1946), Dray (1957), and many others, which deals to some extent with Popper's issue of side conditions. According to Collingwood, the historian aims to get inside a historical figure's own justification of action to understand what was "reasonable" given that figure's tastes and conditions. The Collingwood position was thus a broad version of what would come to be known as rational choice theory; the historian figured out "what it made sense for the actor to do" given the actor's beliefs, knowledge, and psychology. This approach correlated with Collingwood's position that rational, intentional action was the paradigm of causality.

The understanding view was, however, seen by later philosophers of history as overly "idealist," given to dangerous subjectivism. A second view of narrative explanation, responding to this challenge, was the "followability" thesis of Gallie (1968). Like Collingwood's rational constructionism, this view attempted to describe how narrative history actually worked. On this argument, narrative was itself explanatory by virtue of truth, consistent chronology, and a coherent central subject. Narrative was held to combine things that are determined by general laws with things that are contingent, producing a plausible, because followable, story. This notion of "combination" was much looser than the formalities of the covering law model but still left a place for general determinism that is missing in Collingwood.

A third position on historical explanation recognized a central problem in Gallie's followability view—the fact that knowing how the story turns out is central to a historian's "following the story." Mink (1970), for example, argued that history was one of three basic modes of thinking about the world: theoretical (the view of the natural sciences), categoreal (the view of philosophers), and configurational (the view of historians). What made configurational thinking unique was its insistence on putting particular pieces together into larger wholes. This was the process that Walsh (1958) and others had called *colligation*: the assertion that a group of conflicts should be collectively defined as a social movement, for example, or that a certain group of composers made up a school or a style. Conceived across time, colligation became the process of creating "configurations"—that is, histories or plots—of events.[9]

The literature on explanation in the philosophy of history can be seen in hindsight to have been part of the long tradition of "two cultures" positions reaching back to the *Methodenstreit* of the late 19th century. A fairly broad range of philosophers has argued that explanation in the human sciences is different from that in the natural sciences. The early statements of this position tended to be exclusionary (Windelband, Dilthey), but more recent ones have tried to suggest ways of merging interpretive and causal explanation (e.g., von Wright 1971, chap. 4). This philosophical literature thus suggests that sociological views of explanation could benefit from frank discussion of whether and how it is possible to combine interpretive (intentional, teleological, etc.) explanation with causal (determinative, nomic, etc.) explanation. (By these multiple terms, I do not mean to imply that all these distinctions can be simply conflated; rather, that the issue of combining the two sides arises no matter how we happen to divide the two cultures.) The various concepts of explanation in the philosophy of history are examples of how we might proceed.

Most of the literature on interpretive explanation regards the issue of descriptions of events as both central and problematic in the activity of explanation. Before we can explain, we must describe (e.g., see von Wright 1971:135). In particular, as Hart and Honore (1985:29) argue, we generally seek to explain things when they deviate from some recognizedly normal state of affairs. For them, this "deviation from the normal" is as important a part of our idea of causality as is regular association. Such deviation is most obvious in our explanations of action, of course, but it appears as well in the simplest causal models in econometrics or the natural sciences; they all assume that the effects would not have arisen absent the cause. Thus, not only does explanation presuppose description, it also presupposes description from a point of view.

As the analytic philosophy of history shows, it is important to realize that this point of view—and the description that results—is inevitably "narrative" in the sense of involving more than one point in time (cf. Danto 1985:143 ff). For it is usually later events that define what were the salient causal aspects of a prior situation, that tell us what part of the description was important. To give an example Ruben (1990:105) quotes from Mackie (1974), we do not know until Oedipus's parentage is revealed that what matters about his marriage is not that it is with the most beautiful woman in Thebes but rather that it is with his mother.

A final useful insight from the philosophy of explanation concerns our reasons for seeking explanation. Hart and Honore's central aim in discussing causality and the law is to analyze the connection between our beliefs about causality and our attributions of responsibility. Now as it happens, despite the great dominance of scientific causalism in American sociological rhetoric, a central part of most published American sociology today is the attribution of responsibility. Certainly, this is true in my own substantive field of studies of work and occupations, where attributions of responsibility for inequality are central to a majority of studies (see Abbott 1993). That being the case, sociology is deeply in need of careful reflection about the relation between causal analysis and attribution of responsibility. I am unaware of any such discussion. In this area, we have much to learn.

CAUSAL AND CONTINGENT VIEWS OF SOCIAL REALITY

How can views of explanation that derive from interpretive philosophy and the philosophy of history provide viable alternatives to explanation of social life via our current concept of causality? To be sure, they stand much closer to traditional Western social philosophy and to our modern social theorists than does the ANOVA view of causality that characterizes our quantitative work. They make action central. They mix determined and free acts. They embrace contingency. But to espouse them wholly would surrender the important knowledge we have gained about social determinants produced under the causality paradigm. We must recast our strategies of explanation but without losing what we have gained. To do that, we must begin by asking what we want from explanation.

The main desiderata of explanation are consistency and interest. First, even though disciplines grow in fits and starts—pushing out here, surrendering there—our knowledge becomes great only when it has internal consistency. Our theories, our explanations, our methods, and our research programs should resonate with and support one another. In addition to this consistency, our knowledge of society should meet a second standard: It should produce—as Duncan and others wanted—a comprehensive, interesting, and compelling account of social life. That account should be interesting and compelling not only to us in our specialty but also to the larger culture around us.

It is no secret that sociology at present meets neither the standard of consistency nor that of interest, although it has done so in the past and, with luck, will do so in the future. I have just discussed the profound inconsistency between our abstract theories and the concrete theories implicit in our methods. And surely none of us thinks that sociology has, at present, a publicly compelling account of social life.

I shall make one point about compelling public interest before closing the article with my main topic of consistency. One central reason for sociology's disappearance from the public mind has been our contempt for description. The public wants description, but we have despised it. Focusing on causality alone, we refuse to publish articles of pure description, even if that description be quantitatively sophisticated and substantively important. Commercial firms pay millions for such work. Our society is, in fact, "described" in surpassing detail by proprietary market research. But we who like to imagine ourselves responsible for the public's knowledge of society despise description and indeed despise the methods that are generally used for quantitative description. Our social indicators are simply disaggregated variables, ready for input to causal analysis. The notions of complex combinatoric description, of typologies based on multiple variables—these fill the average sociologist with disgust.

Our disgust is disingenuous, for ease of computing has made regression itself a descriptive method. When dozens of regressions can be run in an afternoon and when the average regression-based journal article reports perhaps 5 to 10 percent of the runs actually done, we should stop kidding ourselves about science and hypothesis testing. And taken as a descriptive technique, regression is quite poor. Description aims to reduce a welter of data to something manageable. But regression reduces the dimensionality of the data space only by one. Worse still, that lost dimension usually retains most of its variation. So we have not even understood why that one thing happens. We *have* understood the effects of the independent variables on that one dependent dimension, and in an evaluation context—when we are trying to make decisions about whether to use fertilizer on the field or dopamine in the brain—regression is without question the method of choice.

But as a general method for understanding why society happens the way it does, much less as a strategy for simple description, causally interpreted regression is pretty much a waste of time. Scaling and clustering may throw away the vast majority of dimensionality, but by doing so they often produce compelling and powerful simplifications of complex data. When there are millions of dollars riding on results, the bettors go with descriptive methods, not regression. Market researchers use it to clean up details.

Of course, many of us feel the marketers are fools. Causal analysis is the only true science. Yet, what produced biology's modern understanding of evolutionary trees? Accurate description and numerical taxonomy, known to us as cluster analysis. What has quintupled our ability to find drugs with specific powers? Sequence analysis, a descriptive technique. Most causal discoveries about protein mechanisms are premised on the descriptive geography of proteins produced by the sequence analytic community. Thus, we should not assume that science must be about causality. Much of real science is description. Sociology will not be taken seriously again as a general science of social life until it gets serious about description. (For a similar position resting on a different argument, see Sobel 1996.)

But my more important concerns are with consistency. I have argued that our methods imply theories of society that none of us actually believe. I have implied that we might escape the narrow concept of explanation implicit in our methodologies by employing alternative conceptions of explanation from the philosophy of history. I would like now to trace that path of escape.

When I first turned toward "historical" explanation, I took it as a simple alternative. Instead of breaking social stories down into variables, I would leave them together, comparing and categorizing them as wholes. I would begin with stories as the historians did, but then I would generalize. Thus, I embarked on a 10-year quest for "characteristic plots," looking, for example, at sequences of local professionalization within medicine (Abbott 1991), at careers of individuals across occupations (Abbott and Hrycak 1990), and at patterns in the evolution of the major welfare states (Abbott and DeViney 1992).

Needless to say, this endeavor attracted a lot of hostile press from people who called this work "mere description." I have already sketched the twofold response to that judgment: First, there is nothing bad about description; and second, today's causal methods are effectively descriptions themselves. But my approach *was* wrong, for reasons both my critics and I failed to see. It was not "sociological." I knew full well that the foundational insight of sociology is that the social world is made up of situated actions, of social *relations*, not of independent stories. Social life is a process that continuously embodies itself in constraining structures. My methods assumed away those structures just as fast as did the causal methods I was attacking. I was *locating* social facts, but only within the individual stories of career or occupational cycle, only within time. I was not putting those careers and occupational cycles in motion in relation to one another. I was not locating them in social structure.

To be sure, there are times when the broad assumption of casewise independence is reasonable and useful. So it was legitimate to compare the order of medical professionalization in Detroit with that in Boston and that in Altoona. But it had probably not been legitimate to treat the careers of German musicians as independent; they moved in a system where successes for some people meant failures for others. And it was no surprise that I found among the great welfare states not only no clear internal reasons for the sequencing patterns but no obvious diffusion explanation for them either. For those states were bound up in the single cultural unit of Western Europe.

With any given social phenomenon, we can probably identify its independence of context in social space and social time. Phenomena that are completely free of context are the province of standard causal methods. Phenomena that are strongly conditioned by their temporal context but relatively free of environing social structure are the province of time series or event history or sequence methods. Phenomena that are strongly conditioned by structural contexts but not by temporal ones are the province of network analysis and spatial autocorrelative methods.

But at the heart of sociology are those phenomena that are fully enmeshed both in social time and social space, what I have elsewhere called interactional fields (Abbott 1997). It is because we study interactional fields that we are a discipline of social relations, concerned with the social *process*. The great empirical literature of such analysis is the literature on small group interaction from Goffman onward, the literature on urbanism and city patterns, and the literature on occupations and professions, as well as substantial portions of the literature on crime and historical sociology.

What we should require of explanation is that it give us an account of how such interactional fields work. This account will not be purely causal. For nearly all of this literature gives a large place to free action, often to strategic choice in particular. Second, it will include temporal effects of many sizes, for in each of these areas a past of many depths shapes the present. Third, it will also include a complex understanding of social structure, for that too pervades interactional fields at many scales.

Middle-range theories and empirical methodologies must, it seems to me, meet these three tests if they are to be consistent with our foundational vision of the social world. In terms of middle-range theory, such work has, in fact, been done. Wallerstein's *The Modern World System* (1976) is essentially a theory of such an interactional field, with history and structure of varying sizes and powers. So also was my own book on professions (Abbott 1988). Such books do not predict what will happen; indeed, they suggest that interactional fields are probably too complex for us to predict. But they do show various internal patterns; they do sketch the "rules of the game"; they do portray the limits and possibilities of action in such systems.

We require quantitative methods that do the same thing. If I may use another forbidden word, we will have to employ *simulation*. Game theory will not get us very far because it is ignorant, except in the most general terms, of a serious concern with structure and with complex temporal effects. But simulation may help us understand the limits and possibilities of certain kinds of interactional fields, and that would be profoundly sociological knowledge.

So, for example, in the world of professions, there are local contexts—local historically and local in terms of the competitors facing a profession—in which it is very useful for a profession to be rigidly organized. There are other contexts in which it is not. When work is expanding rapidly, professions are better off being able to expand rapidly to meet it, for example. But the definition of which kind of moment it is for a particular profession resides in the evolution of the system. The phenomenon of locally expanding work is produced by the ensemble of strategic and nonstrategic actions by all the professions competing in a given area at a given time. Thus, there is no parameter we can put on the variable of rigidity of organization, for that parameter's power is situationally determined, emerging from the evolution of the system. It is not systematically time varying or space varying. There is no way to "window" it or see it as a property of anything other than the moment and the situation. Lazarsfeld was right that understanding "the actor in the situation" was the heart of analysis but wrong to think that it was a simple matter of assembling enough general covering laws and applying them. Situations are all there is. A better strategy of explanation is understanding how an interdependent system evolves internally. Simulation may be the only way to do that.

More important, the "meaning of parameters" can itself change for strategic reasons, that is, through deliberate action of actors in a system. Virtually all forms of current positivism assume that the meaning—causal or otherwise—of an event is fixed for the duration of analysis. But we all believe that one of the central, basic human actions is to redefine something so that the very shape of the present, perhaps even the identities of the actors in the present, can be made new. Any serious methodology has to be able to encompass this kind of meaning change as well. Again, simulation seems the best alternative.

We seem then to be at a turning point. Our explanations are of little interest to the general public and are disconnected from our own general views of society. By broadening our concept of explanation, we can address, once again, the foundational problems of our field.

I do not mean to denigrate the achievements of causalism. Causalism has been an immensely successful paradigm for sociological methodology, but the blunt fact is that it is now getting in the way of developments essential to the field. We have to refurbish and rethink our ideal of what it means to explain social life, and we must reintegrate our theories and methods around that ideal.

NOTES

1. "Each society is predisposed to produce a determined quota of voluntary deaths. This predisposition can thus be the subject of a special study, one under the jurisdiction of sociology" (my translation).

2. Some points of this credo are obeyed throughout the book; types of suicides are separated by causes, for example. But the strict one-to-one causal relation assumed in the second sentence is later jettisoned, even though Durkheim is clearly honest in his professions here. And the explicit rejection of equifinality ("Every proved specific difference") is contradicted throughout by Durkheim's thoroughgoing functionalism.

3. An early source on this subject is Bernert (1983). A more recent and important reference—particularly on sociological positivism—is Platt's (1996) book on methods in American sociology. Bulmer (1984) is important for the earlier period. Stigler's (1986) *History of Statistics* unfortunately cuts off in 1900, although it closes with a chapter on the elder Pearson and Yule.

4. Trond Petersen pointed out to me the central importance of Wold in this story. Wold was a central source for Blalock and spoke explicitly in causal terms. In this section on econometrics, I have relied to some extent on Morgan's (1990) book, although not reading the sources quite as she does. It is tempting to speculate about the influence of quantum mechanics on econometrics's wholesale acceptance of the concept of probable causality. Tinbergen, after all, took a Ph.D. in physics and economics in 1929, when quantum mechanics was very much in the air. The idea of probable causality undergirding statistical associationism received its most important support from quantum mechanics, for in quantum mechanics the reason for probable causes was not mere complexity but in-principle failure of direct determination, as Reichenbach (1951:157-65) noted.

5. For Hirschi and Selvin (1967), their book was as much textbook on methods as a substantive critique of the delinquency literature. In it, the new causalism is quite full-fledged.

6. I have not here discussed the large literature on probabilistic causality. Classical citations are Suppes (1970) and Salmon (1971). Nor am I considering the debates in statistics about causality that are associated with the names of Granger, Rubin, and Holland. (For a trenchant analysis of some of these issues, see Sobel 1996.) These arguments are well canvassed in the statistical literature and are somewhat removed from my concern with causality in the life world and the proper relation of causal reasoning and explanation. As Freedman (1997) says in a characteristically pungent phrase (in his denunciation of the TETRAD program for automatically generating path diagrams from data), "[C]ausation has to do with empirical reality not with mathematical proofs based on axioms" (p. 76). It is tempting to wonder whether the theory of probabilistic causality was an independent arrival or a rationalization of what had by the 1970s long since been common practice in both natural and social sciences.

7. See Duncan's many statements during the departmental debates of 1951-52, discussed in Abbott and Gaziano (1995).

8. I have heard the "won the battles" phrase attributed to Duncan but cannot verify its origin. For Duncan's attack on his followers, see *Notes on Social Measurement* (1984). The remarks on relativist sociology are in Duncan (1951). I do not have space in this article to deal with the lengthy defense of the causal interpretation of probabilities by Cartwright (1989) (note that Cartwright was explicit that she had no defense for the causal interpretation of data; see pp. 13, 35). Cartwright's detailed demonstration of the probability/cause connection seems to me to have difficulties because of its interpretation of disturbance terms, its bypassing of the issues of temporal orderability, and its forgetting that backwards reasoning from probabilities to causes is conditional on true knowledge of the causal situation. As many (starting with Whewell) have argued (and as Cartwright states in her second chapter), the theorizing act is always in the last instance autonomous.

9. The term *colligation* is from Whewell ([1858] 1968:129 ff). Whewell's analytic inductive account of the philosophy and history of science was replaced by the simple induction of Mill, then by the work of Mach and Pearson (who nonetheless owed much to Whewell), and later by the more deductive accounts of the positivists.

REFERENCES

Abbott, A. 1988. *The System of Professions*. Chicago: University of Chicago Press.

———. 1991. "The Order of Professionalization." *Work & Occupations* 18:355-84.

———. 1993. "The Sociology of Work and Occupations." *Annual Review of Sociology* 19:187-209.

———. 1997. "Of Time and Space." *Social Forces* 75:1149-82.

Abbott, A. and S. DeViney. 1992. "The Welfare State as Transnational Event." *Social Science History* 16:245-74.

Abbott, A. and E. Gaziano. 1995. "Transition and Tradition." Pp. 221-72 in *A Second Chicago School*, edited by G. A. Fine. Chicago: University of Chicago Press.

Abbott, A. and A. Hrycak. 1990. "Measuring Resemblance in Sequence Data." *American Journal of Sociology* 96:144-85.

Ayer, A. J. 1946. *Language, Truth and Logic*. New York: Dover.

Bernert, C. 1983. "The Career of Causal Analysis in American Sociology." *British Journal of Sociology* 34:230-54.

Blalock, H. M. 1964. *Causal Inference in Non-Experimental Research*. New York: Harcourt, Brace.

Blau, P. M. and O. D. Duncan. 1967. *The American Occupational Structure*. New York: Free Press.

Bulmer, M. 1984. *The Chicago School of Sociology*. Chicago: University of Chicago Press.

Cartwright, N. 1989. *Nature's Capacities and Their Measurement*. Oxford: Oxford University Press.

Cohen, M. R. and E. Nagel. 1934. *An Introduction to Logic and the Scientific Method*. New York: Harcourt, Brace.

Coleman, J. S. 1964. *Introduction to Mathematical Sociology*. New York: Free Press.

Collingwood, R. G. 1946. *The Idea of History*. Oxford: Oxford University Press.

Danto, A. C. 1985. *Narration and Knowledge*. New York: Columbia University Press.

Denzin, N. K. 1970. *Sociological Methods*. Chicago: Aldine.

Dray, W. 1957. *Laws and Explanation in History*. Oxford: Oxford University Press.
Duncan, O. D. 1951. "Proper Aphorisms for a Relativist Sociology." Typescript in the papers of E. W. Burgess, Special Collections Divison, University of Chicago Library, Box 33, Folder 4.
———. 1966. "Path Analysis." *American Journal of Sociology* 72:1-16.
———. 1984. *Notes on Social Measurement*. New York: Russell Sage.
Durkheim, E. 1897. *Le suicide*. Paris: Alcan.
———. 1951. *Suicide*, translated by G. Simpson and J. A. Spaulding. New York: Free Press.
———. [1895] 1964. *The Rules of the Sociological Method*. New York: Free Press.
Freedman, D. A. 1987. "As Others See Us." *Journal of Educational Statistics* 12:101-223.
———. 1997. "From Association to Causation via Regression." *Advances in Applied Mathematics* 18:59-110.
Gallie, W. B. 1968. *Philosophy and the Historical Understanding*. New York: Schocken.
Goldberger, A. S. 1972. "Structural Equations Methods in the Social Sciences." *Econometrica* 40:979-1001.
Haavelmo, T. 1944. "The Probability Approach in Econometrics." *Econometrica* 12 (suppl.).
Hart, H.L.A. and T. Honore. 1985. *Causation in the Law*. 2d ed. Oxford: Oxford University Press.
Hempel, C. G. 1942. "The Function of General Laws in History." *Journal of Philosophy* 39:35-48.
Hirschi, T. and H. C. Selvin. 1967. *Delinquency Research*. New York: Free Press.
Irzik, G. and E. Meyer. 1987. "Causal Modeling." *Philosophy of Science* 54:495-514.
Lazarsfeld, P. F. and M. Rosenberg. 1955. *The Language of Social Research*. Glencoe, IL: Free Press.
MacIver, R. M. 1942. *Social Causation*. New York: Harper & Row.
Mackie, J. 1974. *The Çement of the Universe*. Oxford: Oxford University Press.
Marini, M. M. and B. Singer. 1988. "Causality in the Social Sciences." *Sociological Methodology* 18:347-409.
Mink, L. O. 1970. "History and Fiction as Modes of Comprehension." *New Literary History* 1:541-58.
Morgan, M. S. 1990. *The History of Econometric Ideas*. Cambridge: Cambridge University Press.
Mullins, N. C. 1973. *Theory and Theory Groups in Contemporary American Sociology*. New York: Harper & Row.
Pearson, K. [1892] 1937. *Grammar of Science*. London: Dent.
Pfautz, H. W. and O. D. Duncan. 1950. "A Critical Evaluation of Warner's Work in Community Stratification." *American Sociological Review* 15:205-15.
Platt, J. 1996. *A History of Sociological Research Methods in America*. New York: Cambridge University Press.
Popper, K. [1943] 1962. *The Open Society and Its Enemies*. 2 vols. Princeton, NJ: Princeton University Press.
Reichenbach, H. 1951. *The Rise of Scientific Philosophy*. Berkeley: University of California Press.
Ruben, D.-H. 1990. "Singular Explanation and the Social Sciences." Pp. 95-117 in *Explanation and Its Limits*, edited by D. Knowles. Cambridge: Cambridge University Press.
Salmon, W. C. 1971. *Statistical Explanation and Statistical Relevance*. Pittsburgh, PA: University of Pittsburgh Press.
Simon, H. A. 1952. "On the Definition of the Causal Relation." *Journal of Philosophy* 49:517-28.
———. 1953. "Causal Ordering and Identifiability." Pp. 49-74 in *Studies in Econometric Method*, edited by W. C. Hood and T. C. Koopmans. Chicago: University of Chicago Press.

———. 1954. "Spurious Correlation." *Journal of the American Statistical Association* 49:467-79.

Sobel, M. 1996. "An Introduction to Causal Inference." *Sociological Methods & Research* 24:353-79.

Stigler, S. 1986. *The History of Statistics*. Cambridge, MA: Harvard University Press.

Stinchcombe, A. 1968. *Constructing Social Theories*. New York: Harcourt Brace Jovanovich.

Stouffer, S. A. 1957. "Quantitative Methods." Pp. 25-55 in *Review of Sociology*, edited by J. B. Gittler. New York: Wiley.

Suppes, P. 1970. *A Probabilistic Theory of Causality*. Amsterdam: North-Holland.

Tukey, J. W. 1954. "Causation, Regression, and Path Analysis." Pp. 35-66 in *Statistics and Mathematics in Biology*, edited by O. Kempthorne et al. New York: Hafner.

Turner, R. H. 1953. "The Quest for Universals in Sociological Research." *American Sociological Review* 18:604-11.

von Wright, G. H. 1971. *Explanation and Understanding*. Ithaca, NY: Cornell University Press.

Wallerstein, I. 1976. *The Modern World System*. New York: Academic Press.

Walsh, W. H. 1958. *An Introduction to the Philosophy of History*. London: Hutchinson University Library.

Whewell, W. [1858] 1968. *Theory of Scientific Method*. Indianapolis, IN: Hackett.

Wold, H.O.A., ed. 1964. "On the Definition and Meaning of Causal Concepts." Pp. 265-95 in *Model Building in the Human Sciences*. Entretiens de Monaco en Sciences Humaines, Session 1964. Paris: Centre International d'Etude des Problemes Humaines.

Wright, S. 1921. "Correlation and Causation." *Journal of Agricultural Research* 20:557-85.

———. 1934. "The Method of Path Coefficients." *Annals of Mathematical Statistics* 5:161-215.

———. 1960. "The Treatment of Reciprocal Interaction, With or Without Lag, in Path Analysis." *Biometrics* 16:423-45.

Yule, G. U. 1912. *An Introduction to the Theory of Statistics*. London: Griffin.

Zetterberg, H. L. 1954. *On Theory and Verification in Sociology*. Stockholm: Almquist and Wiksell.

———. 1965. *On Theory and Verification in Sociology*. 3d ed. Totowa, NJ: Bedminster Press.

Znaniecki, F. 1934. *The Method of Sociology*. New York: Farrar and Rinehart.

Andrew Abbott is Ralph Lewis Professor of Sociology at the University of Chicago. He is the author of The System of Professions, *a study of the division of labor. He has also written extensively on the problem of temporality in social science and has developed novel methods for sequence data. Two recent book manuscripts concern scholarly disciplines and their development.* Department and Discipline *is a study of the Chicago sociology department in relation to the larger discipline.* Chaos of Disciplines *is a theoretical and empirical analysis of knowledge change in social science and, more broadly, of self-similar social structures.*