

5.1 Introduction

5.1.1 Causality in Search of a Language

The word *cause* is not in the vocabulary of standard probability theory. It is an embarrassing yet inescapable fact that probability theory, the official mathematical language of many empirical sciences, does not permit us to express sentences such as “Mud does not cause rain”; all we can say is that the two events are mutually correlated, or dependent – meaning that if we find one, we can expect to encounter the other. Scientists seeking causal explanations for complex phenomena or rationales for policy decisions must therefore supplement the language of probability with a vocabulary for causality, one in which the symbolic representation for the causal relationship “Mud does not cause rain” is distinct from the symbolic representation for “Mud is independent of rain.” Oddly, such distinctions have yet to be incorporated into standard scientific analysis.¹

Two languages for causality have been proposed: path analysis or structural equation modeling (SEM) (Wright 1921; Haavelmo 1943); and the Neyman-Rubin potential-outcome model (Neyman 1923; Rubin 1974). The former has been adopted by economists and social scientists (Goldberger 1972; Duncan 1975), while a group of statisticians champion the latter (Rubin 1974; Robins 1986; Holland 1988). These two languages are mathematically equivalent (see Chapter 7, Section 7.4.4), yet neither has become standard in causal modeling—the structural equation framework because it has been greatly misused and inadequately formalized (Freedman 1987) and the potential-outcome framework because it has been only partially formalized and (more significantly) because it rests on an esoteric and seemingly metaphysical vocabulary of counterfactual variables that bears no apparent relation to ordinary understanding of cause-effect processes (see Section 3.6.3).

Currently, potential-outcome models are understood by few and used by even fewer. Structural equation models are used by many, but their causal interpretation is generally questioned or avoided, even by their leading practitioners. In Chapters 3 and 4 we described how

¹A summary of attempts by philosophers to reduce causality to probabilities is given in Chapter 7 (Section 7.5).

structural equation models, in nonparametric form, can provide the semantic basis for theories of interventions. In Section 1.4 we outlined how these models provide the semantical basis for a theory of counterfactuals as well. It is somewhat embarrassing that, these distinctive features are hardly recognized and rarely utilized in the modern SEM literature. The current dominating philosophy treats SEM as just a convenient way to encode density functions (in economics) or covariance information (in social science). Ironically, we are witnessing one of the most bizarre circles in the history of science: causality in search of a language and, simultaneously, the language of causality in search of its meaning.

The purpose of this chapter is to formulate the causal interpretation and outline the proper use of structural equation models, thereby reinstating confidence in SEM as the primary formal language for causal analysis in the social and behavioral sciences. First, however, we present a brief analysis of the current crisis in SEM research in light of its historical development.

5.1.2 SEM: How its Meaning Became Obscured

Structural equation modeling was developed by geneticists (Wright 1921) and economists (Haavelmo 1943; Koopmans 1950, 1953) so that qualitative cause-effect information could be combined with statistical data to provide quantitative assessment of cause-effect relationships among variables of interest. Thus, to the often asked question, “Under what conditions can we give causal interpretation to structural coefficients?” Wright and Haavelmo would have answered, “Always!” According to the founding fathers of SEM, the conditions that make the equation $y = \beta x + \epsilon$ *structural* are precisely those that make the causal connection between X and Y have no other value but β and ensure that nothing about the statistical relationship between x and ϵ can ever change this interpretation of β . Amazingly, this basic understanding of SEM has all but disappeared from the literature, leaving modern econometricians and social scientists in a quandary over β .

Most SEM researchers today are of the opinion that extra ingredients are necessary for structural equations to qualify as carriers of causal claims. Among social scientists, James, Mulaik, and Brett (1982,

p. 45), for example, stated that a condition called *self-containment* is necessary for consecrating the equation $y = \beta x + \epsilon$ with causal meaning, where self-containment stands for $\text{cov}(x, \epsilon) = 0$. According to James et al. (1982), if self-containment does not hold then “neither the equation nor the functional relation represents a causal relation.” Bollen (1989, p. 44) reiterated the necessity of self-containment (under the rubric *isolation* or *pseudo-isolation*)—contrary to the understanding that structural equations attain their causal interpretation prior to, and independently of, any statistical relationships among their constituents. Since the early 1980s, it has become exceedingly rare to find an open endorsement of the original SEM logic: that β defines the sensitivity of $E(Y)$ to experimental manipulations of X ; that ϵ is defined in terms of β , not the other way around; and that the orthogonality condition $\text{cov}(x, \epsilon) = 0$ is neither necessary nor sufficient for the causal interpretation of β (see Sections 3.6.2 and 5.4.1).² It is therefore, not surprising that many SEM textbooks have given up on causal interpretation altogether: “We often see the terms cause, effect, and causal modeling used in the research literature. We do not endorse this practice and therefore do not use these terms here” (Schumaker and Lomax 1996, p. 90).

Econometricians have just as much difficulty with the causal reading of structural parameters. Leamer (1985, p. 258) observed, “It is my surprising conclusion that economists know very well what they mean when they use the words ‘exogenous,’ ‘structural,’ and ‘causal,’ yet no textbook author has written adequate definitions.” There has been little change since Leamer made these observations. Econometric textbooks invariably devote most of their analysis to estimating structural parameters, but they rarely discuss the role of these parameters in policy evaluation. The few books that deal with policy analysis (e.g. Goldberger 1991; Intriligator et al. 1996, p. 28) assume that policy variables satisfy the orthogonality condition by their very nature, thus rendering structural information superfluous. Hendry (1995, p. 62), for instance, explicitly tied the interpretation of β to the orthogonality condition, stating as follows:

²In fact, this condition is not necessary even for the *identification* of β , once β is interpreted (see the identification of α in Figures 5.7 and 5.9).

the status of β may be unclear until the conditions needed to estimate the postulated model are specified. For example, in the model:

$$y_t = z_t\beta + u_t \text{ where } u_t \sim \text{IN}[0, \sigma_u^2],$$

until the relationship between z_t and u_t is specified the meaning of β is uncertain since $E[z_t u_t]$ could be either zero or nonzero on the information provided.

LeRoy (1995, p. 211) goes even further: “It is a commonplace of elementary instruction in economics that endogenous variables are not generally causally ordered, implying that the question ‘What is the effect of y_1 on y_2 ’ where y_1 and y_2 are endogenous variables is generally meaningless.” According to LeRoy, causal relationships cannot be attributed to any variable whose causes have separate influence on the effect variable, a position that denies any causal reading to most of the structural parameters that economists and social scientists labor to estimate.

Cartwright (1995b, p. 49), a renowned philosopher of science, addresses these difficulties by initiating a renewed attack on the tormenting question, “*Why* can we assume that we can read off causes, including causal order, from the parameters in equations whose exogenous variables are uncorrelated?” Cartwright, like SEM’s founders, recognizes that causes cannot be derived from statistical or functional relationships alone and that causal assumptions are prerequisite for validating any causal conclusion. Unlike Wright and Haavelmo, however, she launches an all-out search for the assumptions that would endow the parameter β in the regression equation $y = \beta x + \epsilon$ with a legitimate causal meaning and endeavors to prove that the assumptions she proposes are indeed sufficient. What is revealing in Cartwright’s analysis is that she does not consider the answer Haavelmo would have provided—namely, that the assumptions needed for drawing causal conclusions from parameters are communicated to us by the scientist who declared the equation “structural”; they are already encoded in the *syntax* of the equations and can be read off the associated graph as easily as a shopping list;³

³These assumptions are explicated and operationalized in Section 5.4. Briefly,

they need not be searched for elsewhere, nor do they require new proofs of sufficiency. Again, Haavelmo's answer applies to models of any size and shape, including models with correlated exogenous variables.

These examples bespeak an alarming tendency among economists and social scientists to view a structural equation as an algebraic object that carries functional and statistical assumptions but is void of causal content. This statement from one leading social scientist is typical: "It would be very healthy if more researchers abandoned thinking of and using terms such as cause and effect" (Muthen 1987, p. 180). Perhaps the boldest expression of this tendency was voiced by Holland (1995, p. 54): "I am speaking, of course, about the equation: $\{y = a + bx + \epsilon\}$. What does it mean? The only meaning I have ever determined for such an equation is that it is a shorthand way of describing the conditional distribution of $\{y\}$ given $\{x\}$."⁴

The founders of SEM had an entirely different conception of structures and models. Wright (1923, p. 240) declared that "prior knowledge of the causal relations is assumed as prerequisite" in the theory of path coefficients, and Haavelmo (1943) explicitly interpreted each structural equation as a statement about a hypothetical controlled experiment. Likewise, Marschak (1950), Koopmans (1953), and Simon (1953) stated that the purpose of postulating a structure behind the probability distribution is to cope with the hypothetical changes that can be brought about by policy. One wonders, therefore, what has hap-

if G is the graph associated with a causal model that renders a certain parameter identifiable, then two assumptions are sufficient for authenticating the causal reading of that parameter: (1) every missing arrow, say between X and Y , represents the assumption that X has no effect on Y once we intervene and hold the parents of Y fixed; and (2) every missing bidirected arc $X \longleftrightarrow Y$ represents the assumption that all omitted factors that affect Y are uncorrelated with those that affect X . Each of these assumptions is *testable* in experimental settings, where interventions are feasible (Section 5.4.1).

⁴All but forgotten, the structural interpretation of the equation (Haavelmo 1943) poses no restriction whatsoever on the conditional distribution of $\{y\}$ given $\{x\}$. Paraphrased in our vocabulary, it reads: "In an ideal experiment where we control X to x and any other set Z of variables (not containing X or Y) to z , Y will attain a value y given by $a + bx + \epsilon$, where ϵ is a random variable that is (pointwise) independent of the settings x and z " (see Section 5.4.1). This implies $E[Y|do(x), do(z)] = a + bx + c$ but says nothing about $E(Y|X = x)$.

pened to SEM over the past 50 years, and why the basic (and still valid) teachings of Wright, Haavelmo, Marschak, Koopmans, and Simon have been forgotten.

Some economists attribute the decline in the understanding of structural equations to Lucas's (1976) critique, according to which economic agents anticipating policy interventions would tend to act contrary to SEM's predictions, which often ignore such anticipations. However, since this critique merely shifts the model's invariants and the burden of structural modeling—from the behavioral level to a deeper level that involves agents' motivations and expectations—it does not exonerate economists from defining and representing the causal content of structural equations at some level of discourse.

I believe that the causal content of SEM has gradually escaped the consciousness of SEM practitioners mainly for the following reasons:

1. SEM practitioners have sought to gain respectability for SEM by keeping causal assumptions implicit, since statisticians, the arbiters of respectability, abhor assumptions that are not directly testable.
2. The algebraic language that has dominated SEM lacks the notational facility needed to make causal assumptions, as distinct from statistical assumptions, explicit. By failing to equip causal relations with precise mathematical notation, the founding fathers in fact committed the causal foundations of SEM to oblivion. Their disciples today are seeking foundational answers elsewhere.

Let me elaborate on the latter point. The founders of SEM understood quite well that, in structural models, the equality sign conveys the asymmetrical relation “is determined by” and hence behaves more like an assignment symbol ($:=$) in programming languages than like an algebraic equality. However, perhaps for reasons of mathematical purity, they refrained from introducing a symbol to represent the asymmetry. According to Epstein (1987), in the 1940s Wright gave a seminar on path diagrams to the Cowles Commission (the breeding ground for SEM), but neither side saw particular merit in the other's methods. Why? After all, a diagram is nothing but a set of nonparametric

structural equations in which, to avoid confusion, the equality signs are replaced with arrows.

My explanation is that the early econometricians were extremely careful mathematicians who thought they could keep the mathematics in purely equational-statistical form and just reason about structure in their heads. Indeed, they managed to do so surprisingly well, because they were truly remarkable individuals who *could* do it in their heads. The consequences surfaced in the early 1980s, when their disciples began to mistake the equality sign for an algebraic equality. The upshot was that suddenly the “so-called disturbance terms” did not make any sense at all (Richard 1980, p. 3). We are living with the sad end to this tale. By failing to express their insights in mathematical notation, the founders of SEM brought about the current difficulties surrounding the interpretation of structural equations, as summarized by Holland’s “What does it mean?”

5.1.3 Graphs as a Mathematical Language

Recent developments in graphical methods promise to bring causality back into the mainstream of scientific modeling and analysis. These developments involve an improved understanding of the relationships between graphs and probabilities, on the one hand, and graphs and causality, on the other. But the crucial change has been the emergence of graphs as a mathematical language. This mathematical language is not simply a heuristic mnemonic device for displaying algebraic relationships, as in the writings of Blalock (1962) and Duncan (1975). Rather, graphs provide a fundamental notational system for concepts and relationships that are not easily expressed in the standard mathematical languages of algebraic equations and probability calculus. Moreover, graphical methods now provide a powerful symbolic machinery for deriving the consequences of causal assumptions when such assumptions are combined with statistical data.

A concrete example that illustrates the power of the graphical language—and that will set the stage for the discussions in Sections 5.2 and 5.3—is Simpson’s paradox, discussed in Section 3.3 and further analyzed in Section 6.1. This paradox concerns the reversal of an association between two variables (e.g., gender and admission to school)

that occurs when we partition a population into finer groups, (e.g., departments). Simpson's reversal has been the topic of much statistical research since its discovery in 1899. This research has focused on conditions for escaping the reversal instead of addressing the practical questions posed by the reversal: "Which association is more valid, before or after partitioning?" In linear analysis, the problem surfaces through the choice of regressors—for example, determining whether a variate Z can be added to a regression equation without biasing the result. Such an addition may easily reverse the sign of the coefficients of the other regressors, a phenomenon known as "suppressor effect" (Darlington 1990).

Despite a century of analysis, questions of regressor selection or adjustment for covariates continue to be decided informally, case-by-case, with the decision resting on folklore and intuition rather than on hard mathematics. The standard statistical literature is remarkably silent on this issue. Aside from noting that one should not adjust for a covariate that is affected by the putative cause (X),⁵ the literature provides no guidelines as to what covariates might be admissible for adjustment and what assumptions would be needed for making such a determination formally. The reason for this silence is clear: the solution to Simpson's paradox and the covariate selection problem (as we have seen in Sections 3.3.1 and 4.5.3), rests on causal assumptions, and such assumptions cannot be expressed formally in the standard language of statistics.⁶

In contrast, formulating the covariate selection problem in the language of graphs immediately yields a general solution that is both natural and formal. The investigator expresses causal knowledge (or assumptions) in the familiar qualitative terminology of path diagrams, and once the diagram is complete, a simple procedure decides whether a proposed adjustment (or regression) is appropriate relative to the quantity under evaluation. This procedure, which we called the *back-*

⁵This advice, which rests on the causal relationship "not affected by," is (to the best of my knowledge) the *only* causal notion that has found a place in statistics textbooks. The advice is neither necessary nor sufficient, as readers can verify from the discussion of Chapter 3.

⁶Simpson's reversal, as well as the supressor effect, are paradoxical only when we attach causal reading to the associations involved; see Section 6.1.

door criterion in Definition 3.3.1, was applicable when the quantity of interest is the total effect of X on Y . If instead the direct effect is to be evaluated, then the graphical criterion of Theorem 4.5.3 is applicable. A modified criterion for identifying direct effects (i.e., a path coefficient) in linear models will be given in Theorem 5.3.1.

This example is not an isolated instance of graphical methods affording clarity and understanding. In fact, the conceptual basis for SEM achieves a new level of precision through graphs. What makes a set of equations “structural,” what assumptions are expressed by the authors of such equations, what the testable implications of those assumptions are, and what policy claims a given set of structural equations advertises are some of the questions that receive simple and mathematically precise answers via graphical methods. These and related issues in SEM will be discussed in the following sections.