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Book Reviews

***Causality: Models, Reasoning, and Inference*. By Judea Pearl. Cambridge, Cambridge University Press, 2000, xvi, 384 pages, £30.00 (hardcover), ISBN: 0-521-77362-8.**

> The data is all over the place, the insight is yours, and now an abacus is at your disposal, too. I hope the combination amplifies each of these components. (p. 358)

After it has been silenced for a century by philosophers (various sources blame Russell for this), causality is back in the center of philosophy given the recent publications: D. Hausman's *Causal Asymmetries* (Cambridge University Press, 1998), J. Woodward's *Making Things Happen: A Theory of Causal Explanation* (Oxford University Press, 2003), and N. Cartwright's working paper (better call it a 'working book' because it contains four chapters that are in fact forthcoming papers) *Measuring Causes: Invariance, Modularity and the Causal Markov Condition* (LSE 2000). Since Hume's ban on framing economics in terms of causality, causality has now also been reintroduced to economics by K. Hoover's *Causality in Macroeconomics* (Cambridge University Press, 2001). However, one could bring up that it has never really been absent in econometrics: don't we have something like 'Granger causality'? And don't we have people like Arnold Zellner (at the 2003 AEA meeting someone remarked that if Granger and Zellner would cooperate they could definitely solve the problem of causality in economics) who edited with Dennis Aigner a special issue on Causality in the *Journal of Econometrics* (1988) with contributions by causality experts (besides Zellner and Granger) such as C. Glymour, J. W. Pratt, R. Schlaifer, H. A. Simon, B. Skyrms, and P. Spirtes? Yes we have, but the econometricians' concepts of causality are phrased in a 'pitiful' mathematical language. Probabilistic language is not sufficient; it should be supplemented with a language of causal graphs. Combined, they present us with a 'friendly' mathematical language, ready for computer analysis, a "sort of pocket calculator, an *abacus*, to help us investigate certain problems of cause and effect with mathematical precision" (p. 357). This is the central message of Judea Pearl's book on 'Causality'. This book not only provides an account of why this supplement is required but also how it can be used to improve causal analysis.

Probability theory is rightly the official mathematical language of most disciplines that use causal modeling, including economics and econometrics. Causal expressions are often used in situations that are plagued with uncertainty and subject to exceptions. Probability theory is especially equipped to accommodate uncertainty and to tolerate unexplicated exceptions. Investigators are concerned not merely with the presence or absence of causal connections but also with the relative strengths of those connections and with ways of inferring those connections from noisy observations. Probability theory, aided by methods of statistical analysis, provides both the principles and the means of coping with, and drawing inferences from, such observations. However, as Pearl emphasizes, the word 'cause' is not in the vocabulary of probability theory; we cannot express in the language of

probabilities the sentence, "Mud does not cause rain". All we can say is that mud and rain are mutually correlated or dependent, meaning that if we find one, we can expect 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".

Taking Hume's dictum that all knowledge comes from experience encoded in the mind as correlation, and the observation that correlation does not imply causation, we are led to problems such as: how do people ever acquire knowledge of causation? What patterns of experience would justify calling a connection 'causal'? Moreover: what patterns of experience convince people that a connection is 'causal'? According to Pearl, the possibility of learning causal relationships from raw data can only enter the realm of formal treatment and feasible computation by the aid of graphs. This mathematical language is not simply a heuristic mnemonic device for displaying algebraic relationships. 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 provide a powerful symbolic machinery for deriving the consequences of causal assumptions when such assumptions are combined with statistical data.

Causal effects permit us to predict how systems would respond to hypothetical interventions, for example, policy decisions or actions performed in everyday activity. Such predictions are the hallmark of causal modeling, since they are not discernible from probabilistic information alone; they rest on, and in fact define, causal relationships: Causation is a summary of behavior under interventions. Causal graphs are used to give formal semantics to these interventions, a mathematical language within which causal thoughts can be represented and manipulated. As a result, interventions can be treated as a surgery over equations guided by a graph. And causation means predicting the consequences of such a surgery. But one ingredient is still missing: the 'algebra' to express causality, a 'calculus of interventions': a set of inference rules by which sentences involving interventions and observations can be transformed into other such sentences, thus providing a syntactic method of deriving (or verifying) claims about interventions and the way they interact with observations. Pearl is able to design such a calculus of interventions by introducing the notion of intervention by a new operator "given that I do x" assigned by the symbol

$do(x)$, which stands for setting the value of variable X to a fixed constant, x. Next, he deduces rules for manipulating sentences containing this new symbol. In other words, besides the already available 'algebra for seeing', namely, probability theory, we now have an 'algebra of doing', a 'do calculus', too. Pearl not only provides this algebra but also gives several examples of successful applications.

However, this language is not new. In fact, two languages for causality have been proposed earlier: structural equation modeling and the Neyman–Rubin potential-outcome model. The former has been adopted by economists, while a group of statisticians champion the latter. Pearl shows that these two languages are mathematically equivalent, yet neither has become standard in causal modeling: "the structural equation framework because it has been greatly misused and inadequately formalized ... 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" (p. 134). According to Pearl, one cannot overemphasize the importance of the conceptual clarity that structural equations offer vis-à-vis the potential-outcome model.

Structural equation modeling (SEM) was originally developed by economists so that qualitative cause-effect information could be combined with statistical data to provide quantitative assessment of cause-effect relationships among variables of interests. The conditions that make the equation $y = \beta x + \varepsilon$ '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. This basic understanding of SEM has all but disappeared from the literature, leaving modern econometricians and social scientists in a quandary over β.

Today, there is a general tendency among economists and econometricians to view a structural equation as an algebraic object that carries functional and statistical assumptions but is void of causal content. The current dominating philosophy treats SEM as just a convenient way to encode density functions. "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 it meaning." (p. 135)

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, according to Pearl, exonerate economists from defining and representing the causal content of structural equations at some level of discourse. Pearl believes 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. (p. 138)

The 'founders' of SEM understood quite well that, in structural models, the equality sign conveys the asymmetrical relation 'is determined by'. However, the later Cowles Commission saw no particular merit in causal diagrams. 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. Pearl's explanation for this is that "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." (p.138)

The use of causal graphs, in the 1940s called 'arrow schemes', to describe causal structures was first proposed by Jan Tinbergen (1940). Herman Wold (1949) used Tinbergen's arrow scheme to suggest recursive systems as an alternative way of representing causal structures to the simultaneous equation models of the Cowles Commission. Led by Tinbergen's approach Wold emphasized the notion of process and tried to link time-series structures with causal relations (see Hendry and Morgan 1995). In 1954, the main theme of the meeting of the Econometric Society was 'The possibilities and limitations of econometric models; recursive vs. structural systems', a

debate between the Cowles Commission, as the proponent of simultaneous equation models, and Wold as the proponent of recursive chain models (for a comprehensive discussion of this debate, see Morgan 1991). In an interview Hendry and Morgan (1994: 425) asked Wold whether there was a 'fundamental difference' between him and the Cowles econometricians. Wold answered:

> Well, one of the problems was even discussing the idea of causal chain models. You see, the influence of Bertrand Russell had created a climate of opinion in the thirties and into the fifties in which causal terminology was difficult to use. Tjalling Koopmans, for example, was influenced in this way. I did not accept Russell's arguments about causality, but felt that it was useful to talk about causes and causal chains.

Although the Cowles Commission group mainly ignored the issue of causality and its representation and interpretation in economic models, it was taken seriously and discussed in greater depth by Simon (1953). While Simon and Wold disagreed on certain notions about causal systems—asymmetries and relationships versus time sequences and variables—they came closer to each other with respect to the need for causal system and their purpose for intervention analysis and policy decisions (Morgan 1991: 248–249).

One of Wold's concerns was that Simon's causal ordering was based on definitions imposed on the structure without regard to underlying economic behavioral relationships. Simon had reduced the problem of causality to a mathematical problem of clever transformations of the matrices in this framework. As such it was logically connected with the concept of identifiability. Identifiability was obtained by specifying *a priori* that certain coefficients in the model must be zero and any such specification in a complete structure defines the causal ordering. Because Simon showed that a linear system of equations was identified if and only if it was causally ordered, causality also disappeared from the scene.

> Identification seemed to be the more pressing problem to econometricians focused on the problems of estimation. Equivalence meant, in some respects, causality could be ignored without loss. And identification itself had noncausal roots in the problem of the measurement of demand. Causal language simply faded away. (Hoover 2001: 147)

And with it, the use of causal graphs disappeared too. Pearl is 'thoroughly convinced' that the contemporary crisis in SEM originates in the lack of a mathematical language for handling the causal information embedded in structural equations. "Graphical models have provided such a language.

They have thus helped us answer many of the unsettled questions that drive the current crisis (p. 170)''. His book is an attempt to bring back to, among other disciplines, economics and econometrics, the benefits of causal graphs.

Pearl's *Causality* is a highly recommendable book because it does what it promises: it clarifies the concept of causality by providing a mathematical language to express it simple and clear. Even so, I recommend you read at least (or first) the Epilogue to this book. It contains a public lecture full of very nice slides summarizing the main arguments of this book explained to a lay audience and therefore could be used in any introductory course.

Marcel Boumans
University of Amsterdam

REFERENCES

Hendry, D. F. and Morgan, M. S. (1994) "The ET Interview: Professor H.O.A. Wold: 1908–1992," *Econometric Theory* 10: 419–433.

Hendry, D. F. and Morgan, M. S. (1995) *The Foundations of Econometric Analysis*, Cambridge: Cambridge University Press.

Hoover, K. D. (2001) *Causality in Macroeconomics*, Cambridge: Cambridge University Press.

Lucas, R. E. (1976) "Econometric Policy Evaluation: A Critique," in K. Brunner and A. Meltzer (eds) *The Phillips Curve and Labor Markets, Vol. 1 of Carnegie-Rochester Conferences on Public Policy*, Amsterdam: North-Holland: 19–46.

Morgan, M. S. (1991) "The Stamping Out of Process Analysis in Econometrics," in N. de Marchi and M. Blaug (eds) *Appraising Economic Theory: Studies in the Methodology of Research Programs*, Aldershot: Edward Elgar: 237–265.

Simon, H. A. (1953) "Causal ordering and identifiability," in W. C. Hood and J. C. Koopmans (eds) *Studies in Econometric Method*, New York: Wiley: 49–74.

Tinbergen, J. (1940) "Econometric Business Cycle Research," *Review of Economic Studies* 7: 73–80.

Wold, H. O. A. (1949) "Statistical Estimation of Economic Relationships," *Econometrica* 17 supplement: 1–22.