

CAUSALITY AND ECONOMETRICS: COMMENT

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IN *ECONOMETRICA*, April 1954, Herman O. A. Wold has published a valuable contribution¹ to the understanding of the causal relationship. I should like to discuss briefly the relation between his definitional proposal and the one, upon which he comments on pages 166-7 of his paper, that I have advanced.

1. As Professor Wold points out, the two definitions are closely related. In fact, I am strongly of the opinion that the "intuitive" notions of causality we hold, and which we are trying to explicate by our definitions, are identical. (In his footnote, Professor Wold gives a counterexample, about which I will have more to say below.) Our analyses of causation in the case of a controlled experiment are certainly the same. In the non-experimental case, I must confess to a certain uneasiness at a vagueness in several terms of Professor Wold's definition: "The relationship is then defined as causal if it is *theoretically permissible* to regard the variables as involved in a *fictive controlled experiment* . . ." (italics mine). To apply this definition, we need some criteria as to what is "theoretically permissible," and I sought, in my definition, to supply such criteria.

2. Professor Wold questions whether my definition gives the "common sense" result when applied to the Girshick-Haavelmo model. He says: "although the model involves no explanatory relation for the price variable, Simon concludes that price is causally dependent upon four specified variables of the model." A study of the Girshick-Haavelmo model will show, I think, that the "although" clause of this statement is simply not correct. The first and second equations of the Girshick-Haavelmo model are the demand and supply equations, respectively, for food products, as the authors explicitly state in their discussion of the model. The first, second, fourth, and fifth equations jointly determine: (1) consumption of food products, (2) retail price of food products, (3) production of food products, and (4) wholesale price of food products as functions of exogenous variables and disposable income. The model, then, certainly does include explanatory relations for the prices.

Perhaps Professor Wold means that the Girshick-Haavelmo model is not an *empirically correct* description of the structural relations. With this I would agree. The four variables listed above are designated in the model as y_1 , y_2 , y_4 , and y_5 , respectively. Their structural equations are of the form: $f_1(y_1, y_2) = 0$, $f_2(y_1, y_2, y_4) = 0$, $f_4(y_4, y_5) = 0$, and $f_5(y_5, y_2) = 0$. (I have omitted f_3 , which is the equation determining disposable income, and the corresponding variable y_3 .) I think any serious attempt to derive these static equations from a set of dynamic market mechanisms would have led to a rather different set (Girshick and Haavelmo disclaim an attempt at realism): $g_1(y_1, y_2) = 0$, $g_2(y_1, y_2, y_5) = 0$,

¹ Herman O. A. Wold, "Causality and Econometrics," *ECONOMETRICA*, vol. 22, no. 2 (April 1954), pp. 162-177.

$g_4(y_4, y_6) = 0$, and $g_5(y_4, y_1) = 0$. The first two of these are the demand and supply equations at the retail level, and the last two the supply and demand equations at the wholesale level. However, the causal ordering implicit in this new set of equations is identical with that in the old: the four equations form, in both systems, a single complete subset of second order.

I believe, therefore, that my definition does, in fact, conform to "common sense" in explicating the causal relations that are implicit in the Girshick-Haavelmo model. Whether the causal relations they postulate are those that common sense would suggest to be the empirically correct ones is another question. As indicated above, I think their structural equations are probably wrong, but the suggested alteration happens not to affect the causal ordering of variables. The distinction I have just made between the common-sense character of the model, and the common-sense character of the explication of the causal relations implied by the model brings me to my final point.

3. Professor Wold thinks the common-sense meaning of causality belongs to the object language, not to the metalanguage. I agree. But I do not think this implies any fundamental difference between us. When we construct a model of the world, then to each property of the model that is invariant under "trivial" transformations (i.e., those that have no operational significance in reference to the world) there generally corresponds a property of the world the model describes.

Suppose that we draw an accurate map of the United States. What do we mean when we say: "Chicago is west of Pittsburgh"? We may mean that when we hold the map right-side up (as defined by printing conventions), a point labelled "Chicago" lies to the left of a point labelled "Pittsburgh." Or we may mean that the city denoted by the point labelled "Chicago" lies west (as determined by a compass) of the city denoted by the point labelled "Pittsburgh." By means of "correlative" or "semantic" or "operational" definitions, we set up a correspondence between certain named points on the map and certain cities in the world; and between the direction "left" on the map and the direction "west" in the world.

We have then two notions of "direction": the relation of points on a map, and the relation of cities on the globe. Similarly, in the case of causality, we have a relation between variables in a mathematical model (the relation denoted by my term "cause"); and a relation in the real world between the phenomena denoted by these variables (the relation denoted by the common-sense term "cause").

Now the common-sense word "cause" and the equations of the mathematical model both belong to the object language, because they both refer to the real world. But my term "cause" does not appear anywhere in the mathematical model. It is employed only to talk *about* (denote) a property of the model; hence it occurs in the language we use to talk about the model—a metalanguage.

It can be shown that metalinguistic statements about causal relations in the model imply testable causal relations in the real world only if the model is over-identified. The reason for this assertion will be found in my discussion in the

Hood-Koopmans volume² of the invariance of the causal relation under transformations of the model.

I should not conclude my discussion of this last point without an apology for an unfortunate vagueness in my own previous discussions of the question of whether causality is or is not metalinguistic. Perhaps the analysis above will help dispel any confusion I have created.

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² Herbert A. Simon, "Causal Ordering and Identifiability," in Wm. C. Hood and Tjalling C. Koopmans, eds., *Studies in Econometric Method*, New York: Wiley & Sons, 1953, pp. 49-74.

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