

Cartwright on “Economics”

Lawrence Boland

Simon Fraser University, Burnaby, B.C.

Cartwright, Nancy. (2007). *Hunting Causes and Using Them: Approaches in Philosophy and Economics*. New York: Cambridge University Press. ISBN: 978-0-521-67798

Nancy Cartwright claims that “Causality is a hot topic today both in philosophy and economics”. She may be right about philosophers, but not when it comes to economists. Cartwright talks about “economics” but nothing she says about it corresponds to what is taught in economics classes. Today, economics is dominated by model builders – but not all models involve econometrics. While all model builders do respect an endogenous-exogenous distinction between variables, this distinction will not be on the basis of which type of variable “causes” which, but on which variables are “determined” (and hence explained) by the model and those which are not.

Keywords: *causality, economics, econometrics, model building*

Professor Cartwright’s recent book about causality is a collection of ten previously published articles, three new chapters and three short introductions to the three parts of the book. She says that the “collection is for philosophers, economists and social scientists or for anyone who wants to understand what causality is, how to find out about it and what it is good for” (p. 1). The three parts are roughly allocated to these three separate issues.

In Part I she provides “a defense of causal pluralism” and she claims that “Philosophers and economists alike debate what causation is and, correlatively, how to find out about it” (p. 2). To support her claim with respect to “economists” she refers to a recent [*scil.* 2003] volume of the *Journal of Econometrics* where the discussion concerns a reported test of “the hypothesis that socio-economic status causes health by a combination of the two methods [she] discuss[es] in part II: Granger causality, which is the economists’ version of the probabilistic theory of causality that gives rise to Bayes-nets methods, and an invariance test” (ibid). Most of that volume is devoted to this test of causality and consists of commentaries by econometricians who offer differing characterizations of causality.

Cartwright’s main point to make is that how one tests a causal claim depends on how one characterizes causality and similarly on how one intends to use that causal claim. So, she found the debate in the *Journal of Econometrics* volume to be misdirected because the issue is not “what is the ‘right’ characterization” but rather “what kind of causal system is generating the panel data discussed” (i.e., the data discussed in the lead article of the volume). And as she says, “Causation ... is a highly varied thing” and “What causes should be expected to do and how they do it ... can vary from one kind of system of causal relations to another and from case to case” (ibid).

In Part II she claims to illustrate that “there is no single interesting characterizing feature of causation; hence no off-the-shelf or one-size-fits-all method for finding out about it, no ‘gold standard’ for judging causal relations ... with two different (though related) kinds of

causal system, matching two different philosophical accounts of what causation is, two different methodologies for testing causal claims and two different sets of conclusions that can be drawn once causal claims are accepted” (pp. 2–3). In particular, the second Part critically examines so-called Bayes-nets methods that have their origin in the work of Patrick Suppes and the simultaneous-linear-equations method that is commonly used in econometric models of macroeconomics.

Part III is devoted to how causality is treated in “economics” and begins in the first four chapters by focusing “on hunting causes in economics” while the last chapter focuses “on using them” (p. 175). Herbert Simon’s early econometric work on causal structures gets a specific treatment directly and indirectly by discussing Kevin Hoover’s view of Simon. This Part’s extensive discussion of “economics” is the result of Cartwright’s recognition that many economic policies are based on using “causes to bring about effects” (p. 3).

Causality among economists

Professor Cartwright claims that “Causality is a hot topic today both in philosophy and economics” (p. 43). She may be right about philosophers, but certainly not when it comes to economists. Here, for example, is what one of the commentators says in the *Journal of Econometrics* volume she cites:

I argue that causality per se is not an interesting concept. What is interesting is the question of whether an empirically determined relationship can be used to provide valid forecasts of policy on an outcome. (Heckman 2003, p73).

Throughout, Cartwright talks about “economics” but nothing she says about it corresponds to what is taught in economics classes. Instead, what she talks about is limited to econometrics and econometric theory.

Economics as an academic discipline existed long before econometrics. Even today, while economics departments will have a few courses specifically about econometric analysis and theory, there are many more courses where econometrics plays no role what so ever. Econometrics as a method of data analysis was not developed until the 1930s. It was first developed as a means of simply marrying statistical data with exact algebraic economic models. But classical statistics requires certain things about the observable variables (such as independence) that are necessarily violated when building empirical models of an economy. Econometric theory was specifically developed to overcome the classical requirements and is thus different from simply using statistics.

The only time in the last five or six decades when causality was a burning issue in economics was in the 1970s and 1980s. The issue then was whether inflation was caused by there being too much money in the economy. And as such, this was purely an ideological debate between followers of Milton Friedman and followers of John Maynard Keynes. I suspect that among economists, causality matters only when ideology matters. Economists interested in theory more than ideology will be more satisfied with their alternative: the commonly used distinction between endogenous and exogenous variables. Specifically, as understood by modern economists, endogenous variables are the ones “determined” by a conjectured model but the exogenous ones are not. While there was a time in the 1950s when this distinction had its roots in the ordinary notion of causality, today it does not. So, let me explain why economists see no need to be concerned about causality.

Causality and economic model builders

Today, economics is dominated by model builders – but not all models involve econometrics. Nevertheless, all model builders respect the endogenous-exogenous distinction – not on the basis of which type of variable “causes” which, but on which variables are “determined” (and hence explained) by the model and which are not (however, occasionally the word “cause” will be used but only as a synonym for “determine” and meaning nothing more). There are also two other important distinctions playing a role: One is between elements of a model that are observable and those that are not; and between elements that are fixed over time and those that are not. These distinctions are of fundamental interest to models built for econometrics and theoretical models must be respectful of them, too.

Every model posits relationships between the endogenous and exogenous variables. And most important, these are the only observable variables. The simplest relationship is a linear one; for example: $C = \alpha + \beta \cdot Y$. The C might represent the level of a year’s aggregate consumption expenditure and the Y might represent the level of that year’s aggregate national income. For a simple model, more is needed as usually the economist would be trying to explain both consumption and income. So, another relationship (in this simple case, an identity that defines Y specifically) would be added such as: $Y = C + Z$ where Z might represent the level of that year’s aggregate investment (perhaps including government investments in such things as infrastructure). In this very simple model (consisting of only these two relationships), the C , Y and Z represent observable variables. As a matter of simple algebra, one could “solve” for posited endogenous variables, consumption (C) and income (Y), if one knew the values of the parameters α and β as well as the one unexplained (hence exogenous) variable Z . A more complicated model might explain Z with a third relationship (i.e., a third equation) and render it another endogenous variable but this would require another exogenous variable. This is because without at least one exogenous variable, a model would be indeterminate or at worse circular.

Note that in this simple model, nothing was said about which variables or parameters are fixed in time. And, it would not be necessary if one were only interested in the one set of observed variables (C , Y and Z) for one point in time (viz., at the year’s end). But usually, such a model would be used to explain the endogenous variables at many points in time (i.e., for many years). For this purpose, it is assumed that the parameters α and β are fixed, each at its single value that spans the period of time being considered. When Keynes was explaining the level of aggregate consumption, he explicitly posited that the β represented a psychologically given “marginal propensity to consume”. Specifically, β says that for every dollar of income, each individual spends a fixed percentage – i.e., a fixed percentage of each dollar of income received during the whole span of time encompassed by the years considered. But this percentage is not explained; it is just assumed to be a fixed parameter. That is, it is a parameter, not an exogenous variable. (Actually, the α and β are mathematical artifacts of the linearity assumption; Keynes was just suggesting an obvious interpretation of the β .)

Again, economists who would build such a theoretical model would just assume the fixed values for α and β . But, econometricians who would build such a model will make no such assumptions. Instead, the objective of building an econometric model is to determine – by

means of a type of reverse engineering – the fixed values of α and β using the observed values of the endogenous and exogenous variables over the time-span of interest. Since observations are rarely accurate, econometricians add “error terms” to each of the equations representing the assumed relationships but usually assume that over the time-span in question the mean value of the errors is zero. Many econometricians view their models – once the value of the econometrically determined values of the parameters are substituted for α and β – as true “generators” of the observed endogenous variables (i.e., if you were to plug the values of the observed exogenous variables as well as the determined values of the parameters into the model at each point in time, the values of the endogenous variables would be ones observed allowing only for the error term). If the model is properly specified, only one set of values for the parameters will be found by means of the reverse engineering (i.e., a properly specified model is said to be “identified” – but I will not bother with this issue here other than to say that a properly specified model must have the same number of exogenous variables as there are endogenous variables).

Now note well: in this brief exposition about economic models and econometrics there is no mention of “causes” or “causality” and as far as most economists are concerned, there is no need for it! Beside the avoidance of ideological stances, there are at least two other reasons for this. The main one is that most models involve many exogenous variables and many parameters hence realistically no one of them could be deemed “a cause”. The other is that economists today are more interested in the mathematical properties of their models and not anything they might deem to be metaphysics. That is, mathematical relations such as $Y = f(X)$ says nothing about whether X causes Y. Even if one wishes to say X causes Y, the math by itself can never tell us that. It is just a way of interpreting the math. Herbert Simon (1953) tried to specify a special case where the mathematical structure of the model could be used to force a particular interpretation (which Cartwright discusses in Chapter 13). But, only in a very limited sense is his structure approach about the idea of causality that she thinks economists are hunting. As Kevin Hoover explains,

Simon (1953) offers a useful account of causal order in a system of simultaneous linear equations. One variable in such a system is said to cause another if one must know the value of the first variable in order to solve for the value of the second variable. Simon’s notion of causal order is one of a hierarchy of nested subsystems in which the causes are located in the more central subsystems and the effects in the more peripheral subsystems. (2001, p. 37)

Having to know the value of one variable in order to determine the value of another variable is merely an artifact of the mathematical form Simon invents and nothing more. And, I am not sure that Cartwright realizes that one can discuss all this without reference to random variables and hence without reference to probabilities – even though this is exactly what Simon said (1953, p. 52).

There is, however, one possible causality interpretation when it comes to a model’s exogenous variables – as Hoover explains (2001, Chapter 9). For example, if an exogenous variable represents the level of government investment, one could speak of an investment multiplier. This, of course, implies a level of control over that exogenous variable. Cartwright discusses this interpretation in Chapter 12 and does so by discussing the work of economists Thomas Cooley and Stephen LeRoy (1985). What they were discussing is something like what economists call a multiplier – which, as they would say, represents a

type of hypothetical, intellectual experiment. That is, consider a model which explains national income (among other things) and has, as an exogenous variable, the level of government investments. Such a model can be used to calculate how much national income would change if, *ceteris paribus*, the level of investment were to change. To do this one solves for the level of national income as a function of all the exogenous variables and with an equation representing this solution, one performs the intellectual experiment and thereby determines the potential effect of a change in the level of government investment. Note, however, it is all too easy to mathematically manipulate that equation and see it instead as a solution for the level of investment as a function of all the other variables. I note this only to underline that there is nothing in the mathematics about causes to prevent such a reversal even though the intention of the model is to be able to say that a change in investment could be seen to “cause” a change in income. Of course, if the model is a true representation of the real economy in question and if one could control all of the exogenous variables (and not just a policy variable such as the level of government investment) so that *ceteris paribus* can be assured, then one might be able to make a causal claim. But most economists today would find this an unrealistic stretch although it usually is accepted for heuristic purposes.

Can economists so easily avoid causality?

Contrary to how most economists avoid using any concept of causality in their model building activities (again, they are only concerned with whether their model can be used to mathematically determine the values of endogenous variables), I think every model can be seen to be positing a causal mapping from the vector space of values for the exogenous variables to a vector space of values for endogenous variables. The posited relationship between the parameters and all those variables is what constitutes the mapping (as I explained in my 1989 book, Chapter 6, where I was mostly concerned with the falsifiability of such a mapping). What I proffered is that every model posits a finite list of exogenous variables for the purpose of explaining the values for a given list of (possibly observed) endogenous variables. Implicitly any model claims that the list contains the exogenous variables that are the only ones (through the posited mapping) to be determining the values of the endogenous variables. The theoretical claim is that no other potential exogenous variables matter. If we think of a point in the vector space of exogenous variables as a finite vector of their values, then we could say that vector and the mapping together are the cause of the (observed) vector of the endogenous variables. And again, this interpretation has nothing to do with probabilities. If the values of the endogenous are to be explained, then the mapping and list of variables will constitute a conjecture both about the mapping’s accuracy and the claim about exogenous variables that matter.

I mention all this because Cartwright does not – even though she claims to be discussing how economists “hunt causes”. Her claim is partly due to her seeing that the econometrician’s reverse engineering is a form of hunting but, again, she does not seem to realize that not all economists are econometricians. Moreover, she seems unaware that some economists even reject seeing probabilities as playing an essential role in economic explanations (e.g., Davidson 1991, Rubinstein 1991; see Boland 2003, Chapter 8). But, it could be said that even these economists are hunting causes since every explanation of

observed events (e.g., GDP, prices, employment, etc.) implies a conjectured cause-effect relationship. So, to that extent her claim may still be correct.

Hicks on causality in economics

When I began reading Professor Cartwright's book, my first expectation was that, of course, she would have something to say about the Nobel Laureate Sir John Hicks' well known 1979 book with the inviting title: *Causality in Economics*. Alas, nothing. This is particularly surprising since so much of her discussion is about macroeconomic models (albeit, econometric models) as well as her physics oriented perspective on the philosophy of science. As I said in my 1980 review of this book:

The primary methodological thrust of this book [by Hicks] is that the methodology and causal precepts of physics are inappropriate for economics. Physics methodology presumes the existence of natural constants which are to be discovered or proven. There are no natural constants in economics. Experimental sciences presume timeless (i.e., universal) facts upon which one can argue by 'induction'.... There are no timeless facts in economics. All data collected in economics is historical – that is, *in* time. The use of physics methodology in economics must presume the existence of stable constants; hence the applicability of such methodology is limited to very short periods of time over which the 'constants' can be considered constant.

... Since the time of David Hume, sequential causality is usually associated with physics – that is, with mechanics. If an object is in a state of (stable) equilibrium it will remain in equilibrium unless caused by an outside force to change its position to another equilibrium. In physics, there is no effect (change in position) without a prior cause (an outside force). The problem is that Hume's sequential causality must be instantaneous or constrained to a mechanical trajectory which is fixed by stable constants or coefficients. On the other hand, in economics there may be a considerable time lag between cause and effect. In economics, Hicks argues, whenever one explains the effect as a result of a *prior* cause, one must also explain why it takes so long – that is, what *causes* the delay – without the benefit of a fixed trajectory. (Boland 1980, p. 741)

By the time I finished reading Cartwright's collection of her essays, I was disappointed – particularly given her promise that it was “for philosophers, economists and social scientists”. Perhaps some analytical philosophers will find something of interest but little of it addresses how economists (as opposed to some econometricians) view causality. I think even philosophers will be disappointed given how poorly the collection is put together. As noted earlier, there are only three new chapters. But worse, there is little evidence that the previously published essays were edited to avoid repetitions of material so as to make a coherent book. All this is unfortunate because there was the potential for a very good book that would be “for philosophers, economists and social scientists” had Cartwright actually tried to understand economics rather than just econometrics. To do this, though, she would have had to read – in addition to the work of Kevin Hoover – what economic methodologists who have been trained in economics are saying about economics rather than relying on what a few analytical philosophers not trained in economics might be saying. I think even philosophers interested in economics would find that more useful.

References

- Boland, L. (1980) Review of *Causality in Economics*, *Canadian Journal of Economics*, 13, 740–3
Boland, L. (1989) *The Methodology of Economic Model Building: Methodology after Samuelson* (London: Routledge)
Boland, L. (2003) *The Foundations of Economic Method: A Popperian Perspective* (London: Routledge)

- Cooley, T. and LeRoy, S. (1985) Atheoretical macroeconomics: a critique, *Journal of Monetary Economics*, 16, 283–308
- Heckman, J. (2003) Conditioning, causality and policy analysis, *Journal of Econometrics*, 112, 73–8
- Davidson, P. (1991) Is probability theory relevant for uncertainty?: a post Keynesian perspective, *Journal of Economic Perspectives*, 5, 129–43
- Hicks, J. (1979) *Causality in Economics* (Oxford: Basil Blackwell)
- Hoover, K. *Causality in Macroeconomics* (Cambridge: Cambridge University Press)
- Rubinstein, A. (1991) Comments on the interpretation of game theory, *Econometrica*, 59, 909–24
- Simon, H. (1953) Causal ordering and identifiability, in Hood, W. and Koopmans, T. (eds.), *Studies in Econometric Method*, (New York: Wiley), 49–74

Lawrence Boland teaches economics at Simon Fraser University in Burnaby, British Columbia. He is the author of six books on economic methodology and a Fellow of the Royal Society of Canada,