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Review of Judea Pearl's **CAUSALITY; MODELS, REASONING AND INFERENCE**. Cambridge University Press, 2000. Pp. xvi, 384.

Judea Pearl is one of the leading scholars developing formal methods for the analysis of causation. Although based in a computer science department (at UCLA), Pearl's interests are very broad: he reads and discusses the natural, health and social sciences, philosophy, statistics and, most relevant for readers of this journal, economics. In this book Pearl brings together, with varying degrees of rewriting, his major papers on causality.

Three ideas underlie everything Pearl writes:

- (1) Causal ideas are indispensable in all the above areas. Pearl rejects the calls one periodically hears to dispense entirely with the terms 'cause' and 'effect' or substitute for them terms like 'functional dependence', as if by doing so one could somehow circumvent the need to deal explicitly with causal ideas.
- (2) Causality is different from probability. Even though the two are obviously related, they are not identical, and a separate analysis is required if probabilistic relations are to be interpreted causally.
- (3) The informality with which most of us use causal language leads to much confusion, and this confusion could be avoided if we made more use of formal methods to analyse causality. Indeed, one purpose of this book is to convince us that the relevant formal tools – principally graph theory – are already available and well developed.

For what it is worth, I completely agree with the first two points and the first part of the third. As to whether graphical analysis, or formal methods generally, has as much to contribute to the analysis of causation as Pearl believes, I am not yet convinced, particularly with regard to economics.

Pearl's writing is lively, but not always as precise as one might like. An example will illustrate this. In 'Causal Orderings' (1995) I proposed a way to determine the causal orderings among endogenous variables that can be deduced from assumptions about exogeneity: variable X is defined to cause variable Y if X is a sufficient statistic for all the exogenous variables that affect both X and Y . This requires that none of the exogenous variables that cause X has a separate influence on Y . Pearl defined a structural equation – one that has a causal interpretation – as follows:

An equation $y = \beta X + \varepsilon$ is said to be *structural* if ... [when] we control X to x and any other set Z of variables (not containing X or Y) to z , the value y of Y is given by $\beta X + \varepsilon$, where ε is not a function of the settings x and z (p. 160).

This would seem to be the same thing as what I proposed since if some exogenous variable affecting X has a separate effect on Y , one would have that ε is a function of the setting x of X because of the common dependence of x and ε on that variable. But Pearl rejects this:

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 labour to estimate (p. 136).

He does not explain why my definition of causal ordering is different from his. Similar lack of clarity occurs often, and it can make a close reading of this book an exercise in frustration, at least in some places.

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The Edinburgh Building
Shaftesbury Road
Cambridge CB2 2RU, UK
www.cambridge.org

Telephone +44 (0)1223 312393
Fax +44 (0)1223 315052

Pearl is a big admirer of the Cowles economists who pioneered the analysis of structural equations and causation in the 1940s and 1950s. Haavelmo (1943) in particular proposed essentially the above definition of structure, so that an equation is structural if the right-hand side variable(s) can be

interpreted as causing the left-hand side variable. Pearl observed that following the Cowles period economists and other social scientists have lost sight of this simple but serviceable analysis, instead avoiding systematic discussion of causality. Quoting Leamer (1985):

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 (p. 258).

Whether or not Leamer is justified in saying that economists know very well what they mean when using causal language, he is certainly correct that these issues are not handled clearly in the rare instances when they are discussed explicitly. The problem is that the theory of economic equilibrium as currently formulated does not produce structural models, so that the graphical methods Pearl advocates do not apply (or, as noted below, apply trivially). This is worth some discussion.

The Cowles economists in the 1950s, like social scientists today in areas other than economics, formulated models directly, as opposed to deriving them from more primitive assumptions. It appeared natural to them to endow individual equations – the consumption function, the money demand equation – with a structural interpretation in the sense of Haavelmo. The macroeconometric models of the 1960s continued this interpretation, viewing the 'structural form' of a macroeconometric model as containing information, presumably of a causal nature, over and above that contained in the 'reduced form', where the latter expresses equilibrium values of the endogenous variables as functions of assumed values of exogenous variables.

Now, however, economic models are derived from assumptions on preferences, technology and endowments rather than directly formulated. Assuming optimizing behaviour, equilibrium is defined as a map from the spaces of exogenous variables to the equilibrium values of endogenous variables. Debreu (1959) is the prototype in general equilibrium theory, or see Lucas (1972) in the macroeconomic literature. No distinction is made between structural form and reduced form. The equations expressing the solution of a model admit a causal interpretation, but in equilibrium models it is usually the case that all the exogenous variables enter as determinants of each endogenous variable, implying that the graph of such a model is trivial and useless.

As a result of this change, economists no longer think in terms of structural models as that term was used by the Cowles economists and is used by Pearl. Correspondingly, they find little application for the graphical methods that Pearl advocates (there are a few attempts in the economics literature to analyse causal relations along the lines Pearl advocates; see Sheffrin and Triest (1998), for example). However, this does not mean that economists can continue to ignore the questions that Pearl is asking: we routinely use causal language to describe relations between endogenous variables without

explication or justification. It is true that we occasionally engage in methodological discussion related to causality, as with the 'Lucas critique' (Lucas (1976)), but such analyses are rarely satisfactory (see LeRoy (1995) for a discussion of the Lucas critique), as Pearl would no doubt agree. This is hardly a satisfactory situation, but it will not change until economists get over their notorious impatience with methodological and philosophical discourse.

The book under review is essential reading for economists: we cannot go on avoiding serious analysis of causation or continue pretending that causality can be identified with particular types of correlation. We have to start somewhere in thinking about causality, and this book is a fine candidate.

Stephen F. LeRoy

University of California, Santa Barbara

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