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Guest Editors' Introduction to the Special Issue on Causality at Work

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Not being able to carry out proper experiments as the true (physical) scientists and even the colleagues from the neighboring psychology department do, sociologists have thrown themselves into the promising arms of the attractive "causal" models. From the 1960s onward, linear structural equation modeling became the norm and standard for the causal analysis of the nonexperimental data, to such an extent that it elicited biting comments from Louis Guttman (1977): "There has been a flowering of causal discoveries in sociology at a pace unheard of in the history of science . . . which undoubtedly put sociology at the forefront of all sciences in terms of frequency of discovery of fundamental relationships" (p. 103). Many others for many different reasons and from many different angles have shared Guttman's criticisms and worries.

An early criticism, directly relevant for the practicing social researcher, was Derek Philips's (1971) *Knowledge From What?*, asking incisive questions about the validity of the data on which causal analyses are based (and which later led him to *Abandoning Method*) (Philips 1973). Also from within the empiricist tradition, Lieberson's (1985) *Making It Count* attacked the too simplified and rigid nature of

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traditional causal modeling (see also the discussion in Clogg 1987, chaps. 12, 13). In much the same vein, David Freedman (1991) preferred hard work and detailed investigations and descriptions ("shoe leather") above the easy mechanics of regression models that "substitute technique for work" (see also the discussion in Marsden 1991, chaps. 10-14). The core of these criticisms concerns the lack of fit between, on one hand, the properties of linear structural equation modeling and the underlying statistical assumptions and research design, and, on the other hand, the problems, theories, and data that social scientists are interested in.

Recent work on causal modeling that now dominates the area is only marginally related to these "practical" concerns but is mainly of a different nature (for an excellent account of these recent developments, see the special issue of *SMR* on Causality in the Social Sciences, edited by Raftery 1998, and Winship and Morgan 1999). This work might be characterized as trying to answer the question: When is a relation obtained from a statistical analysis of a causal relationship? This question is not answered at the ontological level, not as "an account of the nature of causation: what differentiates causal relations from non-causal relations and what causal relations, as part of the world, are like" (Humphreys 1989:3); the main interest is epistemological and focuses "on how causal relationships are discovered, how hypotheses about causal relations are tested and confirmed, when it is justifiable to assert a causal claim, and what kinds of causal inferences might be valid" (Humphreys 1989:3).

Causal claims for the relationship between two variables *X* and *Z* have been based traditionally on the fulfillment of the Humean criteria, as elaborated for the social sciences by Paul Lazarsfeld and his coworkers (see Hyman 1955):

1. There exists a nonzero association between *X* and *Y*.
2. *X* precedes *Y*.
3. The relationship between *X* and *Y* is not spurious.

The focus of the recent discussions might be characterized to a large extent as an attempt to clarify the precise meanings of these three conditions, especially the third one. (For an interesting clash between the old and the new formulations, see the discussion between Mellenbergh and Pearl in Adèr and Mellenbergh 1999, chap. 14.) This clarification has its roots in three main developments in statistics. First, there is the Rubin-Holland model, which formalizes the counterfactual account of causality, starting from the randomized experimental design. Second, there is the work of what we might call, after the program they have developed, the group of TETRAD statisticians who take their lead from linear structural equation modeling and try to find out under what conditions alternative causal models might be searched for. Finally, the principles of graphical modeling, especially methods using directed graphs, have been applied to causal modeling, integrating the first two approaches and expanding them to general nonlinear structural equation models. (For various partial accounts of these developments, see Clogg 1988, chaps. 11-14; Glymour and Cooper 1999; Pearl 2000; Winship and Morgan 1999.)

Thanks to these developments, we now understand much better and much more precisely what spuriousness is; what it means that a particular causal model is closed; what the connections are between (interventionist) experimental and observational, nonexperimental designs; what the role is of omitted variables, of latent variables, and of covariates in causal analyses; how to interpret indirect effects; how linear and nonlinear structural equation models are related to each other; and so forth. Moreover, we have gained more precise and general insights into the damaging influence on the possibility of drawing causal conclusions of disturbing factors such as differential nonresponse, selective noncompliance, hidden bias, and in general, selection bias (see, among many others, the references above; Rosenbaum 1995; Manski 1995). Within well-defined causal models, their disturbing influence can be assessed and sometimes even annihilated. In sum, these accomplishments are remarkable and true progress has been made.

However, not everybody agrees that all of this constitutes a straight success story (for a variety of disagreeing opinions, see, e.g., McKim and Turner 1997; Abbott 1998). Part of the criticism stems from ontological concerns about what constitutes causality. It is essentially assumed in these (graphical) models that we (tacitly) know what causality is and that it is meaningful to say, given certain conditions within a particular causal (structural equation) model, that a directed arrow between two variables is causal. But what constitutes the specific nature of a causal relationship is not further delineated. Other critics focus on whether the claims these procedures make are valid from a statistical point of view, and still others, and the ones who interest us most here, criticize the lack of fit between these models and what social researchers (have to) do in practice.

We feel that there is still a rather wide gap between the empirical research carried out by many social researchers and the counterfactual and graphical approaches to causality. Even without having digested this literature, methodologically trained social researchers already tend to be quite cautious in their causal claims and to speak about their findings in terms of association rather than causation. Their main worries with regard to causal analyses are not fully solved by these latest developments. Researchers are interested in what is the best theoretical model from a substantive causal point of view, given the many models that are possible and that are not invalidated by the data. They need models that adequately reflect the theoretically plausible causal mechanisms and that are not too rigid or make unrealistic assumptions about the data.

This, of course, is not said to deny or belittle the importance of these recent developments. However, we feel that the success of the counterfactual and graphical approaches should not give the impression that all problems with regard to causal analyses are solved now and the other issues in causal analyses may be forgotten. It is like when a student (he happened to be male and majoring in econometrics), after we pointed out that a particular relationship between two variables might not be linear, answered, completely baffled by our remark, "Of course it's linear; I did a linear regression analysis." One should not blame Karl Pearson for having developed the wonderful linear regression model, but one should blame the teachers of this student (and each of us) for not having sufficiently pointed out to him that models should follow the substantive questions and data researchers work with and not the other way around.

It is in this latter vein that the sessions on causality, mentioned in the Authors' Note, were organized, asking a group of social science researchers to talk about *causality at work* and their struggles with concrete causal analyses. Four papers from these presentations now appear together in this issue.

The first article, by Willem E. Saris, might seem a standard application of linear structural equation modeling to establish a causal relation between, in this case, the variables income and satisfaction with life. However, in several respects, this article highlights points of view that are crucial for any research with some claims to causal inference. First, several model assumptions that are usually taken for granted are explicitly investigated. This applies to the standard assumption that the variables are either reliably measured or that unreliability is taken care of by introducing latent variables, although the latter possibility often leads to unidentified models. Saris shows how unreliability estimates can be obtained outside the causal model itself by means of independent information coming out of a rather new and challenging procedure. Furthermore, he questions explicitly the assumption, too often taken for granted, of a linear relationship between the two variables concerned. But most interesting is his investigation of possible misspecifications of the original causal model. This model would never have been found by the TETRAD procedure, because it involves essentially a theoretically based redefinition of the original variables and because it concerns a spurious zero correlation. The occurrence of such spurious null associations is essentially ruled out by the faithfulness or stability assumption embedded in TETRAD and related approaches (Glymour and Cooper 1999; Pearl 2000).

The second article by Johannes van der Zouwen and Theo van Tilburg addresses in an unusual way one of the most fundamental assumptions usually made about structural equation models, namely, that the measurement errors are independently distributed. Their research topic is to estimate the size of the personal networks the respondents are involved in. The design is a panel study in which for some respondents the same interviewers were employed in different waves and for other respondents different interviewers. There were clear indications that the panel observations were not independent of each other. The dependence might have been caused by individual test-retest effects or by having been interviewed by the same interviewer. These kinds of dependence of observations could be modeled by means of correlated error terms in combination with multilevel (random coefficient) models. Van der Zouwen and Van Tilburg, however, chose "shoe leather" over modeling and made an intensive study of the recorded interview sessions. Their study of the interviews produced a causal story not about the substantive topic but about a threat to validity of the independence assumption in a panel design such as is often used in studies directed at causal inference. This provided clues for preventing this bias in future research.

Peter Abell in the third article focuses on the situation that the number of cases is so small that statistics cannot be used, and even the logic of comparative case studies (Ragin 1987; King, Keohane, and Verba 1994) is hardly applicable. Is it possible to reconcile the idea of causality with data about unique events? Abell defends the position of establishing causal relationships, perhaps as the only way out, on the basis of what he calls "internal evidence," actually by constructing a convincing story (narrative). Abell himself admits the controversial nature of this position. However, it may well be that his major concerns are not that far away from what traditional causal modeling approaches try to accomplish. Cox and Wermuth (1996) distinguished between three interrelated senses of causality: first, as an association that cannot be explained away by other variables; second, as an inferred consequence of some intervention; third, as the first and the second, but then augmented by some understanding of a process or mechanism accounting for what is observed (pp. 219-28). They actually favored the third sense of causality. We fully agree. Causal diagrams are just and, in essence, shorthand notations for a more convincing and extended story (or narrative) that we would like to tell. As Cox and Wermuth indicated, these notions are hard to formalize. They present 12 qualitative conditions (adapted from A. Bradford Hill) to operationalize this "interpretative" process; this kind of reasoning may join with the thrust of Abell's argument to deepen our understanding of causality and of causal inference outside the realm of statistics.

The final contribution is by Patrick Doreian. He reflects on the meaning and role of causality in social network analysis. In this area, causal analyses are less common than in standard survey analysis, but the network literature contains explicit claims that network structure is crucial for sociological explanation, which is not the same as causal interpretation but does come quite close. Doreian reviews several approaches to causality from the point of view of their significance and usefulness for social network research. In his conclusion, he stresses the importance of coupling theory, ideas about mechanisms, and empirical information as a prerequisite to making causal interpretations. Furthermore, he regards statistical causality as no more, but also no less, than a source of potential data-analytic tools that can be used in diverse causal approaches. This lines up with Saris's paper in the requirement of statistical models that contain credible relations between variables expressing theoretical ideas about causal relations, although the type of model Doreian has in mind expresses more fine-grained behavioral detail than the models in Saris's contribution.

These four articles illustrate that to carry out sociological research with claims coming anywhere near to causal explanation, there are important points to consider in addition to those following from the currently dominant discourse about counterfactual interpretations and graphical models. The authors plead for careful consideration of assumptions underlying statistical models, credible model specifications closely linked to theoretically plausible mechanisms, and an open mind for the possibility of causal inference outside the domain of statistical replication. For the working social scientist and the practical methodologist who wish to come close to causal explanation, these points are just as indispensable as the careful elaboration of the meaning of spuriousness of associations and the most appropriate control for disturbing effects.

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