# CAUSATION, STATISTICS AND SOCIOLOGY

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## Introduction

In a paper of great insight, though sadly posthumous, Bernert (1983) has noted a long-standing uncertainty among sociologists in regard to the concept of causation and its use in their work. "Uncritical adulation" in the later nineteenth century gave way to "complete rejection" in the early twentieth, followed in turn, in the years after the second world war, by "pragmatic utilization" – a position which, one might add, has itself been subject to rising criticism in the period since Bernert's review. These vicissitudes in the "career of a concept" have to be understood, as Bernert shows, in the context not only of the development of sociology itself but of larger scientific and philosophical debates.[1] In this essay I seek, much in the spirit of Bernert's contribution, to draw attention to some results of statisticians being increasingly involved in such debates, and further to consider the reception in, and potential for, sociology of the new understandings of causation that have thus emerged.

The founders of modern statistics might be regarded as representatives of the era in which the concept of causation was viewed with scepticism. At least for Pearson (1892), it was a mere "fetish", carried over from metaphysical, pre-scientific thinking, which was to be abandoned and replaced by that of correlation, at once both more general and more precise. However, an opportunity for statisticians to make a more constructive contribution came at a later point with the introduction by

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[1] Bernert's paper concentrates on the concept of causation in American sociology but is in fact of quite general relevance.

philosophers, in the 1940s and 1950s, of the idea of “probabilistic”, as opposed to “deterministic”, causation: i.e. the idea, roughly, that rather than causes being seen as necessitating their effects, they might be regarded simply as raising the probability of their occurrence (for reviews and more recent developments, see Salmon, 1980; Eells, 1991). A probabilistic view of causation might be associated with the argument that the world itself is non-deterministic; but such a view could also be favoured simply on the grounds that, whether the world is deterministic or not, it is too complicated, and our knowledge of it too error-prone, to permit anything other than probabilistic accounts to be provided. The latter position, at least, is one that would seem likely to commend itself to most sociologists – despite some recent attempts to uphold both the desirability and possibility of causal explanations in sociology that are of an entirely deterministic kind (see e.g. Ragin, 1987; Becker, 1992; and for critical comment, Lieberson, 1992, 1994; Sobel, 1995; Goldthorpe, 1997a,b).

In what follows, I take up three different understandings of causation that have been importantly shaped by contributions from statisticians. These I label as:

(i) causation as robust dependence;
(ii) causation as consequential manipulation; and
(iii) causation as generative process.

I sketch out these positions in a deliberately broad and non-technical way. My concern is not with different individual formulations of each position and their internal coherence from a philosophical or a statistical point of view. I am interested, rather, in differences among these positions considered generically and with the question of what each might have to offer to “working” sociologists who wish to engage in causal analysis of some kind.[2] I treat the three ideas of causation in the above order, and then, drawing especially on the last, outline a further position which, it seems to me, could be – and indeed to some extent already is – both viable and valuable in sociology.

## Causation as Robust Dependence

The starting point here is with the proposition, widely recognised in both philosophy and statistics, that while correlation – or, more generally, association – does not imply causation, causation must in some way or

[2] Other reviews of issues of causality from a statistical point of view, from which I have greatly benefited, are Holland (1986a); Berk (1988); Cox (1992); and Sobel (1995).

other imply association. The key problem that has then to be addressed is that of how to establish whether, or how far, the observed degree of association of variable X with variable Y, where X is temporally prior to Y, can be equated with the degree to which X is *causally significant* for Y.[3] It may indeed be that the probability of Y, given X, is greater than the probability of Y given not -X; but this is not in itself sufficient to demonstrate that X is a cause of Y. For example, it could be that a third variable (or set of variables), Z, is the cause of both X and Y, so that, if one conditions on Z, the association between X and Y disappears: i.e. Y becomes statistically independent of X and any supposed causal link between X and Y is revealed as spurious. It could, though, also be that conditioning on Z does not entirely remove the association between X and Y but merely weakens it. In this case, the implication is not that X is a spurious cause of Y, but only that there is *some part* of the observed association between them that does not reflect the causal significance that X has for Y. A solution to the problem of moving from association to causation has then generally been pursued through an argument to the effect that X is a "genuine" cause of Y in so far as the dependence of Y on X can be shown to be robust: that is to say, cannot be eliminated through one or more other variables being introduced into the analysis and then in some way "controlled" (see especially Simon, 1954; Suppes, 1970).

One particularly influential version of the attempt to understand causation in this way is that proposed by Granger (1969) in the context of the analysis of econometric time-series, which has the further, more distinctive, feature of treating causation explicitly in terms of predictive power. A variable, X, "Granger causes" Y if, after taking into account all information apart from values of X, these values still add to one's ability to predict future values of Y. In principle, "all information" here refers to all information that has been accumulated in the universe up to the point at which the prediction of Y is made. In practice, however, Z has to refer to some particular information set, and what counts as a Granger cause would seem to be any non-zero partial correlation that improves the analyst's forecasting ability. Thus, as Holland (1986a) has argued,

[3] I am aware of some special cases in which it might be argued that causation is present in the absence of association: e.g. where X does have an effect on Y which, however, happens to be *exactly* cancelled out by a further and opposing effect that X exerts on Y via a third variable, Z. For present purposes, I believe that such cases can be safely disregarded. I might also add that here, as throughout, I assume that effects cannot precede causes and further that plural or "multifactorial" causation may operate.

Granger causation is established essentially through the detection and elimination of spurious causal significance, or of what Granger himself calls "non-causality": X is *not* a Granger cause of Y, relative to the information in Z, to the extent that the correlation between X and Y disappears, given Z. That is to say, the idea of robust dependence is crucial.

This same idea is also to be found, though again with a particular slant, in methodological programmes developed within sociology – most obviously, perhaps, in Lazarsfeld's proposals for "elaboration" in the analysis of survey data (see e.g. Kendall and Lazarsfeld, 1950; Lazarsfeld and Rosenberg (eds.), 1955; Lazarsfeld, Pasanella and Rosenberg (eds.), 1972). Lazarsfeld is, like Granger, concerned with detecting spurious causation, but in the interests less of prediction than of explanation. Thus, a further and seemingly more positive strategy that Lazarsfeld advocates is to begin with a correlation between X and Y that is of substantive interest – say, a correlation between area of residence and vote; but then, rather than supposing any direct causal link, to seek an explanation *of the correlation itself* by finding one or more prior variables, Z – say, social class or ethnicity – which, when brought into the analysis, will reduce the partial correlation of X and Y to as close to zero as possible. To the extent that this is achieved, Z can be viewed as the cause of both X and Y – or, at all events, until such time as further "elaboration" might bring the robustness of their dependence on Z itself into question.

The regression techniques taken over from econometrics and biometrics, including causal path analysis, that became familiar in quantitative sociology from the 1970s onwards marked important advances on Lazarsfeldian "elaboration" in both their refinement and scope. But, as Davis (1985) has shown (cf. also Clogg and Haritou, 1997), so far as the basic understanding of causation is concerned – causation as robust dependence – a clear continuity can be traced. It is in fact the methodological tradition thus represented that has served as the main vehicle for the "pragmatic utilization" of the concept of causation by sociologists which Bernert sees as characteristic of the post-war years. However, as I suggested at the start, growing dissatisfaction with this position has of late been apparent, and among sociologists whose primary interest is in empirical research as well as among theorists and methodologists.

At the source of this dissatisfaction is a problem with the idea of causation as robust dependence to which attention has been drawn from various quarters. If causation is viewed in this way, then, it would appear, establishing causation becomes *entirely* a matter of statistical inference, into which no wider considerations need enter. Causation can be derived directly from the analysis of empirical regularities, following principles that are equally applicable across all different fields of inquiry, and without the requirement for any "subject-matter" input in the form of background knowledge or, more crucially, theory. This implication might not appear too disturbing if, as with Granger, the essential criterion of causation is taken to be increased predictive power. But most philosophers of science would find this a too limited view, and would wish to regard causation as entailing something more than (if not other than) predictability – on the lines, say, of "predictability *in accordance with theory*" (cf. Feigl, 1953; Bunge, 1979). Moreover, among economists, and even among econometricians (e.g. Geweke, 1984; Basmann, 1988; Zellner, 1988), there are many who would maintain that while "Granger causation" may be an idea of great practical utility for the purposes of forecasting, it can lead to *causal explanation* only when the demonstrated statistical relationships are provided with some rationale in theory and, moreover, in theory ultimately at the micro-economic level. In a discussion of prediction in economics, Sen (1986, p. 14) observes that the magnitudes of concern to the forecaster are all social magnitudes, and that variables such as prices, investment, consumption and money supply "do not, naturally, move on their own, untouched by human volition". Thus, while "mindless macroeconomics" may serve as a basis for predictions – or, at all events, for "simple and immediate" ones – any "deep explanation" of the movement of the magnitudes involved can, in the end, only be gained through theory, and of a kind that makes reference to the "objectives, knowledge, reasoning and decisions" of individuals acting in society.

In sociology itself forecasting is a far less prominent activity than in economics, and it is then scarcely surprising to find that the treatment of causation in terms of predictability has been still more sharply rejected, and that arguments analogous to that of Sen on the need to go beyond the analysis of variables have been very widely expressed. Especially from the standpoint of methodological individualism, sociologists have strongly criticised the supposition that statistical techniques can in themselves provide adequate causal explanations of social phenomena. Such techniques can show only relations among variables, and not how these relations are actually produced – as they can indeed only be produced – through the action and interaction of individuals (see Boudon, 1976, 1987; Coleman, 1986; Abbott, 1992; also Lindenberg and Frey, 1993; Esser; 1996; Hedström and Swedberg, 1998a,b).

For example, if, in a causal path analysis, a path is shown as leading from educational attainment to level of occupation or of income, it does not make much sense to talk, on this basis, of education *causing* occupation or income. Individuals get jobs because other individuals or employing organisations offer them jobs or because they make a place for themselves, as self-employed workers, in some market for goods or services. And likewise they get income because employers pay them or because they secure fees or make profits. Thus, even if it is clear from statistical analysis that how well individuals fare as regards jobs and income is dependent in some part on their educational attainment – and that this dependence is indeed robust – the question remains of just how this dependence comes about. It could be that education provides saleable knowledge and skills; but it could also be that education is used by employers chiefly as an indicator of job-seekers' psychological or social characteristics; or, again, that education allows individuals to pass "credentialist" filters chiefly set up to suit employers' convenience or to restrict the supply of labour to particular kinds of employment. To establish a causal link between education and occupation or income would then require, in the first instance, situating the variable of "educational attainment" within some generalised narrative of action which would represent one or other such process that is of a "causally adequate" kind. And in the interests of clarity, consistency and subsequent empirical testing, it would then be further desirable that any narrative thus advanced should be not merely *ad hoc* but rather one informed by a reasonably well-developed theory of social action.

It should, moreover, be noted that such questioning of the capacity of "variable sociology" to produce causal explanations has received strong reinforcement from objections raised by statisticians to the way in which techniques such as causal path analysis have actually been applied in sociology. Most notably, Freedman (1992a, 1992b, 1997 especially, also 1983, 1985, 1991; and cf. Clogg and Haritou, 1997) has built up a cogent critique around three main points: first, that such modelling itself requires a theoretical input to determine the variables to be included, their causal ordering, the functional form of relationships between them etc; second, that in so far as the theory is mistaken – i.e. is inconsistent with the social processes that actually generate the data used – the results of the analysis will be vitiated; and, third, that available sociological theory may just not be strong enough to help produce models that can be treated as genuinely "structural" – i.e. so parameterised that their coefficients are sufficiently invariant and autonomous to sustain claims about the consequences of changes in the variables deemed to be "exogenous". For instance, a model might purport to show, on the basis of past observations, the degree to which inequalities in income among classes or ethnic groups are dependent on differences in their educational attainment; but if, as a result, say, of policy intervention, educational differentials were to be reduced, it could be seriously doubted whether reductions in income inequalities would then follow in the manner expected under the model.

In sum, an understanding of causation simply as robust dependence would seem best regarded more as a feature of sociology's past than of its future – of the period in which it was widely, although for the most part unreflectingly, believed that the making of causal inferences would be facilitated *pari passu* with the advance of statistical methodology. To conclude thus is not, I would stress, to imply that no such advance was achieved, nor that techniques such as causal path analysis have proved of no value in sociology. Rather, it is to suggest, and the point will in due course be developed further, that the potential of such techniques for sociology has been misjudged – though less, it should be said, by the real pioneers than by their epigoni[4] – and now stands in need of serious re-evaluation.

## Causation as Consequential Manipulation

Among statisticians, the idea of causation as consequential manipulation would appear to have emerged in reaction to that of causation as robust dependence from a relatively early stage. Cook and Campbell (1979, p. 26) claim to be expressing a long-standing view when they contend that this latter idea, or what they themselves call the "partialling approach", does not adequately accord with the understanding of causation in "practical science" – which they would, apparently, see as best exemplified by medical or agricultural science. Here, attention centres specifically on "the consequences of performing particular acts" or, in

[4] Lazarsfeld, for example, always urged that elaboration should go together with "interpretation" which involved specifying intervening variables in the supposed causal connection and the provision of some appropriate "story-line". Again, Duncan's standard work (1975) could scarcely be more explicit on the problems that sociologists must face, and overcome, if they are to produce valid causal path models. It is of particular interest to read one of his main cautionary passages in conjunction with Freedman's critique, outlined in the text above: "A strong possibility in any area of research at a given time is that there are *no* structural relations among the variables currently recognized and measured in that area. Hence, whatever its mathematical properties, no model describing covariation of those variables will be a structural model. What is needed under the circumstances is a theory that invents the proper variables…There were no structural equation models for the epidemiology of malaria until the true agent and vector of the disease were identified, although there were plenty of correlations between prevalence of the disease and environmental conditions." (1975, p. 152).

other words, on establishing causation through experimental methods; and this, they urge, is the paradigm for causal analysis that should in general be followed. Subsequently, a number of statisticians (see especially Rubin, 1974, 1977, 1990; Holland, 1986a,b) have developed and refined this position in a technically impressive way.

In outline, the argument is as follows. Causes can only be those factors that could, conceptually at least, serve as “treatments” in experiments: i.e. causes must in some sense be manipulable. In turn, the indication of genuine causation is that if a causal factor, X, is manipulated, then, given appropriate controls, a systematic effect is produced on the response variable, Y. Understood in this way, causation is always relative. It is, in principle, determined by *comparing* what would have happened to a “unit” in regard to Y if this unit had been exposed to X (treatment) with what would have happened if it had not been exposed to X (control). This formulation gives rise to what Holland (1986a) has called the “Fundamental Problem of Causal Inference”: i.e. it is not possible in the same experiment for a unit to be both exposed and not exposed to the treatment. But the problem has a statistical solution. One can take the whole population of units involved and compare the *average* response for exposed units with the average response for control units, with the difference between the two being then regarded as the average causal effect.[5] For this solution to be viable, however, it is essential that various conditions are met. Units must be assigned to the treatment or control subsets entirely at random; and the response of a unit must be unaffected either by the process of assignment itself or by the treatment (or absence of treatment) of other units. In sum, the conditions required are, ideally, those of randomised experimental design, as elaborated in statistical work from Fisher’s (1935) classic study onwards.

There would seem to be wide agreement that the idea of causation as consequential manipulation is stronger or “deeper” than that of causation as robust dependence (cf. Holland 1986a, Cox, 1992; Sobel, 1995, 1996). With the latter, it is observed, a variable X can never be regarded as having causal significance for Y in anything more than a provisional sense; for it is impossible to be sure that all other relevant variables have in fact been controlled. At any point, further information might be produced that would show that the dependence of Y on X is

[5] Holland (1986a, p. 947) distinguishes this “statistical” solution from the “scientific” solution typically pursued in laboratory experiments which rests on various assumptions concerning the homogeneity of units and the invariance of measurements made of their properties.

not robust after all or, in other words, that the apparent causal force of X is, at least to some extent, spurious. In contrast, in so far as causation is inferred from the results of appropriately designed experiments, the issue of spuriousness is avoided: the random assignment of units to exposure or non-exposure to the treatment variable replaces the attempt – the success of which must always be uncertain – to identify and statistically control all other variables that might be of causal significance.

Such an argument carries force. None the less, it is at the same time important to recognise that, in moving from the one understanding of causation to the other, a far from negligible redefinition appears to occur of the actual problem being addressed. To put the matter briefly, while exponents of causation as robust dependence are concerned with establishing *the causes of effects*, exponents of causation as consequential manipulation are concerned – and more narrowly, it might be thought – with establishing *the effects of causes*. Holland (1986a, p. 959) indeed acknowledges this. Although "looking for the causes of effects is a worthwhile scientific endeavour", he argues, "… it is not the proper perspective in a theoretical analysis of causation". It is more to the point to take causes simply as "given" or "known", and to concentrate on the question of how their effects can most securely be measured. The main justification offered for this stance would seem to be (see especially Holland, 1986b, p. 970; cf. also 1988) that while statements in the form "X is a cause of Y" are always likely to be proved wrong as knowledge advances, statements in the form "Y is an effect of X", *once they have been experimentally verified*, do not subsequently become false: "Old, replicable experiments never die, they just get reinterpreted."

In assessing how appropriate to sociology the idea of causation as consequential manipulation might be, this shift in focus must not be lost sight of, and I shall indeed return to it. But a more immediate issue is the extent to which the idea can be applied at all, given that most sociological research is not – and, for both practical and ethical reasons, cannot be – experimental in character.

What would in this regard be recommended by those subscribing to the principle of "no causation without manipulation" is that in their empirical work sociologists should seek as far as possible to mimic experimental designs and, in particular, through what have been called, in a rather special sense, "observational studies". Such studies are those in which a treatment or, in a social context, a political or administrative "intervention" of some kind actually takes place; or, at very least, in which it is possible to understand the situation studied *as if* some treatment or intervention had occurred (cf. Rosenbaum, 1995, p. 1). The problem of approximating the requirements of randomised experimental design, it is argued, can then be addressed by making the process of unit assignment, whether actual or supposed, itself a prime concern of the inquiry. Specifically, researchers should attempt to identify, and then to represent through covariates in their data analyses, all influences on the response variable that could conceivably be involved in, or follow from, this process. Thus, in a study of, say, the effects of a vocational education and training scheme on workers' future earnings, it would be necessary to investigate any possible selection biases in recruitment to the scheme (i.e. in the assignment of individuals to the treatment rather than the control subset); any unintended effects of recruitment or non-recruitment (e.g. on workers' motivation); any links fortuitously established with labour markets during the scheme etc., so that all such factors might be appropriately taken into account in the ultimate attempt to determine the effect on earnings of the treatment *per se*: that is, the education and training actually provided.

A difficulty at once apparent here is that of how it can be known if the set of covariates that is eventually established does indeed warrant the assumption that, given this set, treatment assignment and unit response *are* independent of each other. Have *all* relevant influences been represented and adequately measured and controlled? A whole battery of statistical techniques has in fact been developed to help answer such questions (see e.g. Rosenbaum, 1995). However, valuable though these techniques are, it is still difficult to avoid the conclusion that, in non-experimental social research, attempts to determine the effects of causes will lead not to results that "never die" but only to ones that have differing degrees of plausibility. And since this plausibility will in part depend on the existing subject-matter knowledge and theory that, presumably, guide the selection of covariates, such results will have to be provisional in just the same way and for just the same reasons as those of attempts to determine the causes of effects via the "partialling approach".

Furthermore, it is still difficult to see how observational studies in the sense in question could have anything other than a quite marginal role in sociology. While they could well be taken to represent the preferred design in evaluation research, it would appear no more than a statement of fact to say that in most other forms of inquiry in which sociologists presently engage, they could have little application – and even if this statement might then invite the conclusion that sociological research is not in general of a kind adequate to sustain causal analysis.

In this regard, the crux of the matter is of course the insistence of Rubin, Holland and others that causes must be manipulable, and their consequent unwillingness to allow causal significance to be accorded to variables that are not manipulable, at least in principle. In this latter category are those variables that are "intrinsic" to units – i.e. part of their very constitution. Proponents of a manipulative view of causation would argue that an intrinsic variable may be considered as an *attribute* of a unit and shown to be associated with other variables, but that it cannot meaningfully be said to have "effects" on them, since in the case of such a variable it does not make any sense to envisage a unit as taking a different value to that it actually has. The only way for an intrinsic variable to change its value would be for the unit itself to change in some way – so that it would no longer be the same unit. Thus, to give a sociological example, one could discuss the association that exists between sex or race, on the one hand, and, say, educational attainment, on the other. But it would be no more meaningful to speak of sex or race as being causes of such attainment than it would be to make statements about what level of education Ms M would have achieved had she been a man or Mr N had he been a woman.

It is in fact this restriction imposed on variables that can be treated as causes that has led to most objections from sociologists and other social scientists, and also from philosophers, to the principle of "no causation without manipulation" (see e.g. Geweke, 1984; Glymour, 1986; Granger, 1986; Berk, 1988). However, what I wish further to suggest here is that, from a sociological standpoint at least, this restriction is worrying not just because of the difficulties that arise over the causal significance of attributes, on which discussion has in fact so far centred, but also, and indeed more so, because of those that arise in another, quite different respect: that is, over the causal significance of *action*. This argument can be developed on the basis of a simple but illuminating example from Holland (1986a).

Holland considers the three following statements, each of which could be taken to suggest causation in some sense.

(A) She did well on the exam because she is a woman.
(B) She did well on the exam because she studied for it.
(C) She did well on the exam because she was coached by her teacher.

To begin with (C), this refers to an intervention – i.e. coaching by the teacher – and thus the idea of causation as consequential manipulation, which Holland supports, is clearly applicable. In apparent contrast, the reference in (A) is to an attribute – sex – and in this case the suggestion of causation would, from Holland's position, be mistaken. However, as Berk (1988, p. 167) has observed, in a sociological context, what may seem *prima facie* to be a reference to an attribute, such as sex or race, often turns out to be a reference, rather, to a social construct built up around an attribute (cf. also Rubin, 1986). Thus, (A) could be quite plausibly taken as claiming that women do well not because of their (biologically fixed) sex but because of their (in principle, alterable) gender; and a "manipulative" causal interpretation would then be possible, with the implication that if the social construction or perception of gender were to be changed in some way, women would do less well.

It is, I would believe, statement (B) that, from Holland's point of view, creates the really serious problems. Here there is reference neither to an intervention in regard to a manipulable factor nor to an attribute. The obvious elaboration of (B) would be as follows: she had the goal of doing well in the exam; she believed that studying for the exam was the best way of achieving this goal; therefore she chose to study; therefore, her belief being correct, she did well. It may be noted that the form of this narrative is of the general kind which, as earlier seen, has been proposed by both economists and sociologists in order that adequate recognition may be made of the human action that must underlie all statistically demonstrated social regularities: i.e. a narrative given, to use again Sen's words, in terms of individuals' "objectives, knowledge, reasoning and decisions". And most sociologists would, I believe, wish to regard this kind of explanatory narrative as being causal in character: the woman's doing well was caused by her taking appropriate means to this end. But, as Holland (1986a, p. 955) indeed appreciates, such accounts cannot in any very convincing way be reconciled with the idea of causation as consequential manipulation, and primarily because of "the voluntary aspect of the supposed cause".[6] Thus, either a limit to the

[6] The difficulty for Holland here is that of reconciling purposive or "outcome-oriented" and rational action on the part of an individual with the idea of "caused" action in the sense he would favour, which must take on the character of a response to an intervention. It might be noted that a somewhat related objection to treating the reasons for actions as their causes was advanced by chiefly neo-Wittgensteinian philosophers on the lines that causation must entail causes and effects that are logically independent, whereas the reasons for an action and the actual course it follows will, at least in the case of rational action, be logically connected (see e.g. MacIntyre, 1962). However, the force of this objection has been increasingly questioned and the idea of reasons for action as representing at all events one kind of causation among others would appear by now to have gained rather wide philosophical acceptance (see e.g. Toulmin, 1970; Mackie, 1974, Ch. 11; Davidson,

applicability of this idea has here to be accepted or else sociologists must be required to reform, in at least one rather crucial respect, the language of causation that they are accustomed to using. This problem of agency, as it might be called, is one reason, Holland concedes, why the argument over what constitutes proper causal inference has to be left, and is likely to remain, “without any definitive resolution”.

It has, moreover, to be noted that a version of the problem may well arise in “observational” studies in sociology, in the special sense noted above: i.e. studies that seek to determine the effects of some kind of intervention and that would thus appear to offer the best possibility for implementing a manipulative approach to causation. In such studies, it cannot be supposed that the response of the units involved – that is, ultimately of the individuals affected or potentially affected by the intervention – will be of the same nature as that of the units in an experiment in some applied natural science. These individuals are likely to know that the intervention is taking place, to have beliefs about what its aims are and what might follow from it, and then to relate their understanding of the situation to their own interests and goals and to act accordingly – which could in fact mean acting so as actually to counter or subvert the intervention. In the case, say, of the introduction of some kind of positive discrimination in education, with the aim of reducing class or ethnic differentials in attainment, it could be that members of those classes or ethnic groups whose children would not benefit and who might lose their competitive advantage in schools and labour markets could respond – that is, act – so as to preserve this advantage: as, for example, by devoting more of their own resources to their children’s education or by trying to modify processes of educational or occupational selection so that their children would still be favoured. And such a response could indeed occur, in a pre-emptive way, even where the intervention was not made: that is, within educational administrations or geographical areas assigned to the “control” rather than the “treatment” subset.

In such circumstances, at least one of the crucial requirements of randomised experimental design would then clearly be breached: i.e. that the response of a unit should not be influenced by whether other units are treated or not. And still more basic issues do in any event arise. For example, is an intervention to be regarded as causally consequential if it would have had an effect had it not at the same time caused an offsetting response? And would it make any sociological

1980, Chs. 1 and 14 especially). On the application of this same idea in economics, see Helm (1984).

sense to try to control for such a response, even supposing that this were in some way possible?

The very fact that such questions can be asked serves then to re-emphasise the difficulties of translating an approach to causation developed within applied natural science into a social science context.[7] So far at least as sociology is concerned, the ultimate source of these difficulties might be specified as follows. The approach allows conceptual space for human action, and in particular for action of a purposive or "outcome-oriented" kind, *only in* the roles of experimenter or "intervener". Once the experiment or intervention is made, all else has to follow in the manner simply of bacteria responding to a drug or plants to a fertiliser: i.e. in ways to which considerations of individuals' "objectives, knowledge, reasoning and decisions" have no further relevance. In turn, a rather paradoxical if not contradictory position is arrived at. It is maintained that only through purposive action taken in the role of experimenter or intervener can genuinely causal processes be set in motion – "no causation without manipulation"; yet action taken by individuals in *other* roles, in the everyday pursuit of their goals by what they believe to be the best means (their response to interventions included) cannot be accorded causal significance and, in this case, precisely because of its "voluntary aspect".

The idea of causation as consequential manipulation does therefore face sociologists with something of a dilemma. There is wide agreement that one has here a more rigorously formulated, even if narrower, understanding of causation than that founded on the idea of robust dependence; yet it appears far less appropriate to, and applicable in, sociological analysis. Two main reactions on the part of sociologists have so far been apparent. One, which is perhaps best expressed by Sobel (1995, 1996), entails acceptance of the manipulative approach as that which, as it were, sets the standard for the making of causal inferences. Sociologists should therefore seek wherever possible to conduct research on an experimental or at least quasi-experimental

[7] It might be thought that similar problems with experiments to those envisaged in the text could also arise in applied natural science. For example, the (perhaps apocryphal?) case is sometimes cited of an agricultural experiment in which the treatment of certain plots resulted in very heavy crops which then, however, attracted large numbers of foraging birds, so that the eventual yield on these plots was less than on those not treated. But the birds just wanted to eat: they were not trying to stop the treatment working by countering its effects. Again, there are well-known problems of how to take into account patient non-compliance in clinical trials, which clearly involves action (or inaction) on the part of patients. But it would still not be generally supposed that patients have the objective of actually subverting trials.

basis and, if this is not possible, still to take this approach as providing the conceptual framework within which the validity of causal inferences should be judged – discomfiting though this may often be. The other, contrasting reaction is that to be found most fully argued in the work of Lieberson (1985). This entails a straight rejection of the attempt to impose the experimental model (or, at any rate, that adopted in medical or agricultural research) onto sociology, on the grounds that this represents an undue "scientism" – i.e. an undue regard for the form rather than the substance of scientific method – and with the implication, then, that sociologists have to find their own ways of thinking about causation, proper to the kinds of research that they can realistically carry out and the problems that they can realistically address.

The position that I would myself wish to take up in this regard, while representing an appreciative response to those of both Sobel and Lieberson, is one that is more strongly influenced by the third understanding of causation that I initially identified, that of causation as generative process.

## Causation as Generative Process

This idea of causation has been advanced by statisticians in several versions. It does not, though, to the same extent as the two understandings of causation already considered reflect specifically statistical thinking. It would appear to derive, rather, from an attempt to spell out what must be *added to* any statistical criteria before an argument for causation can convincingly be made. Thus, Cox (1992, p. 297) introduces the idea in noting a "major limitation" of the manipulative approach to causation – and likewise, it would seem, of the approach via robust dependence (cf. Cox and Wermuth, 1996, p. 220-221): namely, that "no explicit notion of an underlying process" is introduced – no notion of a process "at an observational level that is deeper than that involved in the data under immediate analysis". Similarly, Simon and Iwasaki have maintained that, in moving from association to causation, more must be entailed than just time precedence or manipulation in establishing the necessary asymmetry – i.e. that X has causal significance for Y rather than *vice versa*. The assumption must also be present that the association is created by some "mechanism" operating "at a more microscopic level" than that at which the association is established (1988, p. 157). In other words, these authors would alike insist (and cf. also Freedman, 1991, 1992a,b) on tying the concept of causation to some process existing in time and space, even if not perhaps directly observable, that actually generates the causal effect of

X on Y and, in so doing, produces the statistical relationship that is empirically in evidence. At the same time, it should be said, they would also recognise that the accounts that are advanced of such causal processes, in order to illuminate the "black boxes" left by purely statistical analysis, can never be taken as definitive. They must in all cases be ones that are open to empirical test; and even where they appear to be supported, it has still to be accepted that finer-grained accounts, at some yet deeper level, will in principle always be possible.[8]

Such an approach to causation is clearly seen by its proponents as being essentially that which prevails, even if only implicitly, in general scientific practice (cf. Cox, 1992, p. 297; Simon and Iwasaki, 1988, p. 149-151; Freedman, 1991) and, presumably, in non-experimental as well as experimental fields. In fact, the subject-matter area in which this approach has perhaps been developed most explicitly is that of epidemiology (see e.g. Bradford Hill,1937/1991, 1965); and it is at all events this that provides the obvious paradigm case – that of smoking and lung cancer. Statistical analysis of observational data was able to show a strong association between smoking and lung cancer and, further, that this was robust to the introduction of a range of possible "common" causal factors. But what was crucial to the claim for a causal link was the elaboration of an underlying, generative process on the basis of the isolation of known carcinogens in cigarette smoke, histopathological evidence from the bronchial epithelium of smokers and so on. Freedman (1997, p. 129) emphasises the diversity of sources from which the evidence that supports the proposed generative process derives, and notes that its force "depends on the complex interplay among these various studies and the [statistical] data-sets".

As I have said, those statisticians who have upheld the idea of causation as generative process have tended to represent it as a necessary augmentation of the two understandings of causation earlier examined. But whether the same relationship is in both cases involved might be questioned. In regard to causation as robust dependence,

[8] As Suppes (1970, p. 91) has aptly observed, the accounts of causal processes or mechanisms given by one generation become themselves the "black boxes" for the next. It may be added that it is essentially Holland's recognition of this point that leads him to wish to concentrate, as a statistician, on determining the effects of causes rather than the causes of effects – "on what can be done well rather than on what we might like to do, however poorly" (1988, p. 451). But it could be replied, first, that this is to be unduly discouraged by what is a quite general feature of the pursuit of scientific knowledge; and second that, at least in the case of sociology, what can be done well and less well by statistics appears less clear-cut than Holland might suppose (cf. Smith, 1991).

causation as generative process would indeed seem an obvious complement. It at once allows for the objection that causation cannot be established simply through general procedures of statistical inference, without need for subject-matter input. If some account is required of the processes that are believed to be creating the statistically demonstrated dependence, then this account will have to be given largely on the basis of subject-matter knowledge; and the more thoroughly the account is informed by prevailing theory, rather than being merely *ad hoc*, the more coherent – and testable – it will be (cf. Bradford Hill, 1965; Cox and Wermuth, 1996, p. 225-226).

However, in regard to causation as consequential manipulation, the idea of causation as generative process would appear not just as complement but also in certain respects as corrective. To begin with, a focus on just how causal effects are brought about serves to reduce the significance accorded to different kinds of independent variable. Thus, even if it is thought improper to speak of an attribute as being a true cause of, rather than merely associated with, a dependent variable, the key issue can still be seen as that of how the relationship, however labelled, is actually produced. For example, even if "She did well on the exam because she is a woman" is taken to refer to the fixed attribute of sex (rather than to potentially changeable gender), what is important is the nature and validity of the account given of the process that underlies the association appealed to – as, say, an account on the lines that the hard-wiring of females' brains has evolved in ways that give them an advantage over men in the kind of examination in question. And at the same time, in a social science context, the attaching of causal significance to action, far from being a source of difficulty, could rather be taken as the *standard* way of constructing an account of a causal process: "She did well on the exam because she studied for it" is no longer in any way problematic.[9]

Furthermore, it is also important to recognise that an emphasis on causal processes serves to direct attention back to the question of the causes of effects as opposed to that of the effects of (assumed or, supposedly, known) causes (cf. Smith, 1991). In turn, a shift is implied away from the strong "verificationist" position which would see the purpose of causal analysis as being to determine the effects of causes, via experimental methods, in a "once-for-all" way, and which, as well as being open to some philosophical doubts, is in any event scarcely supportable in sociological practice. An understanding of causation in

[9] Nor would be: "They took measures to counter the policy intervention because they believed it was detrimental to their interests".

terms of generative processes consorts far better in fact with a "falsificationist" position. Hypothetical but adequate accounts of such processes are advanced – i.e. the processes envisaged would in principle be capable of generating the statistical relationships addressed – and further empirical inquiry is then undertaken to try to test whether it is these processes that are actually at work. This might well lead to a negative result, but even a positive one would remain no more than provisional since, as earlier remarked, it is accepted that final accounts of causal processes will never be reached.

If, then, the idea of causation as generative process can be seen not only as augmenting the ideas of causation as robust dependence and of causation as consequential manipulation but also, in the latter case, as entailing some degree of modification and reorientation, a basis does, I believe, become discernible on which an alternative approach to causal analysis, appropriate to sociological inquiry, might be developed. That is, one that would enable sociologists to go beyond the merely "pragmatic utilisation" of the concept of causation as, say, through unreflective causal modelling, without, however, requiring them to take up an understanding of causation too restrictive to allow them to pursue their own legitimate purposes.

## An Alternative for Sociology

The approach to causal analysis that is here proposed, in part drawing on and in part elaborating a position that I have already to some extent developed elsewhere (see especially Goldthorpe, 1996a, 1998), is presented in the form of a three-phase sequence: (i) establishing the phenomena that form the *explananda*; (ii) hypothesising generative processes at the level of social action; and (iii) testing the hypotheses. It should, however, be stressed that such a presentation is intended primarily to ease exposition. In practice, the three phases are unlikely to be so readily separable in any particular piece of sociological work as this schematic treatment might suggest.

### *(i) Establishing the Phenomena*

This phrase is taken from Merton (1987), who seeks to make the seemingly obvious but, as he shows, often neglected point that before advancing explanations of social phenomena, sociologists would do well to have good evidence that these phenomena really exist and *that they express sufficient regularity to require and allow explanation*. Merton's emphasis on regularity has here particular importance. To begin with, it would seem necessary for sociologists to recognise that their explanatory concerns are in fact with regularities rather than singularities, such as, say, individual lives or unique historical events.[10] And further, the nature of the basic linkage between sociology and statistics is in this way clearly brought out. If sociologists' *explananda* consist of social regularities of one kind or another, then statistics is, if not the only, at all events the most reliable and versatile means of demonstrating that such regularities exist and of clarifying their nature; and especially so, it might be added, in the case of regularities that are not readily apparent to the "lay members" of a society in the course of their everyday lives but are revealed only through the – perhaps rather sophisticated – analysis of data that have been collected extensively in time or space.

However, establishing the phenomena is an essentially descriptive exercise and in so far as it is achieved statistically it is statistics in descriptive mode that will be relevant. In this connection, it is of interest to note that various critics of current causal modelling methods in sociology (e.g. Lieberson, 1985, p. 213-219 especially; Freedman, 1992a,b; Abbott, 1997), have regretted the way in which enthusiasm for such methods has led to the disparagement of overtly descriptive statistical work, and would in effect join with Merton in urging on sociologists the importance of using quantitative data to show, in Lieberson's words, "what is happening" before they attempt to explain "why it is happening". What then may be suggested – as indeed the critics in question all in one way or another do – is that the whole statistical technology that has underpinned the sociological reception of the idea of causation as robust dependence, from Lazarsfeldian elaboration through to causal path analysis, should be radically re-evaluated. That is to say, instead of being regarded as a means of inferring causation directly from data, its primary use should rather be seen *as* descriptive, involving the analysis of joint and conditional distributions in order to determine no more than patterns of association (or correlation). Or, at very most, representations of the data might serve to *suggest* causal accounts, which, however, will need always to be further developed theoretically and then tested as quite separate undertakings.[11] Moreover, once the independent role of description is in

[10] I have elsewhere (Goldthorpe, 1997a,b) developed a critique of the contrary view, and in particular of the attempted blurring of sociological and historical concerns.

[11] In this regard, the use of graph theoretical representations of structures of conditional independence and association among variables would seem to have potential value (cf. Cox and Wermuth, 1996), although this method has not so far been widely applied in sociology. Computerised algorithms have also been developed to search for possible representations of this kind on the basis of

this way accepted, a range of other statistical techniques than those that have been aimed at causal analysis would seem capable of making a major contribution: for example, loglinear (and related) methods of analysing categorical data, where no distinction between independent and dependent variables need be entailed and attention centres specifically on structures of association and interaction; or again, as Abbott (1997) argues, various non-probabilistic techniques of scaling, clustering and sequencing that are even more clearly dedicated to descriptive tasks.

What gives arguments for the importance of description their real force is not just that instances can readily be found in the sociological literature of the recent past of what might be regarded as “premature” causal analysis – i.e. instances in which causal models were applied that later descriptive work showed to be based on mistaken suppositions (cf. Goldthorpe, 1996a). In addition, and more positively, various cases can also be cited in which the chief statistical accomplishment has been to identify and characterise important social regularities that were hitherto unappreciated, or incorrectly understood, by in effect separating out these regularities from their particular contexts. For example, loglinear modelling has been applied to demonstrate how temporal constancy and a large degree of cross-national commonality in relative rates of social mobility – or patterns of social fluidity – can underlie historically and geographically specific and often widely fluctuating absolute rates (Hauser *et al*., 1975; Featherman, Jones and Hauser, 1975; Goldthorpe, 1987; Erikson and Goldthorpe, 1992). Likewise, sequential logit modelling, as pioneered by Mare (1981), has been used in order to show up more or less constant class differentials in educational attainment during eras in which educational provision has steadily expanded and in which the “effects” of class origins on educational attainment overall would thus appear to decline – that is, simply on account of increased rates of participation (see e.g. Shavit and Blossfeld (eds.), 1993). Or again, event history analysis has enabled uniformities in the pattern of life-course events in relation to family formation or dissolution to be distinguished across periods and places characterised by widely differing political, economic and social conditions (Blossfeld and Huinink, 1991; Blossfeld and Rohwer, 1995a). It is important that the use of rather advanced statistical techniques for these purposes of what might be called sophisticated description should

correlation matrices from particular data-sets. For a lively debate on what might or might not be thus contributed to the understanding of causation, see McKim and Turner (eds.) (1997).

be clearly distinguished from their use in attempts at deriving causal relations directly from data analysis.

*Hypothesising Generative Processes*

Social regularities, once relatively securely established by descriptive methods, are then to be regarded as the basic *explananda* of sociological analysis: sociological problems are ones that can all in one way or another be expressed in terms of social regularities – their formation, continuity, interrelation, change, disruption etc.[12] When, therefore, analysis becomes causal, social regularities represent the effects for which causes have to be discovered. And this task, contrary to what proponents of the idea of causation as robust dependence would seem to have supposed, cannot be a purely statistical one but requires a crucial subject-matter input.

From the position of methodological individualism that I would here adopt – and from which most of the critiques earlier noted of a purely "variable sociology" explicitly or implicitly derive – this input has then to take the form of some account of the action and interaction of individuals. In effect, a narrative of action must be provided that purports to capture the central tendencies that arise within the diverse courses of action that are followed by particular actors in situations of a certain type: i.e. situations that can be regarded as sharing essential similarities in so far as actors' goals and the nature of the opportunities and constraints that condition their action in pursuit of these goals are concerned. And, in turn, a case must be made to show how these central tendencies in action would, if operative, actually give rise, through their intended and unintended consequences, to the regularities that constitute the *explananda.*

The theory that underlies such hypothesised processes will then obviously be a theory of social action of some kind; and, in this respect, the two main alternatives that would appear available might for convenience be labelled as rational action theory and norm-oriented action theory. On grounds I have set out elsewhere (1998), I would

[12] It is important to note that such problems do in fact arise not only, as it were, endogenously to the development of sociology but also exogenously to this development – most obviously, perhaps, from various kinds of applied, even purely "administrative", social research. While I would then entirely agree with authors such as Hedström and Swedberg (1998a,b) in their insistence that the main requirement of theory is that it should explain, I believe that they place a too exclusive emphasis on the role of theory in the discovery of problems and, correspondingly, underestimate that of empirical research – and especially of large-scale survey research – with primarily descriptive goals (cf. Erikson, 1998).

regard the former as having conceptual, explanatory and interpretative privilege over the latter, though quite possibly needing to be complemented by it. Rational action theory allows for the fuller expression of the idea of reasons as causes for action; and an appeal to the rationality of action, in the sense of its grounding in what for actors are good reasons for their actions, in terms of perceived costs and benefits, represents a uniquely attractive end-point for any sociological explanation to reach. However, for present purposes, the important point is that *whatever* theory of action is favoured, it should be used to enable as explicit and coherent a formulation as possible of the generative processes that are proposed and in this way facilitate their evaluation in terms of both their causal adequacy and their empirical presence.[13]

In particular, it is at this stage that questions of what might be called causal form and causal hierarchy should be clarified. Thus, authors such as Lieberson (1985, Ch. 4 especially) and Blossfeld and Rohwer (1995b, Ch. 1) have stressed the need to specify whether causal processes are seen as symmetrical or, rather, one-way and irreversible, and whether they entail lags, thresholds or other distinctive temporal features in their effects. And Lieberson (1985, Ch. 7 especially) has further emphasised the need to distinguish between "basic" causal processes and ones of a more "superficial" kind (the former often being less open to direct observation than the latter). Thus, to revert to an earlier example, if differentials in educational attainment are in fact treated as a basic cause of income inequalities among classes or ethnic groups, then action – such as some kind of political intervention – that brought about a reduction in these differentials would be expected to close income gaps also. But if educational differentials are seen as only a superficial cause of income inequalities, with the basic cause lying elsewhere – say, in processes grounded in more generalised social inequalities or in discrimination – then what would be expected to follow from their reduction would not be a corresponding decrease in income inequalities but simply changes *consequent upon the latter remaining unaltered*: for

[13] A concern for the theoretical basis of hypothesised generative processes is also important to prevent purely *ad hoc* switching – as occurs where, say, sociologists draw on rational action theory in explanations of regularities in the class-vote relationship but then on norm-oriented action theory in explanations of why individuals vote at all. Such switching *may* be appropriate but the grounds for it have always to be spelled out: i.e. the attempt should be made to specify which kinds of process will operate under which conditions. As I have argued elsewhere (1996a), there are dangers in thinking that sociologists can simply accumulate a collection of models of causal processes or mechanisms of many different kinds, items from which can then be used (or discarded) just as seems convenient.

instance, a weakening of the association between education and income while, perhaps, that between other factors – say, family contacts – and entry into well-paid employment became stronger.

*Testing the Hypotheses*

As earlier indicated, the first test of any causal explanation of a social regularity that is put forward must be that of its adequacy: would the generative process hypothesised, assuming it to be operative, in fact be capable of producing the regularity in question? It is here worth pointing out that the fuller and more refined the description of the regularity, the stronger the explanatory demands that will be made and the more likely it is that certain candidate accounts can be eliminated at this stage.[14] However, it may be supposed that more than one adequate account will be possible, and further testing is then required to try to determine which – if any – of the processes hypothesised is actually at work. In other words, the issue shifts from that of the adequacy in principle of an account of a causal process to that of its empirical validity.

In this connection, what crucially matters are the implications that follow from any account that is advanced: if the generative process suggested does in fact operate to produce, or help to produce, an established regularity, then what *else* should be empirically observable? It may be that the process, or at least some features of it, should be observable directly; but if the action and interaction of relatively large numbers of individuals is involved or interaction that is not of a localised,

[14] For example, once it is recognised through descriptive work of the kind referred to in the text above that what needs to be explained about class differentials in education is why they have in most societies remained little changed *in a context of generally increasing rates of educational participation,* it at once becomes apparent that "culturalist" accounts (e.g. Bourdieu, 1973; Willis, 1977) do not meet the initial requirement of causal adequacy. If the main source of these differentials were indeed to lie in radically divergent class subcultures, with working-class families attaching a lower value to education than families in more advantaged class positions and their children being thus systematically alienated from the educational system, then what one would have to expect in course of the expansion of this system would be *widening* differentials. But there is no evidence of this. Working-class children have in fact taken up expanding educational opportunities at much the same rate as children of other class origins, although they have not in most countries being able to exploit these new opportunities to the extent necessary to close the attainment gap. I have elsewhere attempted, together with Richard Breen (Goldthorpe, 1996b; Breen and Goldthorpe, 1997) to develop an alternative account of educational decision-making in different class situations that is adequate to explaining the regularities empirically established – and likewise those revealing declining gender differentials.

face-to-face kind, then this may scarcely be feasible.[15] The alternative is to devise more indirect tests by specifying other effects to which the process should give rise apart from those constituting the regularities it purports to explain, although likewise of an empirically ascertainable kind. Such direct or indirect tests may be made through whatever methods appear most appropriate; and it is indeed important that separate tests of particular implications should be undertaken, and repeated, on the basis of different data-sets and analytical techniques (cf. Berk, 1988).[16] Thus, while it might seem that, at this stage, attention does after all come to focus on the effects of – given – causes rather than on the causes of effects, this is within the context not of randomised experimental design but of (what should be) a theoretically informed account of a generative process that is subject to ongoing evaluation, and with the outcome being falsification or, if testing is withstood, simply corroboration, rather than the verification of effects of a "once-and-for-all" kind.

To illustrate, one could take the case of the consequences for children of parents' marital break-up.[17] An association would appear to be established between break-up, on the one hand, and, on the other, children leaving school at the minimum age and experiencing various other seemingly adverse effects. But disagreement arises over whether, or how far, break-up can be attributed causal significance in these respects. For instance, it is not difficult to think of possible "common" causes – say, personality factors or parental conflict – that could lie behind both marital instability *and* poor parenting and its consequences for children. The key issue may then be regarded as that of whether the children of those couples who *do* break up would have fared better if their parents had in fact stayed together, and in this way Holland's "Fundamental Problem of Causal Inference" is directly encountered: the

[15] A related issue that arises here is that of the part to be played in the testing of accounts of generative processes at the level of social action by "subjective" data: i.e. data that relate directly to individuals' orientations towards, and definitions of, the situations in which they act. While there can be no objection to the use of such data in principle, legitimate doubts at the level of practice, and especially regarding data quality, do persist. For relevant discussion, see Opp (1998) and Erikson (1998).

[16] It is of course quite likely that the data-sets that are used to establish particular social regularities will not be those most suitable, from the point of view of the information they contain, for purposes of testing supposed generative processes. This points up the importance of recognising the distinction between these phases of inquiry.

[17] What follows is much influenced by, and draws on, an as yet unpublished paper by Máire Ní Bhrolcháin, "'Divorce Effects' and Causality in the Social Sciences".

same couple cannot both break up and not break up. Moreover, a statistical solution via experimental design is here scarcely possible; and from the point of view of causation as consequential manipulation, the strategy to be pursued would then have to be that of viewing break-up as if it were an intervention, and attempting to overcome the "assignment" problem by introducing a set of relevant covariates into the analysis: i.e. so that a comparison could be made between the children of parents who did and who did not break up on the basis of, as it were, "all else equal to the time of break-up". However, as earlier remarked, it remains far from clear how the completeness of such a set could ever be determined and definitive results thus claimed, any more than they could be from the standpoint of causation as robust dependence. It would indeed appear that the more attention analysts have given to the problems of defining and including appropriate covariates, the more sceptical their conclusions have become (see especially Ní Bhrolcháin, Chappell and Diamond, 1994).

The alternative strategy that is here proposed is that those who wish to investigate what, causally, underlies the association between marital break-up and adverse features of children's future lives should begin by spelling out as fully as they are able the way or ways in which they believe that the effects in question are produced – i.e. by giving accounts of adequate generative processes; and that these accounts should then be empirically tested, by reference to their further implications, as extensively as possible. The more detailed the accounts are, the more likely it is that they will differ in their implications so as to allow critical comparisons to be made: as, say, between children who have lost a parent through marital dissolution and those who have lost a parent through death; between siblings who experience their parents' break-up at different ages; between children who remain with a single parent after break-up and those who acquire a step-parent; between children experiencing break-up in differing contexts, in terms of prevailing rates of break-up, the extent of social support for single parents etc.

In fact, one recent contribution can be taken as marking at least a first step in seeking to implement such a strategy. Jonsson and Gähler (1997), considering the possible effects of marital break-up on children's educational attainment in Sweden, first identify a number of "plausible causal mechanisms" and then carry out analyses on a large-scale longitudinal data-set in order to test for the presence of such mechanisms. Interestingly, the mechanism, or generative process, for which strongest corroboration was found was one that had received little previous attention in the debate: that is, a "downward mobility" process through which, when children are separated from the parent with the higher educational and/or occupational achievement, their own educational and occupational aspirations tend to fall (see also Gähler, 1998). There are in fact quite close analogies here, via the "structural" theory of aspirations (Keller and Zavalloni, 1964), with processes that have been suggested, and have received some support, in explaining persistence and change in class and gender differentials in educational attainment more generally (cf. Boudon, 1974; Gambetta, 1987; Goldthorpe, 1996b; Breen and Goldthorpe, 1997). None the less, it is important to note that what the authors claim is still only evidence for, and not definitive proof of, the operation of such a process, and they are careful to point out what might prove to be special features of the Swedish case.[18]

For the present, the – diversified and repeated – testing of suggested generative processes on the basis of particular implications derivable from them is, perhaps, the most that can be asked for. It should, though, finally be said that the logical conclusion to which the entire approach outlined would lead is that of testing on the basis of statistical models *of these processes themselves*. The important distinction in this regard is that made by Cox (1990) between "empirical" and "substantive" statistical models, or by Rogosa (1992) between statistical models *per se* and scientific models expressed in statistical form (cf. also Sørensen, 1998). Models of the former kind are those which sociologists normally use and are concerned with relations among variables that may be determined through techniques of rather general applicability. Models of the latter kind, however, are intended to represent real processes that have causal force (whether or not directly observable). They are therefore crucially informed by subject-matter theory and can in turn serve as the vehicles through which such theory is exposed to test in a fairly comprehensive way. In particular, as Cox has observed, it should be possible for such models to be applied in simulation exercises: "The essential idea is that if the investigator cannot use the model directly to simulate artificial data, how can 'Nature' [or, one could add, 'Society' –

[18] Another relevant study, though more psychological in orientation, is that reported by Rutter (1981; and cf. also 1994) who advances the hypothesis that, in explaining the association between marital break-up and children's disorderly behaviour, the "mediating mechanism" is tension resulting from marital conflict rather than the break-up itself. Rutter is then able to show how this hypothesis can be tested, again on the basis of longitudinal data, through the comparison of cases of temporary separations arising from marital conflict and for other reasons and also of cases where, following such a separation associated with conflict, a reduction or increase in conflict was subsequently recorded.

JHG] have used anything like that method to generate real data?" (1990, p. 172). In sociology, accounts of processes capable of producing observed regularities are not yet for the most part expressed in sufficiently specific and theoretically informed ways to permit "substantive" models to be developed – greater efforts at formalisation might help in this respect – and, correspondingly, the simulation approach to hypothesis testing is not at a very advanced stage. None the less, there are by now at least indications that its potential in helping to integrate theoretical and quantitative empirical work is becoming more fully appreciated (see e.g. Halpin, 1998).

## Conclusion

The general point that, I believe, emerges most clearly from the foregoing might be put as follows. If contributions made by statisticians to the understanding of causation are to be taken over with advantage in any specific field of inquiry, then what is crucial is that the right relationship should exist between statistical and subject-matter concerns.

Thus, it could be said that the idea of causation as robust dependence does have a certain appropriateness in so far as the main aim of research is prediction – and, in particular, prediction in the real world rather than in the laboratory or, in other words, forecasting. The importance that this idea has had in economic forecasting is not therefore at all surprising. However, where the ultimate aim of research is not prediction *per se* but rather causal explanation, an idea of causation that is expressed in terms of predictive power – as, for example, "Granger" causation – is likely to be found wanting. Causal explanations cannot be arrived at through statistical methodology alone: a subject-matter input is also required in the form of background knowledge and, crucially, theory. This is the upshot of the critiques made by sociological theorists and statisticians alike of the pragmatic or, one could say, atheoretical use of the concept of causation by quantitative sociologists on the basis of essentially "partialling" procedures from Lazarsfeldian elaboration through to causal path analysis.

Likewise, the idea of causation as consequential manipulation is apt to research that can be undertaken primarily through experimental methods and, especially, to revert to Cook and Campbell, to "practical science" where the central concern is indeed with "the consequences of performing particular acts". The development of this idea in the context of medical and agricultural research is as understandable as the development of that of causation as robust dependence within applied econometrics. However, the extension of the manipulative approach into sociology would not appear promising, other than in rather special circumstances. It is not just that in sociological research practical and ethical barriers to experiments, or interventions, often arise: it can be accepted that statisticians have made major advances in the methodology of quasi-experimental studies, even if the latter can scarcely claim to provide "once-and-for-all" results in the way that might be thought possible with true experiments. The more fundamental difficulty is that, under the – highly anthropocentric – principle of "no causation without manipulation", the recognition that can be given to the action of individuals as having causal force is in fact peculiarly limited. That is, it extends only to those actually in the role of experimenter or intervener: otherwise, what Holland calls the "voluntary aspect" of action, and including in the case of action taken in response to an intervention, creates major problems.

The idea of causation as generative process is not, in the same way as the two other ideas of causation that have been considered, linked to a particular body of statistical work. It does, none the less, appear to offer the best basis, as I have sought finally to show, on which statistical and substantive concerns can be related in causal analysis in sociology. First, it places the emphasis on the causes of effects: in other words, it implies that such analysis begins with the effects – the phenomena – for which a causal explanation is then sought. And in sociology, it is in establishing the phenomena that statistics has a basic contribution to make, in an essentially descriptive mode. Second, the idea of a generative process specified at a "deeper" or "more microscopic" level than that of the data that constitute the *explananda* fits closely with the analytical approach of at least those sociologists adhering to the principle of methodological individualism, who would thus insist on the need for causal explanations of social phenomena to be grounded ultimately in accounts of the action and interaction of individuals, and who have criticised a purely "variable sociology" from this point of view. Third, the recognition that final, definitive accounts of generative processes will never be reached means that empirical evaluations of such accounts – in regard to whether the processes they suggest do in fact operate to produce the effects attributed to them – are not expected to achieve once-and-for-all verification but either falsification or, at best, what might be described as corroboration pending improvement. Statistics has then again an evident role to play in testing such accounts via their particular, empirically ascertainable implications on, for now, a "catch-as-catch-can" basis. But this role will be enlarged in so far as sociologists are able to develop their accounts of generative processes to the point at which statistical methods can also be applied in creating “substantive” models of these processes themselves.

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