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Causality and Temporal Order in Macroeconomics or Why Even Economists Don't Know How to Get Causes from Probabilities*

KEVIN D. HOOVER

I INTRODUCTION

Economists talk intermittently about causality; but they are likely to keep their discussions firmly grounded in the problem at hand. I have long thought that this led to confusion. One needs to have the concept of causality worked out in advance of particular applications. One should strive for a level of generality, so that analytical concepts are not purpose-built (and therefore likely jerry-built). My immodest goal in importing philosophical considerations into the methodology of economics has been to reform the practices of economists, and not simply to report and codify them.

With such prejudices in mind, it is surprising to discover the extraordinary degree of deference that philosophers pay to physics: particularly, in thinking that the adequacy of their causal analyses to quantum mechanics is the acid test of its success. It is not usual to pay economics similar deference. Even so, in this article I shall examine the consequences of paying deference to macroeconomics as it is actually practiced. My central theme is this: causal accounts that depend fundamentally on the temporal asymmetry of cause and

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effect face grave difficulties in macroeconomics; causal accounts based on counterfactuals are better suited to the job at hand.

2 CAUSAL CLAIMS AND MACROECONOMIC PRACTICE

Roughly, macroeconomics is the economics of the economy as a whole. Keynes ([1936], p. 293) distinguished between the theory of the determination of aggregate output and the theory of the individual economic unit (the firm or the consumer), which, being small relative to the economy as a whole, takes aggregate output as fixed. Macroeconomics in practice is about the interrelationship among economic aggregates and indexes—the relationships between *inter alia* GNP, the general price and wage levels, employment, unemployment, interest rates, exchange rates, the balance of payments, the budget deficit, and tax rates.

Economics is a social science; but macroeconomics, is remarkably dehumanized. People are at best referred to as representatives of a class; and, mostly, the talk is about variables. The variables employed in macroeconomics are defined pragmatically by the statisticians employed in government offices. Sometimes they take economists' advice, sometimes they do not. There is, therefore, often a mismatch between the variable as it is ideally conceptualized in macroeconomic theories and the data actually collected. For example: in theory, unemployment should be measured relative to the prevailing wage in jobs a potential worker is qualified to perform; but when governments collect unemployment statistics they generally ask only whether a potential worker is seeking work without reference to any particular wage or qualification.

Beyond such practical problems, theory itself is often not sharp. We all have a sense of what it means to experience inflation—*i.e.* generally rising prices. And economists sound precise when they state that the retail price index rose last month by 4·9 per cent. But what does this mean? Some prices rose a lot, some a little, some may even have fallen. What number gets reported for inflation depends on the weights placed on these various changes in prices. Within broad boundaries, quite arbitrary weighting schemes are theoretically acceptable, yet lead to tremendous differences in the actual numbers reported.[1]

Policy, historically and in current practice, motivates most of macroeconomics. Consequently, causal claims in macroeconomics are largely about the controllability of these imperfect macroeconomic variables. Even noninterventionists base their arguments on controllability: the economy is structured, they claim, in such a way that attempts to control key variables are either futile or perverse. Despite talk in the 1950s and 1960s of fine-tuning the economy, the control that interests policy-makers and practical economists is typically of a rather coarse kind. Generally, the issue is not whether policy can secure a

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[1] Perhaps fuzzy sets and the theory of fuzzy logic would be helpful here.

growth rate of GNP of 3·75 per cent in the first quarter of 1992 (although such issues do sometimes arise) but whether a reduction in tax rates can reverse a declining level of GNP or whether monetary authorities 'leaning against the wind' (*i.e.* supplying reserves when the economy is declining and absorbing reserves when it is booming) can smooth the business cycle somewhat. The causal relations of interest are then between variables rather loosely: 'does money cause output?' is more important to practical macroeconomics than 'does M1 at time t cause GNP at time t + 2?'

It is sometimes argued that control is not the central issue, that as disinterested scientists (economists love to think of themselves as scientists) they should be concerned with explanation. The issues of control and explanation actually operate on different levels. I take it that an adequate causal explanation provides the basis for informed attempts to control particular variables.

Such explanation is generic. Frequently, however, questions are asked requiring singular explanations: did the Iraqi invasion of Kuwait cause the US recession of 1990/91? As a matter of division of labor, such questions belong to the historian, not to the policy advisor. Although macroeconomists sometimes play both parts, they more often live for the present and future and generally seek generic explanations. For the typical macroeconomist, then, causality is most usefully described as processes which are the relations of variables rather than as the relations of events. In arguing for causal processes, Salmon ([1984], pp. 140, 141) implies that it may be possible to translate freely between analysis based on processes and analysis based on events.

3 TEMPORAL CAUSALITY AND THE S–R BASIS

Causal claims in macroeconomics are usually implicit and casual. When they are explicit, they are most frequently justified by an appeal to so-called 'Granger causality' [Granger, 1969; Sims, 1972]. In practicable tests, a variable X Granger-causes a variable Y if the error variance of a regression of Y on its own past history and on the past history of X is statistically significantly lower than the error variance of a regression of Y on its own past history alone.

There is a great historical divide in economics between analyses based on process and analyses based on equilibrium. Before the 1940s, macroeconomics was largely the analysis of business cycles, and process analysis, or economics dynamics, held sway. After Keynes's *General Theory* [1936], equilibrium analysis became dominant in macroeconomic theory. The tradition of dynamic analysis was preserved, however, more in Sweden and the United Kingdom than in the United States, as a distinct subfield of econometrics (Morgan [1989, 1990]). Granger belongs to that tradition.

Dynamic analysis takes time seriously. If we add to that fact, the Humean observation that causes precede effects, we have the basis for Granger-causality. Once we recall, in a world of normal errors at least, that regression is equivalent to estimating conditional probabilities, we also see that Granger-causality employs a sort of screening criterion (Salmon [1984], pp. 43–45, *passim*): X causes Y if Y's past history does not screen off X's effect on Y. There is a large literature on extending Granger-causality from bivariate to multivariate orderings. Granger ([1980], p. 330) provides a more general definition of cause:

$$Y_n \text{ is said to cause } X_{n+1} \text{ if } \mathrm{Prob}(X_{n+1} \in A \mid \Omega_n) \neq \mathrm{Prob}(X_{n+1} \in A \mid \Omega_n - Y_n) \text{ for some } A,$$

where X_n and Y_n are time-ordered sets of variables defined for time $t = -\infty, \ldots, 0, 1, \ldots, n$; and Ω_n is the set of non-redundant information available at time n. The requirement that the left-hand side simply be unequal to rather than strictly less than the right-hand side suggests that negative relevance may also be taken into account.

A charitable reading of Granger's analysis could see it as attempting to provide an operational method of establishing Salmon's [1984] 'statistical-relevance (S–R) basis'. Nancy Cartwright ([1989], pp. 55–9) argues that Granger's analysis is inadequate as an S–R basis. The important point for the moment, however, is that it bears a family resemblance to other S–R accounts, and, for that matter, to other probabilistic accounts of causality. Salmon ([1984], pp. 36–47) argues that an S–R explanation is not in itself an adequate causal explanation. Something must be added to any account based on correlations. Salmon ([1984], pp. 141ff.), adds the idea of a markable process. Although Cartwright ([1989], Section 6.7) appears also to find use for similar ideas, she joins Granger, Suppes [1970] and others in putting extraordinary weight on temporal ordering (Cartwright [1989], Ch. 1).

4 THE NATURE OF MACROECONOMICS

I shall argue presently that the nature of macroeconomics itself undermines temporal ordering as a foundation for causal analysis. Before that, however, let me suggest some critical features of macroeconomics that are useful in the argument.

As I have already observed, macroeconomics deals in aggregates. These aggregates are composed of the behaviors of individuals. Consumption as reported in the national income accounts is just the summation of the purchases of a nation's citizens. It is tempting then to see economic agents as human molecules (cf. Nelson [1990]) and the relations postulated in macroeconomic theory or measured in macroeconometrics as the analogues of the ideal gas laws or other macrophysical relations. But there is a crucial and obvious difference: molecules do not make choices, people do; and they do so with reference not just to the immediate past and their immediate surroundings, but also with reference to future goals and to global or macro relations (*e.g.* people use the aggregate price level to calculate their real wages in striking wage bargains; firms use information on aggregate GNP in assessing the likely demand for their products). A shorthand term for this feature of macroeconomics is *agency*: unlike gases, which are composed of inert molecules, economic aggregates are constituted by agents.[2] Recognition of an agency problem is the foundation for the research strategy known as the 'representative-agent model'. An economy is described as if it were populated by a single agent, a Robinson Crusoe whose budget constraint is the entire GNP of the economy.[3]

Robinson-Crusoe models gloss over another feature of macroeconomics: the *nonhomogeneity* of economic aggregates. The people who constitute economic aggregates are not alike one with another, and do not remain constant in their tastes and circumstances over time. The same numerical value for consumption in the economy as a whole could represent very different patterns of consumption depending on the distribution of income, the demographics of the consumers, and whether it is 1991 or 1941 or 1721. Any stability in such aggregates clearly arises from averaging over behaviors that diverge in fine details. Income studies suggest that the allocation of income into broad categories—food, clothing, housing—may be relatively constant, but allocation into yoghurt, beer, books, and motorcars varies considerably over time and between age groups, regions, and social classes.

Greater stability in aggregate relations can no doubt be obtained by accounting for the distribution of income, changing tastes, and so forth. This is the empirical counterpart of the program of establishing microfoundations for macroeconomics. In any practical setting, however, one is still limited to (somewhat finer) aggregates: *e.g.* accounting for the distribution of income among quintiles of the population may help explain consumption; but the consumption of a fifth of the population is still a nonhomogeneous aggregate. Pushing the microfoundational program to its logical extreme would require us to account in detail for the economic behavior of each individual and build up macroeconomic aggregates from there. There is, however, an insuperable difficulty in pursuing such a program: the economy is too complex. This is what I have elsewhere called the 'Cournot problem', in recognition of its clear formulation by the mathematician and early mathematical economist Augustin Cournot ([1838/1927], p. 127; see Hoover [1984, 1988], Chapter

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[2] The term 'agency' should not be confused here with its use in the context of principal/agent models in microeconomics.

[3] Such single agent models are typically the only ones called 'representative-agent models', but there is no deep difference in principle between such models and others in which a few different types of agents coexist (*e.g.* overlapping-generations models in which, say, two types of agents—old and young—exist at any time, but in which there are an infinite number of agents over all future time as each young generation becomes old, dies, and is replaced by a younger generation).

9: also see Friedman [1955] and Hayek [1979], p. 75, fn. 8). Computers are not now or likely to be in the foreseeable future powerful enough to manage such a reduction of aggregates to their components. But this difficulty is not simply a practical one. More fundamentally, the preconditions for such a reduction do not exist. Neoclassical microeconomics, the economic theory of which the profession is so proud, generally assumes that tastes, knowledge, underlying resources, and other background conditions are either fixed, or, at the least, evolve in determinable ways. This is almost surely false. Far from having complete, transitive, and reflexive preferences, people—subject to binding constraints to be sure and not completely inconsistent—choose in whimsical, partially informed, and arbitrary ways. Equally, they choose with respect to relatively subtle changes in background conditions. The efficiency gains of a free market system arise precisely because of local adaptability which produces behaviors that, unless viewed from the inside as it were, appear to be random and erratic. The economy is characterized by *informational complexity*, and can be viewed as a giant computer for solving production and allocation problems (Hayek [1937, 1945).[4] One consequence of informational complexity is that the economy is invariably stochastic. Incorporating finer and finer information in the construction of an S–R basis will reduce the residual of unexplained random noise only up to a point. Indeed, a good deal of the stability of aggregates no doubt arises from the mutual canceling out of idiosyncratic behavior to reveal typical or average behavior. This does not suggest that a program of reduction in the direction of microeconomics will not succeed, only that there is probably some optimal level of reduction that none the less involves considerable aggregation.

Recognition of nonhomogeneity and informational complexity does not imply the independence of macroeconomics from microeconomics; indeed agency reminds us that the two are closely tied together. The relationship between them is one of supervenience. Macroeconomic aggregates are what they are and behave as they do because of the underlying behavior of individual people. One cannot, however, give a complete accounting of macroeconomics from the microeconomics alone. Macroeconomics supervenes on, but is not reducible to, microeconomics.

5 TEMPORAL ORDERING AND OBSERVATIONAL EQUIVALENCE

Although there are no barriers to extending the analysis to nonlinear, noncontinuous functions, much of the debate about causal structure in

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[4] The relationship between this description and the position of economists of the Austrian school in the 'socialist calculation' debate, as well as to their general '*verstehen*' approach, is obvious; see Hayek [1935a, b, 1940].

economics uses the framework of linear models (see Hoover [1990], Appendix.). A model such as

$$\text{Structure I}\quad \begin{cases} (1)\ a_{11}x_1+a_{12}x_2=e_1 \\ (2)\ a_{21}x_1+a_{22}x_2=e_2, \end{cases}$$

where the a_{ij}s (and, for the moment, the e_js) are parameters, and the x_is are variables, is *interdependent*. A model such as

$$\text{Structure II}\quad \begin{cases} (3)\ b_{31}x_1=e_3 \\ (4)\ b_{41}x_1+b_{42}x_2=e_4, \end{cases}$$

is *recursive*: equation (3) is used to solve for x_1, and the value of x_1 is used in equation (4) to solve for x_2. For Herbert Simon [1953], in Structure II, x_1 causes x_2, while in Structure I, causation is mutual. Simon also allowed in more complicated structures that the variables determined in interdependent blocks might be recursively ordered ahead of, and therefore cause, variables in other blocks.

Similar notions of causal structure were central to debates in econometrics in the 1940s to 1960s.[5] The Cowles Commission program in econometrics revolved around the problems of estimating interdependent structures such as I. Hermann Wold and his colleagues argued that systems such as Structure II, which they referred to as *causal chains*, were preferred for technical reasons and because they gave more accurate rendering of causal asymmetry: if x_1 causes x_2, x_2 does not cause x_1. In these debates, the e_js were taken to be random shock terms. Basmann [1965] demonstrated conclusively that any system represented as a causal chain could be equally well represented as an interdependent system. Based on the equivalence between regression and conditionalization, Basmann's point is simply that structure I can be taken to represent the joint probability distribution of x_1 and x_2, while structure II is a partition of that distribution into the product of a conditional and marginal distribution. Thus, a theorem from the theory of probability states

$$(5)\ \Pr(x_1,\ x_2)=\Pr(x_2|x_1)\Pr(x_1)=\Pr(x_1|x_2)\Pr(x_2).$$

The left-hand expression corresponds to structure I (interdependence); the center expression corresponds to structure II (x_1 causes x_2); and the right-handed expression reverses the direction of causation (x_2 causes x_1). Data on the x_is alone cannot distinguish one case from another.

Simon did not consider stochastic systems explicitly, but he was aware of the

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[5] Morgan [1991] gives a detailed and accessible account.

root problem (Simon [1953], pp. 24–6). Allow the e_is to be deterministic parameters. Then for the same values of the b_{ij}s, e_3 and e_4, the variables x_1 and x_2 will have the same solution in structure II as in structure I.[6] In general, since any linear combinations of (1) and (2) have the same solution, any causal orders are possible.

Simon's solution to such observational equivalence can be restated in slightly different terminology (for details see Hoover [1990], pp. 210–15). The x_is are variables, while the a_{ij}s, b_{ij}s and e_is are parameters, where a parameter is a variable subject to direct control in the sense that its value can be determined independently of the values of other parameters. Causal order is determined completely by the parameterization. Thus, if the b_{ij}s are parameters, the causal order is the one implicit in structure II: x_1 causes x_2. This order does not change if structure II is transformed into structure I, because, if the b_{ij}s are parameters, the a_{ij}s are complicated functions of the b_{ij}s and not parameters themselves. Any setting of, say, a_{12} requires particular choices of the b_{ij}s, which in turn restrict the range of at least some of the other a_{ij}s. Causal order in the world is then a question of knowing what are true parameters or of finding some way to infer true parameters.[7]

Cartwright's solution is quite different, relying on temporal order. She cites the econometrician Edmund Malinvaud to the efect that apparent cases of mutual causality between x_1 and x_2 would turn out with fine enough division of the time line to be cases of $x_{1,t}$ causing $x_{2,t+1}$ causing $x_{1,t+2}$ and so forth (Cartwright [1989], p. 127). She thus refines Wold's causal chains by insisting that, when a variable is ordered recursively ahead of another, it also occurs before it in time; *i.e.* contemporaneous causality is ruled out. If all the causal factors are fully specified then the e_is, here again interpreted as random errors, are uncorrelated with each other. In such a system, she shows, observational equivalence is detectable because one cannot take linear combinations of true causal equations and still have the errors in different equations uncorrelated with each other (*cf.* LeRoy [1991], pp. 12, 13). For example, if Structure II represents the true causal order (e_3 and e_4 uncorrelated), then one linear transformation converting it to structure I would imply $e_1 = (e_3 + e_4)$ and $e_2 = e_3$, so that e_1 and e_2 would be correlated.

6 OBJECTIONS TO TEMPORAL ORDERING

Cartwright's imposition of temporal order on causal chains is formally sufficient to eliminate the problem of observational equivalence. I now want to argue that the three features of macroeconomics already discussed—agency,

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[6] *E.g.*, structural II with $b_{31} = a_{11} - a_{12}a_{21}/a_{22}$, $e_3 = e_1 - a_{12}e_2/a_{22}$, $b_{41} = a_{21}$, $b_{42} = a_{22}$, $e_4 = e_2$, yields the same solution as I with apparently different causal orders.

[7] Cartwright ([1989], pp. 20–2]. in arguments attributed to Clark Glymour, misinterprets Simon's perfectly adequate solution to the problem of observational equivalence.

nonhomogeneity and informational complexity—undermine causal accounts like Cartwright's based on temporal ordering. Actual macroeconometric models cannot be usefully forced into the mold of temporally ordered causal chains. I shall indicate three problems.

(1) *Frequency of Observation*

Cartwright (1989, p. 17) imagines an economic structure to be a temporally ordered causal chain. This formulation is econometrically too restrictive.

Consider Structure III (Figure 1), which is the analogue to Structure II, but with many variables. Each variable is indexed by a time subscript. Each equation includes all of the variables on the right-hand side that are dated before the variable on the left-hand side. If $e' = [e_1, e_2, \ldots e_n]$ is the row vector of random error terms from Structure III, then $E(ee') = \Sigma$ is the variance/covariance matrix of the e_is. Cartwright's requirement that e_is be uncorrelated is the requirement that Σ be diagonal, since the off-diagonal elements of Σ are the covariances which must be zero for the e_is to be uncorrelated.[8] Her suggestion is that if all the appropriate causal factors are included on the right-hand sides of the equations in Structure III that Σ will be diagonal by construction.

$$x_{1,t}1 = \sum_{j=1}^{n} \sum_{k=-\infty}^{t-1} b_{1j,k}x_{j,k} + e_1$$

$$x_{2,t+1}1 = \sum_{j=1}^{n} \sum_{k=-\infty}^{t} b_{2j,k}x_{j,k} + e_2$$

$$x_{3,t+2}1 = \sum_{j=1}^{n} \sum_{k=-\infty}^{t+1} b_{3j,k}x_{j,k} + e_3$$

$$\cdot$$

$$\cdot$$

$$\cdot$$

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$$\cdot$$

$$x_{n,t+n-1}1 = \sum_{j=1}^{n} \sum_{k=-\infty}^{t+n-1} b_{nj,k}x_{j,k} + e_n$$

FIGURE 1. Structure III

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[8] Time subscripts are suppressed where no confusion will result.

Structure III is very nearly what macroeconometricians refer to as a vector autoregression: each current variable is regressed on its own past values and the past values of every other variable. In practice, the variance/covariance matrices from vector autoregressions are never diagonal. Why? Why are they not diagonal by construction as they are in Cartwright's account? There must be some specification error. Perhaps we have omitted a causally relevant factor or perhaps we have not allowed for long enough lags.[9] These are genuine problems, but there is another more troubling one. In Cartwright's account, variables dated the same should not have explanatory power for each other, once past-dated variables are taken into account. But, in fact, they do.

Macroeconomic data are reported most often annually or quarterly, not uncommonly monthly, rarely weekly, and extremely rarely daily or more frequently. Prices, interest rates, and stock data are sampled at some particular time of the year, quarter or month. Flow data (*e.g.* GNP which equals the production of new goods and services per unit time) add up all the units occurring during the year, quarter, or month. Thus, GNP for the first quarter is the production on each day of January, February, and March added together. Granger ([1969], pp. 377–8) suggests that apparent contemporaneous causality would vanish if data were sampled at fine enough intervals. But such finer and finer intervals would exacerbate certain conceptual difficulties in the foundations of economics. There are hours during the day when there is no production; does GNP fall to nought in those hours and grow astronomically when production resumes? Such wild fluctuations in GNP are economically meaningless. Few goods perish in the instant of their production (electricity and the services of prostitutes come to mind); but, if a good endures, it is not new production (and therefore not GNP) but part of the stock of capital. The standard answer to this is to say that GNP is really the flow of services from the stocks of (depreciating) goods. Some goods just depreciate, and yield their services up faster than others. There is in practice, however, no way to quantify and measure such flows that suffers from any fewer or less serious conceptual problems than temporal aggregation itself. Economists therefore are unlikely to, even in principle, force macroeconomics into the straitjacket of causal structures that rule out contemporeaneous causality.

(2) *Hidden Variables and Temporal Reversal*

Nonhomogeneity and informational complexity complicate any attempts to sample macroeconomic data too frequently. Agency presents its own complications. The Appendix presents details of a model of the following form: the price level rises to make the demand for money equal to the stock of money inherited from the last period; the demand for money itself depends not only

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[9] This is, in part, what is behind the reversal of the Granger-causal ordering of money and income as Sims and others moved from bivariate to multivariate tests of Granger-causality; see Cartwright ([1989], pp. 56, 57).

positively on the price level but also negatively on the rate of inflation (the percentage change in the price level), because inflation imposes a real cost on anyone who holds the money while it loses value; and the money supply is set as the sum of a genuine random process and the apparently random process from a deterministic pseudo-random-number generator. If the public is ignorant of the deterministic component of the money supply then money Granger-causes prices. However, if the public (but not the econometrician) knows the truth about the deterministic component, then prices Granger-cause money. This is because if they know that the deterministic component of the money stock will increase, they expect prices to rise in future; they, therefore, expect inflation and a loss on holding cash; they, therefore, reduce their current demand for money, which pushes current prices up in order to equilibrate supply and demand. Increases in current prices help to predict money in future, even though prices can actually rise only if money increases at some time; *i.e.* control of the money stock is sufficient to control price changes.[10]

It was crucial to this example that the public be better informed than the econometrician. Indeed, one way to think about the issue is arising from an omitted third cause. If expectations were directly observable, then an expectations variable would screen off the apparent causal effect of prices on money. The existence of such a variable is problematic. True, people form expectations and act upon them (that is the agency issue), but such expectations do not exist independently of the actions they affect; they are not palpable like so many pounds of rice bought by a consumer; they are hidden variables. Of course, one could ask people to sate their expectations. That, however, would be simply their guess about how they would act or would have acted in a situation that was not yet at hand or had passed already. Such expectations are no more directly observable to an individual than their own preferences, and are subject to the same whimsy arbitrariness and adjustment to subtle changes in background conditions. Furthermore, it is not an individual's expectations that matter, but those of the entire population. Thus, expectations are subject to all of the same problems of nonhomogeneity and informational complexity that plague all macroeconomic aggregates. Programs to reduce these problems and to better assess expectations are likely to be fruitful; pushing those programs to their logical extreme would eliminate macroeconomics altogether.

(3) *Long-run versus Short-run Causality*

Steady states are hypothetical economic configurations in which all short-run variations have stabilized so that prices and interest rates are constant, and stock and flow quantities are either constant or growing at constant

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[10] See Hoover and Sheffrin [1992] for a real-world example of such 'reverse Granger-causality.'

exponential rates. Economic theory provides reasonably persuasive accounts of steady-states, and the real world provides economic configurations for which steady states are good approximations. Economic theory rarely provides persuasive accounts of short-run transitional phenomena. Steady states are timeless; they are the result of allowing notional time to run on to infinity. In this they are not unlike conceptual devices in physics such as adiabatic expansion. Although timeless, variables in a steady-state may be recursively ordered—perhaps in a manner quite different from the short-run temporal ordering.

Consider the following model:

$$\text{Structure IV}\quad \begin{cases} (6)\ \Delta P_t = \Delta M_{t-1} + \beta(M_{t-1} - P_{t-1}) + \delta + e_t \\ (7)\ \Delta M_t = \phi\Delta P_{t-1} + \mu M_{t-1} + \tau + w_t, \end{cases}$$

where $\Delta X_t = X_t - X_{t-1}$ for any X, Greek letters are parameters, and e_t and w_t are uncorrelated random errors. Structure IV is an 'error-correction' model in which deviations from the steady state are corrected over time.

In the short run, P causes M and M causes P; but temporal order is respected. Figure 2 shows the short-run causal structure ('→' is read 'causes'). To find the steady state of Structure IV, set all changes to zero and all random shocks to their mean value, also zero. Steady-state values do not refer to particular times, and are thus indicated by variables without time subscripts. The steady-state solution is

$$(8)\ P = M + \delta/\beta$$
$$(9)\ M = \tau.$$

M is recursively ordered ahead of P. Despite the absence of a temporal order in the steady state, M causes P in Simon's sense. This seems a natural use of 'cause'. In the short run, a change to any of the parameters of Structure IV compels a change to both M and P in a well-defined temporal succession. But the steady-state value of M cannot be affected by the setting of δ or β, while any change to its own steady-state value due to a change in τ forces the steady-state value of P to change.

Economic theory is largely about steady states. The economic theory of transitional dynamics has little empirical support and carries little conviction among economists. At best, economists have phenomenal laws describing short-run behavior. Very different short-run behaviors are compatible with the same steady states: *e.g.*, if $\phi = 0$ in equation (7), P would not cause M even in the short run. One might argue that this is just a matter of ignorance; and, that as economics progresses, the transitioinal details will be filled in, so that steady states are fully explained by the dynamic processes that generate them. Unfortunately, informational complexity and nonhomogeneity would force an explanation that describes each individual's behavior. Even if that were

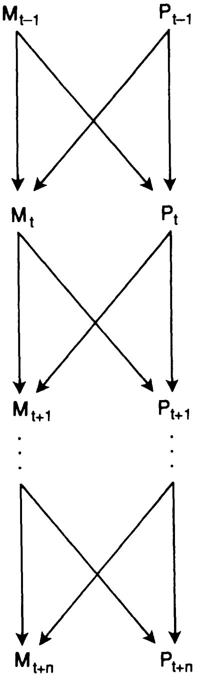

FIGURE 2.

possible, the explanandum has shifted to the individual from the macroeconomic variable: macroeconomics would be denatured.[11]

7 EVEN ECONOMISTS DON'T KNOW HOW TO GET CAUSES FROM PROBABILITIES

My argument has been that a causal concept that fundamentally requires temporal ordering is not compatible with macroeconomics as it is practiced. It might be argued that further knowledge and further advances will lead to a macroeconomic theory in which all causal relations are temporally ordered. While I cannot absolutely rule it out, I believe that there are good reasons not to believe that macroeconomics will develop that way. It is a not very well-supported prejudice to insist that it will. One might agree with my prognosis for macroeconomics, yet still continue to insist on temporal ordering: if macroeconomics cannot be beat into that mold, so much the worse for macroeconomics. Such a tack would ignore the fact that questions like 'If the Federal Reserve

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[11] I take Salmon ([1984], pp. 272, 273) to be making a similar point when he speaks of the difference between explaining plagues and explaining the death of an individual from the plague.

sells \$50,000,000 worth of Treasury bonds, will interest rates rise?' are not only sensible and perfectly well formulated, they also have a natural causal interpretation. Our concepts of causality must be adequate to their formulation and interpretation.

My argument has been directed largely against Cartwright's account of temporal order. Partly, this is because it is so clearly formulated; partly, it is because it is the account with which I am most familiar. One way to interpret my argument, however, is not so much as an attack on Cartwright's position as an elaboration of her thesis that to get causes from probabilities requires the input of a lot of causal knowledge. If temporal ordering does not provide a way of resolving observational equivalence, another way must be found. Simon's suggestion that a causal ordering is invariant to well-defined classes of interventions is, I think, precisely correct.[12] Simon's account bears a family resemblance to Salmon's [1984, ch. 5, 6] view that a causal process is one that will transmit marks. Cartwright ([1989], p. 234ff.) endorses Salmon's account of causal propagation, which suits her notion of capacities as invariant dispositions as well. I would go further and suggest, without fully developing the argument, that any account which relies on the invariance of capacities or structures and the propagation of interventions or marks can be cast as an acount based on contrary-to-fact conditionals. Cartwright's easy transition from S–R formulations to INUS conditions would seem to support this view. In contrast, Salmon's ([1984], pp. 185–90) dismissal of the INUS analysis along with all necessity/sufficiency accounts appears somewhat hasty, especially given his counterfactual formulation of causal interactions (p. 172).

In conclusion, as far as macroeconomics is concerned, an account of causal propagation will solve our most pressing problems, while insisting on temporal ordering threatens to undercut macroeconomic explanations altogether.

APPENDIX. AN ILLUSTRATION OF EXPECTATIONS-INDUCED REVERSAL OF GRANGER-CAUSALITY

The following is an illustration of reverse Granger-causality based on a model similar to one discussed in Hoover ([1988], pp. 187–89). All variables are measured in natural logarithms and indicated by upper case Roman letters; estimated residuals from regressions are indicated by lower case Roman letters; and all random (and pseudo-random) shocks are indicated by lower case Greek letters. Shocks are all mean zero, not serially correlated, constant variance and uncorrelated with other shocks. Superscript 'e' indicates expected values, subscripts refer to time periods, and $E_t(\cdot)$ is the mathematical

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[12] This is elaborated in Hoover [1990].

expectations operator conditional on information available at time t. Now consider the following model.

(1) $M_t^S = \sigma_t + \delta_t + \bar{M}$ (money supply function)

Here σ_t is a true stochastic variable; while δ_t is a pseudo-random variable—*i.e.* one generated from a deterministic function that emulates the statistical properties of a true random variable so well that its deterministic nature is virtually undetectable to anyone who does not know the actual function and initial values.

(2) $M_t^D = P_t - {}_tI_{t+1}^e + \varepsilon_t$ (money demand function)

where ${}_tI_{t+1}^e$ is the expectation at time t of inflation between times t and t + 1;

(3) $I_t = P_t - P_{t-1}$ (definition of inflation)

(4) $M_t^S = M_t^D = M_t$ (equilibrium condition)

From (1) to (4), we can determine current prices as a function of current money and expected future prices:

(5) $P_t = \frac{1}{2}M_t - \frac{1}{2}{}_tP_{t+1}^e - \frac{1}{2}\varepsilon_t$

Under the rational expectations hypothesis

(6) ${}_tP_{t+1}^e = E_t(P_{t+1})$

Proceeding on the assumption that the public does not know the true values of the pseudo-random variable δ_t and hence that it can be treated as a truly random variable, we can solve for prices. Advancing the subscripts in (5) by 1 and taking expectations

(7) $E_t(P_{t+1}) = \frac{1}{2}\bar{M} + \frac{1}{2}E_t({}_{t+1}P_{t+2}^e)$

On weak assumptions this can be restated

(8) $E_t(P_{t+1}) = \frac{1}{2}\bar{M} + \frac{1}{2}E_t(P_{t+2})$

Repeatedly advancing the subscripts in (5) taking expectations and substituting for the expectations term in (8) n times yields

(9) $E_t(P_{t+1}) = \frac{1}{2}\bar{M}_t \sum_{j=2}^{n} (\frac{1}{2})^j + (\frac{1}{2})^n E_t(P_{t+n})$

As $n \rightarrow \infty$, the right-hand term converges to zero, and the middle term converges to unity. Substituting into (5) yields

(10) $P_t = \frac{1}{2}M_t - \bar{M} - \frac{1}{2}\varepsilon_t$

Now consider the regression

(11) $P_t = a + bM_t + e_t$

It is obvious from inspection that $a \approx \bar{M}$, $b \approx \frac{1}{2}$ and $\mathrm{var}(e_t) \approx \mathrm{var}(\varepsilon_t)/4$. By construction earlier values for M cannot have explanatory power for P. Compare these results to those from a regression that omits M_t as a regressor

$$(12)\quad P_t = a' + e'_t$$

The $\mathrm{var}(e'_t) = \mathrm{var}(P_t - a) = \mathrm{var}(bM_t + e_t) \approx b^2\mathrm{var}(\sigma_t) + b^2\mathrm{var}(\delta_t) + \mathrm{var}(\varepsilon_t)/4$. Since the $\mathrm{var}(e_t) < \mathrm{var}(e'_t)$, M_t *instantaneously Granger-causes* P_t.

Now consider the regression

$$(13)\quad M_t = c + dP_t + v_t$$

It is obvious by construction that $c \approx \bar{M}$, $d \approx 0$ and $\mathrm{var}(v_t) \approx \mathrm{var}(\sigma_t) + \mathrm{var}(\delta_t)$. Compare these results to those from a regression that omits P_t as a regressor

$$(14)\quad M_t = c' + v'_t$$

Again it is straightforward to see that $c' \approx \bar{M}$ and $\mathrm{var}(v'_t) \approx \mathrm{var}(v_t) \approx \mathrm{var}(\sigma_t) + \mathrm{var}(\delta_t)$. Obviously P_t does not instantaneously Granger-cause M_t.

Now consider the case in which the public but not the econometrician knows the true values of δ_t and uses them in the process of forming their expectations of future prices. We may modify the derivation above starting with (7) which becomes

$$(7)'\quad E_t(P_{t+1}) = \tfrac{1}{2}\bar{M} + \tfrac{1}{2}\delta_t + \tfrac{1}{2}E_t({}_{t+1}P^e_{t+2})$$

Following through the derivation, we get

$$(9)'\quad E_t(P_{t+1}) = \tfrac{1}{2}\bar{M}_t \sum_{j=2}^{n} (\tfrac{1}{2})^j + \sum_{j=2}^{n} (\tfrac{1}{2})^j\, \delta_{t+j-1} + (\tfrac{1}{2})^n\, E_t(P_{t+n})$$

and, finally,

$$(10)'\quad P_t = \tfrac{1}{2}M_t - \bar{M} + \sum_{j=2}^{n} (\tfrac{1}{2})^j\, \delta_{t+j-1} - \tfrac{1}{2}\varepsilon_t$$

Again consider the regression

$$(11)'\quad P_t = a + bM_{t-1} + e_t$$

It follows directly that $a \approx \bar{M}$, $b \approx 0$ and $\mathrm{var}(e_t) \approx \mathrm{var}(\delta_t) + \mathrm{var}(\varepsilon_t)/4$.

In constrast, for the regression that omits M_{t-1}

$$(12)\quad P_t = a' + e'_t$$

$\mathrm{var}(e'_t) = \mathrm{var}(P_t - a) = \mathrm{var}(bM_t + e_t) \approx b^2\mathrm{var}(\sigma_t) + (1 + b^2)\mathrm{var}(\delta_t) + \mathrm{var}(\varepsilon_t)/4$.

Since the $\mathrm{var}(e'_t) > \mathrm{var}(e_t)$, M_t Granger-causes P_t.

Now consider the regression

$$(13)\ {}'M_t = c + dP_{t-1} + v_t$$

$$d \approx \mathrm{cov}(M_t,P_{t-1})/\mathrm{var}(P_{t-1}).\ \mathrm{Cov}(M_t,P_{t-1}) = E[(M_t - \bar{M}_t)\,(P_{t-1} - \bar{P}_{t-1})]$$

$$\approx E[(\sigma_t + \delta_t)\,(\tfrac{1}{2}\sigma_{t-1} + \tfrac{1}{2}\delta_{t-1} + \sum_{j=1}^{\infty} (\tfrac{1}{2})^j\, \delta_{t+j-1} - \tfrac{1}{2}\varepsilon_{t-1})]$$

$$= \tfrac{1}{2}\mathrm{var}(\delta_t) > 0.$$

Now since it is obvious that $\mathrm{var}(P_{t-1})>0$, $d>0$. It is extremely messy to calculate $\mathrm{var}(v_t)$. However, the fact that $d>0$ combined with the fact that omitting a variable from a regression cannot decrease the residual variance of the regression if sample sizes are large enough, implies that P_{t-1} has predictive power for M_t: *i.e.* P_{t-1} Granger-causes M_t, which is the reversal of causal direction that we sought.

It might be objected that in the case in which the public does not know δ_t, we have sought only instantaneous Granger-causality, and indeed that we have not played by the rules of strict temporal ordering because money demand and supply and expectations were all dated in the initial model at time t. It is, however, easy to write down a model in which temporal ordering is respected that is exactly analogous to the model examined here. Solutions to that model have precisely the same character as those presented here, but are even more tedious and messy to derive.

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