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THE LOGIC OF CAUSAL INFERENCE

Econometrics and the Conditional Analysis of Causation

Kevin D. Hoover
University of California, Davis

> Discontented people might talk of corruption in the Commons, closeness in the Commons and the necessity of reforming the Commons, said Mr. Spenlow solemnly, in conclusion; but when the price of wheat per bushel had been the highest, the Commons had been the busiest; and a man might lay his hand upon his heart, and say this to the whole world, – 'Touch the Commons, and down comes the country!'
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> Charles Dickens, *David Copperfield*

> The worst of him is that he is much more interested in getting on with the job than in spending time in deciding whether the job is worth getting on with. He so clearly prefers the mazes of arithmetic to the mazes of logic, that I must ask him to forgive the criticisms of one whose tastes in statistical theory have been, beginning many years ago, the other way round.
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> J. M. Keynes, "Professor Tinbergen's Method"

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No one denies that causal inference is fraught with difficulties. But among economists, those who prefer the mazes of arithmetic far outnumber those who prefer the mazes of logic. Let me therefore declare my thesis at the start: the analysis of causality is not a problem in statistical technique; it is a problem in the logic of empirical inference. Statistical techniques are of course important to the practice of inferring causal direction; better statistical techniques will no doubt improve causal inference; but, in order to deploy those techniques effectively, an appropriate understanding of the concept of causal order is needed.

Claims about causal order are implicit throughout empirical economics.[1] Yet economists on the whole betray impatience with any careful examinations of the conceptual issues surrounding the analysis of causality and the problem of causal inference. The general attitude is, ignore interminable and boring metaphysics and get on with the job.[2]

A cautionary tale from another discipline reveals the pitfalls of such an attitude. The medical profession, by and large, believes that the retroviruses HIV-1 and HIV-2 cause acquired immune deficiency syndrome (AIDS). However, the imminent virologist Peter Duesberg (1987, 1988a, 1988b) argues that the HIV viruses are not the cause of AIDS. Since they were set down by Robert Koch in the nineteenth century, medical researchers have generally agreed that an organism causes a disease if and only if (i) it is found in all cases of the disease; (ii) it has been isolated and grown in pure cultures; and (iii) when the pure culture is inoculated into man or animals, the disease occurs in every case (Duesberg, 1987, p. 1199, fn. 3; 1988a). Duesberg argues that the HIV viruses fail to fulfill these criteria.

A number of famous AIDS researchers argue against Duesberg along two lines. First, they offer evidence that the HIV viruses in fact fulfill the criteria. More interestingly, they argue that these criteria are merely heuristic, codifying sound laboratory practice, but falling short of a definition of causality (Blattner, Gallo, and Temin, 1988a, 1988b; cf. Holland 1986, pp. 955–57). Implicitly, they argue that once a conceptually more appropriate definition of causality is adopted, existing evidence is adequate to establish the causal role of the HIV viruses.

The conceptual debate over what is a cause in medicine is not a bit of airy philosophizing, but could govern the direction of medical research

1. And sometimes explicit, as in the debate over the causal direction between money and prices (or nominal income). See inter alia Fisher ([1911] 1931), Friedman and Schwartz (1963a, 1963b), Tobin (1970), Friedman (1970), Sims (1972), Kaldor (1982), and King and Plosser (1984).
2. The popularity of tests of "Granger-causality" is partly owing to the ease with which they may be econometrically implemented and an easy equivocation between Granger's (1969, 1980) carefully delimited notion of causality and other nonequivalent senses. For a general nontechnical discussion of Granger-causality, see Hoover (1988); for a discussion of its relationship to other notions of causality, see Zellner (1979).

and clinical practice. Economics poses less vital (or fatal) questions than does medicine; yet misjudgments of economic policy due to faulty understanding of causality might subtract more from human happiness than medical misjudgments ever have.

Economics has not lacked discussions of causality.[3] But there has often been a tension between econometric discussions in which operationalism is stressed and methodological discussions the practical implications of which are unclear. In this article I attempt to ease this tension somewhat. I suggest a scheme for gathering evidence relevant to causal inference that is at once operational and philosophically well grounded. As befits an article that stands at the juncture between philosophy and econometrics, the examples of causal inference are kept simple to highlight the principles involved. This article is nonetheless part of a larger program, the aim of which is to develop and apply techniques of causal inference to concrete problems in economics. Hoover (1990) and Hoover and Sheffrin (1990) apply the techniques sketched here to the determination of the causal direction between money and prices and between taxation and government expenditure.

The work draws on several sources. The central source in the economics literature is Herbert Simon's (1953) article, which defines causal order in linear systems. Simon's article is not unknown to econometricians, but they seem rarely to have appreciated how it can be used as a foundation for an inferential scheme. Properly understood, Simon's analysis identifies the causal ordering among variables as a property that is invariant to interventions of control over the parameters governing those variables. That causality has something to do with invariance under control is fairly close to commonsense ideas. It is not surprising, then, that economists in applied work from time to time use invariance to interventions as a hallmark of causality. Nevertheless, a systematic articulation of an inferential scheme based on this idea has not, as far as I know, been attempted in the econometrics literature. My principal aim, therefore, is to generalize and extend Simon's analysis until it is rich enough to capture actual econometric problems and form an adequate basis for such a scheme. To do this, I will have to clarify a critical distinction between variables and parameters that is only partially articulated in Simon's analysis, and I will have to supplement that analysis with the useful notion of a causal field, borrowed from the philosophical literature.

Of course, a fully articulated notion of a causal ordering and an operational inferential scheme are all for naught if the notion itself is not conceptually sound. The second aim of this article, then, is to dem-

3. See, e.g., Hicks (1979), Addison, Burton, and Torrance (1980a, 1980b), and Hammond (1986). Also, an entire number of the *Journal of Econometrics* (1988) is devoted to causality.

onstrate that the econometric analysis is compatible with and supported by a coherent philosophical account of causality. I rely mainly, but not exclusively, upon J. L. Mackie's (1980) analysis of causal relations based on counterfactuals.

A FORMALIZATION OF CAUSAL ORDERING IN ECONOMETRICS

The Syntax of Cause

Simon (1953) considers simultaneous systems of 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. Causality is therefore related to block recursion in simultaneous systems. To illustrate Simon's analysis, begin with a simple linear system.

$$p_{11}q_1 = p_{10} \quad (1)$$

$$p_{21}q_1 + p_{22}q_2 = p_{20} \quad (2)$$

$$p_{33}q_3 = p_{30} \quad (3)$$

$$p_{42}q_2 + p_{43}q_3 + p_{44}q_4 = p_{40} \quad (4)$$

where the q_j are variables and the p_{ij} are parameters. The distinction between variables and parameters is roughly that parameters are "variables" subject to direct control, and that variables without further qualification are subject only to indirect control. This may appear to be an unusual distinction, but I postpone a more thorough discussion until after the broad outlines of Simon's analysis are clear.

Equation (1) is a *minimal self-contained subsystem* of equations (1)–(4): if one knew the values of the elements of the field and of the parameters p_{10} and p_{11}, one could determine the value of q_1 in equation (1) without reference to any other equation. Equation (3) is also a minimal self-contained subsystem. Equations (1) and (2) together are also a *self-contained subsystem,* although not a minimal one: once one knows their parameters, both q_1 and q_2 can be determined. Since the value of q_1 is independent of the value of q_2, whereas the value of q_2 is not independent of the value of q_1, q_1 causes q_2. Equations (1)–(3) are self-contained. Similarly, equations (1)–(4) are self-contained: q_1 causes q_2, and q_2 and q_3 cause q_4.

The Semantic Problem

Causal orderings, as I have defined them so far, are purely formal (syntactic) properties of formal systems. If we already know the system – its parameters, variables, and functional forms – then the analysis given permits us to say in a well-defined manner exactly what causes what. In empirical work, however, we generally have observations on variables, have at best some theoretically based guess of the functional forms, and must estimate the parameters. In such a case, determining causal order presents a problem. A causal ordering in Simon's analysis is a relationship between variables. Even if we restrict ourselves to linear systems, equations **(1)**–**(4)** are only one of an infinite number of representations which determine precisely the same values for the variables. Each has a different parameterization, and every possible causal order may be represented.

For the system to represent a definite causal order, it is necessary to make the additional stipulation that the parameters are independent in the sense that the value of one has no implications for the values of the others. To see what is at stake, consider the simpler system:

$$a_{11}x_1 + a_{12}x_2 = a_{10} \tag{5}$$

$$a_{22}x_2 = a_{20}, \tag{6}$$

where the a_{ij}s are parameters and the x_js are variables. In this system x_2 apparently causes x_1. Compare this with the system:

$$a_{11}x_1 + a_{12}x_2 = a_{10}, \tag{7}$$

$$b_{21}x_1 + b_{22}x_2 = b_{20}. \tag{8}$$

Here x_1 and x_2 would appear to exhibit mutual causation or simultaneity. The values of the variables, however, may be the same in each system. This is possible, for example, if either of two sets of identities holds:

$$b_{21} = a_{11},\ b_{22} = a_{12} + a_{22} \text{ and } b_{20} = a_{10} + a_{20}, \text{ or} \tag{A}$$

$$a_{22} = (a_{11}b_{22}/b_{21}) - a_{12} \text{ and } a_{20} = (a_{11}b_{20}/b_{21}) - a_{10}. \tag{B}$$

Knowing which causal order actually describes the variables is then a matter of knowing whether the a_{2j}s or the b_{2j}s are the true parameters. If the a_{2j}s are true parameters, they may be chosen independently of each other and independently of the a_{1j}s. But then no matter how the a_{1j}s are chosen, the b_{2j}s must adjust to maintain the identities **(A)**. Equally, if the b_{2j}s are true parameters, they may be chosen independently of each other and the a_{1j}s, and the a_{2j}s must adjust to maintain the identities **(B)**. The problem is that there is no choosing between these forms simply on the basis of the observed values of the variables: the two systems are *observationally equivalent*. Yet, which causal order is correct makes a considerable difference. Suppose that we can intervene and alter the value of the parameter a_{11}. If the first ordering is correct, only the value of the x_1 is altered; whereas if the second order is correct, the values of both x_1 and x_2 are altered.

Simon (1953, pp. 24–26) offers a solution to this problem. Assume that there exist experimenters (among whom nature counts as one) who can alter the parameters of a causal system. This class of interventions defines a new higher order relation called *direct control*. If by altering a parameter, say a_{10} in (7), the experimenter can change the value of a variable, say x_2 in (8), he has indirect control over x_2. "The causal ordering specifies which variable will be affected at a particular point (a particular complete subset) of the structure" (Simon 1953, p. 27). That is, the causal order is the property of the economic structure that determines which variables can be altered independently of which other variables.

Causal relations on Simon's view are invariant to interventions among the parameters. Simon refers to the step up to talk of actual or hypothetical interventions as the adoption of a "metalanguage" of direct control. The point is that a distinction must be drawn between the formal representations of the causal relations and the actual causal relations that those representations are meant to capture. The fact that variables show a particular functional relationship or pattern of covariance does not capture the essence of causality. The important thing is that those relations remain stable in the face of interventions of control (actual or hypothetical). Solving the problem of observational equivalence through an appeal to interventions of control is an implicit recognition that cause is not a formal property of a model without reference to the real world. Subsequently, Simon was more explicit: the formal, syntactic properties of causality within models must be supplemented by semantic interpretation if they are to have content (see Zellner, 1979, p. 25; also see Simon, 1955, pp. 194, 195).

The problem of observational equivalence arises in various guises. Simon sees it as a problem of selecting causal orderings (cf. Basmann, 1965, 1988; Granger, 1969, p. 374). What distinguishes one causal order from another for Simon is the invariance of the remaining members of a set of parameters to an intervention of control over one of them. The so-called Lucas critique is simply the claim that estimated econometric models do not usually coincide with the true underlying causal structure but with an observationally equivalent form (Lucas, 1976). The estimated coefficients are not invariant but rather complex functions of invariant parameters. Cooley and LeRoy's (1985) and Bernanke's (1986) criticisms of the use of vector autoregressions (VARs) in the analysis of policy can also be seen as pointing out the problem of observationally equivalent causal orderings. Proponents of the VAR methodology suggested that the technique was atheoretical. But, in fact, their proposed methods of decomposing the estimated variances amounts to an imposition of a causal ordering – that is, to the implicit assertion of the invariance of particular estimated parameters. The critics of VAR methodology merely point out that, although these claims of invariance for certain parameters are crucial to policy analysis using VARs, there is rarely any substantial justification for the imposed causal order. Cooley and LeRoy agree with Lucas that the solution is to derive theoretically sound models from first principles, so that the causal ordering imposed on the data reflects the invariant or "deep" parameters (tastes and technology) of the economic system. In his famous paper on observational equivalence, Thomas Sargent (1976) points out that different theories may generate observationally equivalent forms for estimation. A single set of data cannot then discriminate between them.[4]

This last point is important, for it suggests the inferential scheme to be developed later in this article. Causal order is not a property of the statistically observed relations between variables. Rather it is a property of the underlying and not directly observable data-generating process. If we are to learn anything about the causal ordering of this data-generating process, we must move beyond a set of data drawn from a single regime. Controlled experiments alter parameters in particular minimal self-contained subsystems. Observing which relations between variables remain invariant to these interventions allows the experiment to discriminate between alternative causal orders. In economics, however, controlled experiments are rare. Institutional changes, new policies, technical innovations and so forth do, however, provide "fictive controlled experiments," which may be the basis for ascertaining causal relations.[5]

Parameters, Interventions, and Policy

At the semantic as opposed to the syntactic level, a causal ordering divides pragmatic economic concepts into variables, which are not directly controllable, and parameters, which are the direct subjects of interventions of control albeit invariant to interventions of control over other parameters.[6] A more common view treats a parameter simply as a variable that happens to be constant (Katzner, 1983, p. 90). The dif-

4. Theoretical monism would dissolve Sargent's problem by ruling out all but one theory a priori. In particular, Sargent (1976) shows that natural-rate and nonnatural-rate models are observationally equivalent. Lucas or Cooley and LeRoy could reject the nonnatural rate models without any empirical basis simply because those models violate what they regard as sound economic theory.
5. The expression in quotation marks is due to Wold (1954, p. 166).
6. Earlier, I referred to direct control over a variable. In light of the formal analysis of causality in this section, this should be glossed to direct control over the parameters that determine the variable.

ference between the definition of a parameter used in this article and this alternative definition is a matter of terminology and not of principle.

In contrast, Cooley, LeRoy and Raymon (1984a, 1984b) argue for the absolute constancy of parameters from wider principles. For them a variable is an economic quantity with a nondegenerate probability distribution. If a putative parameter changes because of an intervention of control, it should be classified as a variable. They argue that this is an implication of rational expectations: if people know that an economic quantity does in fact change, they assign some probabilities to the range of its values, and these probabilities correspond to the objectively correct probabilities.

This argument is based upon an overly strict and overly consistent application of rational expectations (cf. Hoover, 1988, Ch. 8, sec. 8.4; Lucas, 1987, pp. 8, 9, esp. fn. 1). The argument emasculates policy. If changes in policy cannot be described as changes in parameters, they cannot be described at all. Cooley et al. argue against this, that policy can be described as different series of realizations of random shocks to policy variables. But if variables are random with constant parameters, they are not chosen by policymakers; and, if they are not chosen by policymakers, they cannot describe changes in policy. Further, if the changes in these variables (putative parameters) are rare enough, there will be no basis for the public to form objectively correct assessments of relevant probabilities. Rational expectations could not then be applied, and it would be best to describe what occurred as a change in regime or change in parameters.

There is, however, a still more telling point. Cooley et al. draw the conclusion that an economic quantity that sometimes changes should be thought of as a variable with a well-defined probability distribution from a *particular* theory – the rational expectations hypothesis. In defining what causal structure is within any economic theory, the truth of a contingent hypothesis such as rational expectations cannot be taken as a theoretical presupposition.[7]

Which economic quantities are to be taken to be parameters and which variables is not a simple matter of whether or not they sometimes change. Rather it is a pragmatic choice based on substantial knowledge (usually institutional) of whether or not they are directly controlled within the system with which we are immediately concerned. The parameters of a formal system (such as equations (**1**)–(**4**)) cannot be reduced to a single point (one value for each parameter) but must remain a space

7. Unless, of course, one is prepared to argue that the rational expectations hypothesis is not contingent but necessary: i.e., any true economic theory must embrace it a priori. Cooley and LeRoy (1985) seem to equate economic theory with optimization (despite the obvious nonsense this makes out of the history of economic thought). If the rational expectations hypothesis is seen as a deductive consequence of optimization, then they may be prepared to argue for its a priori truth.

that expresses the entire range of possible interventions of control over each parameter.

The view of the relationship between parameters and variables that I advocate here is not inconsistent with the commonsense view that some *variables* are subject to direct control. In the context of linear systems (at least), a directly controlled variable would have an equation in which a single parameter in fact specified the value of the variable.

THE CONDITIONAL ANALYSIS OF CAUSALITY

Although it is not universally accepted, Simon's analysis yields a common and natural gloss of "cause": A causes B if control of A yields control over B. I take control to be the key notion of cause. Control may be direct or indirect. Y is *indirectly controlled* if it is causally linked to X, and X is (directly or indirectly) controlled. Simon takes direct control to be primitive and spends most of his effort on explicating the indirect causal linkage. But the point of the AIDS example is that the conceptual soundness of a characterization of causality is pragmatically crucial. So, the question of whether Simon's analysis is conceptually sound must now be faced. I hope to show that an analysis based on conditional propositions provides an adequate philosophic foundation for Simon's characterization of causality. What is more, these philosophic considerations suggest ways in which Simon's analysis must be extended if it is to serve as the basis for a practicable scheme for the econometric inference of causal direction. These extensions, in turn, actually serve to enrich the original philosophic notions.

There have been a number of accounts of causality based on conditional or, more particularly, counterfactual propositions. Perhaps the two most prominent are those of David Lewis (1973a) and J. L. Mackie (1980). Here, I shall follow Mackie's account. While there is no need to justify this choice in detail here, it is not entirely an arbitrary preference. Lewis's and Mackie's accounts of causality in terms of counterfactuals are in fact quite similar. Where Lewis and Mackie differ more fundamentally is in the analysis of counterfactuals themselves. Lewis (1973b) analyzes counterfactuals in terms of similarity relations between possible worlds, whereas Mackie (1973, Ch. 3) analyzes them as ellipitical arguments. There is really no need to decide between these accounts here. Whichever account turned out to be finally favored could readily be adapted to the needs of causal analysis. I prefer Mackie's causal analysis largely because its vocabulary is attractive and well adapted to the pragmatic application of its counterfactual analysis to econometric inference.

INUS Conditions

Two questions can be posed: What is meant by "A is the cause of B"? What is it "in the objects" (in Hume's phrase) that constitutes the causal relation? Mackie (1980, Ch. 2) answers the first question, "A causes B" means that A is necessary for B in the circumstances, i.e., not-A implies not-B in the circumstances. Mackie carefully considers whether cause is not a matter of sufficiency as well as necessity and precisely how to flesh out "in the circumstances." I do not wish to recapitulate his subtle analysis here; for the relevant points to Simon's work can best be brought out in the context of Mackie's investigation of the second question. In Chapter 3, Mackie investigates to what extent a sophisticated regularity theory of causation adequately answers this question. A sophisticated regularity theory is an extension of Hume's notion that the only causal relation in the objects is the constant conjunction of what is called the cause with what is called the effect. Although the theory developed is not wholly adequate either as a description of what causality is in the objects or as a tool for analyzing Simon's account, it nonetheless illuminates some important features of causal inference in econometrics.

On Mackie's account a comprehensive set of antecedent conditions (A) *causes* a consequence (C) if and only if A is necessary and sufficient for C.[8] Hence, the complex proposition follows that if A is true, then C is also true; and if C is true, A is true. If it is possible by direct control to bring about A, then C will also be brought about. Actual control is not needed. For "A causes C" is true, sustains the counterfactual, "if A had been true, then C would have been true."[9] The set A does not rule out causal overdetermination. A should be taken to be the disjunction of every *minimally sufficient* subset of antecedent conditions for C. Each of these subsets (A_i) is the conjunction of conditions, or absences of countervailing conditions, such that if the truth-value of any of the conjuncts were different, it would no longer be true that A_i implies C. If one or more of the A_i are true, then C is true; and, if C is true, at least one of the A_i is true. The comprehensive set A may then be called the *full* cause, and each A_i may be called a *complete* cause of C.

For the most part, however, we do not seem to be interested in complete causes; and any requirement that a cause be necessary *and* sufficient for C seems overly strong. Necessity seems crucial: if C is true, A must be true; every consequence must have a cause. The notion of a full cause reminds us, however, that if C is a skinned cat, each A_i represents a different way to skin it. No one A_i is necessary for C, although A is.[10]

8. "Necessary" and "sufficient" cannot be translated here in terms of material implication. What sense of implication is appropriate in part of the larger question, what is the correct underlying analysis of counterfactuals? See Mackie (1973, Ch. 3).
9. I use the term "sustains," following Mackie, to indicate that the causal claim warrants our ordinary use of the counterfactual, even though the causal claim and the counterfactual may not stand in a truth-functional relation to each other.
10. Each A_i is, however, "necessary in the circumstances," where "the circumstances" are the absences of the other A_k, $k \neq i$. See Mackie (1980, p. 31).

Common usage suggests that we may wish to weaken the criteria for a cause still further. Any of the conjuncts of A_i may be thought of as a cause; although, unless A_i has only one element, this conjunct will not be sufficient for C. Hence, if A_1 consists of a certain density of water in the atmosphere and a temperature below the dew point while C is a rainstorm, not only is A_1 the cause of C, but most of us would willingly agree that the low temperature on its own was also *a* cause of the storm although not a sufficient (or perhaps necessary) cause. In order to capture this use of "cause," Mackie proposes that an antecedent a_j (an element of A_i) is *a* cause of C if a_j is an Insufficient, Nonredundant member of an Unnecessary but Sufficient set of antecedents of C.[11] This he dubs the *INUS condition*.

Using the INUS condition to define "cause" should appeal to the economist faced with complex economic problems. For any economic situation that we wish to explain, we probably do not know every A_i. Indeed, for any A_i we are unlikely to know every one of the conjuncts it comprises. And, even if we do know all or most of the conjuncts, all but a few may be of little interest to us, appearing as background or environmental considerations. The institutional structure of Wall Street may be of crucial importance in obtaining a particular yield curve for government securities; yet a bond trader at the New York Federal Reserve Bank is fully justified in referring to a particular open-market sale as the "cause" of a change in the yield curve, while ignoring the institutional structure.

We may in our minds divide up the universe of antecedent conditions of a consequence C into those which are relevant, A, and those which are irrelevant, non-A. A may be divided into its disjuncts, the A_i; and one or more may be selected for our special concern. The conjuncts of a particular A_i may also be divided into particular causes (INUS conditions) that command our attention and the remainder that we relegate to the *causal field*.[12]

The partition of a minimally sufficient subset of A into a cause and a field allows us to focus our attention on some aspect of a causal problem, while not forgetting that any particular cause may be only an INUS condition. Not all partitions, however, are equally worthy. In a well-worn example, the gas leak rather than the striking of a match is most usefully considered the cause of the explosion in a house, although the reverse would be true in a gas plant. In either case, the presence of air would normally be relegated to the causal field, although not if the

11. Each a_j is "necessary in the circumstances," where "the circumstances" are the absences of the other A_k $k \neq i$, since a_j is nonredundant. I am, of course, passing over issues of causal overdetermination, although Mackie (1980, pp. 43–47) discusses these in some detail.
12. The notion of a causal field originates with Anderson (1938).

explosion took place on the moon. In general, the causal field contains the background circumstances and standing conditions that may safely be taken for granted.

Singling out one cause from a sufficient set may simply express a "conversational point" (Mackie, 1980, p. 35). The notion of a causal field has an obvious normative or legal use. It is true that if Lincoln had not been at Ford's Theater or Booth's pistol had not been loaded, then Booth's pulling the trigger could not have caused Lincoln's death. But for attributing legal or moral blame, the first two conjuncts of the minimally sufficient set of conditions for Lincoln's death are rightly placed in the field, and our complete attention is directed to the assassin's action.

It would be wrong, however, to believe that the causal field has only a normative use. The causal field of any problem is also a pound for those standing conditions which simply do not change.[13] Equally, although the causal field may be known to change, it may still be sufficiently stable to represent the boundary conditions for our particular causal interest.

Any variable that causes another in Simon's sense may be regarded as an INUS condition for that other variable.[14] Say that q_1 in equations (**1**)–(**4**) takes the value 17 and q_2 takes the value 8. The variable q_1 causes q_2. It being 17 is, however, insufficient for q_2 to be 8; that depends on the values of p_{21}, p_{22}, and p_{20} as well. But given these values, $q = 17$ is nonredundant; were q_1 to be 18, q_2 would not be 8. Yet, the set of particular values of q_1, p_{21}, p_{22}, and p_{20} is sufficient but not necessary for $q_2 = 8$; there is an infinite number of alternative sets of values for these variables that make $q_2 = 8$.

One objection to relating Simon's account of causation to Mackie's INUS conditions is that the conditional statements that relate antecedents to consequences in Mackie's account do not refer to measured quantities, whereas Simon's notation implies (and my example presumes) that variables are functionally related quantities. Simon's approach need not deal in quantitative variables. Simon (1952) presents a similar analysis in fairly general set-theoretic notation, while the generalization of Simon's analysis due to Mesarovic (1969), which is further developed in the Appendix, does not restrict variables to be measurable. Furthermore, there is no bar to interpreting functional relations in terms of INUS conditions. Mackie (1980, Ch. 6) distinguishes between *neolithic* (or unquantified) cause and *functional* (or quantified) cause. A functional

13. This can be compared to the notion of nonexcitation in econometrics; see Engle et al. (1983, p. 285).
14. The suggestion that causal variables are INUS conditions has been put forth independently by Cartwright (1989, pp. 25–29).

cause implies a neolithic cause: e.g., the fact that dropping a ball from Carfax Tower causes it to fall at a velocity of $\frac{1}{2}gt^2$ implies that dropping a ball from Carfax Tower causes it to fall.

Mackie (1980, pp. 77–80) is at pains to deny that causal claims are necessarily general. On the one hand, the fundamental notion of a cause is that the cause is necessary in the circumstances for the effect; but any such particular causal claim implies no regularity whatsoever. On the other hand, generalizations from observed regularities may sometimes sustain the counterfactuals involved in singular causal judgments. "But even here it is the generalization that supports the causal statement, rather than the causal statement that implies the generalization" (Mackie 1980, p. 78). Furthermore, Mackie argues that general causal statements should be viewed as "quantified variants of the corresponding singular ones," singular causal statements being regarded as primary.

Simon's analysis is explicated, as in my example, using deterministic linear functions. But the acknowledged raison d'être of his paper is to relate causality to the econometric problem of identification. Simon ignores random error terms because he is making a point about the structure of systems of simultaneous equations for which they would be a notational encumbrance. But Simon means for his point to carry over to *estimated* systems of simultaneous equations for which error terms are essential. Such estimated systems should be thought of as empirical observations of regularities among economic variables – as generalizations. The cost of shifting to an econometric interpretation of Simon is that we now must give an INUS account of the error terms. I shall attempt to do so presently, but first I must face up to the principal weakness of the INUS account.

Invariance, Interventions, and Asymmetry

Cause is an asymmetrical relationship: causes produce effects; effects do not produce causes. This is the shoal on which regularity accounts typically founder. That A is regularly associated with B does not tell us which is cause and which is effect. The INUS account is a sophisticated regularity account. And while A is an INUS condition of B need not imply that B is an INUS condition for A, this will often be the case. A common move is to ground causal priority in temporal priority.[15] A would be a cause of B, if it were an INUS condition of B and it occurred before B. Mackie (1980, Ch. 7) rightly rejects this move because it rules out ubiquitous cases of instantaneous causation – the moon causes the

15. Which is the foundation of the notion of Granger-causality in econometrics; see Granger (1980).

tides or my head causes the depression in my pillow – as well as backward causation, which may have a real model according to some theories of physics.[16]

Instead, initially at least, Mackie finds the source of causal priority precisely where Simon finds it: "The causally prior item, then, seems to be the one which we can directly control, and by which we can indirectly control the other. Causes are effective, effects are not (Mackie, 1980, p. 168).[17] An account of causal efficacy in terms of direct control is not entirely satisfactory in Mackie's view. First, it seems hopelessly anthropomorphic. Second, it seems to argue in a circle; for what is direct control but causal efficacy.

Although Mackie is right to be dissatisfied on purely philosophical grounds, I do not think that these problems need detain us long when dealing with pragmatic economics or that we need to concern ourselves with Mackie's replacement of direct control with a notion of *fixity* of causes and effects. Economics is about *human* decisions; an anthropomorphic approach is then generally appropriate. There are, to be sure, nonhuman factors sometimes involved in economic processes, and there are parameters (e.g., the aggregate marginal propensity to consume) that, although they are the product of human choices, are not within the direct control of any specific individual. But for pragmatic purposes, we take an anthropologist's view, rather than a policy advisor's view, of these factors and parameters. For econometrics, it is enough to perform the thought experiment "if we could directly control X, then . . . ," to define a workable concept of cause and to use the analogy, as Simon suggests, of nature with an experimenter to glean information useful to causal inference.

The circularity of defining causality through direct control cannot be gainsaid. Several authors have made the point that "a causal connection can be proved only from causal connections already known" (Anderson, 1938, p. 128).[18] This need not be a problem when some of our causal commitments are not really questioned.

In some cases, it takes neither deep theorizing nor sophisticated econometrics to know where direct control lies. In most countries, for instance, the central bank directly controls the monetary base. The central bank therefore causes changes in the monetary base. For pragmatic econometrics, it is enough then to notice that propositions about direct control, which may be disputable in another context, may sometimes

16. Simon (1952, p. 51) also rejects temporal priority as a basis for causal asymmetry for the same reason; cf. Simon (1953, p. 12). Mackie (1980, Ch. 7) also suggests two further reasons for rejecting temporal priority: viz., because the logical structures of temporal and causal asymmetry are different, and because temporal priority cannot account for the explanatory power of causes.
17. Cf. Cartwright (1983, Ch. 1), who stresses the effectiveness of causes.
18. Cf. Cartwright (1989, p. 39) and Spohn (1983, p. 380).

be taken to be indubitable in the process of inferring indirect causal orderings.

The importance of control in understanding causality highlights another key feature of causal relations: *invariance*. Anderson (1938, p. 126) observes, "on the assumption of variability, we could not say that there was any causal connection at all." Invariance is implicit in counterfactuals and related dispositional claims. A counterfactual cannot be rightly asserted when its antecedents are unfulfilled if, when its antecedents are fulfilled by direct control, it no longer entails the same consequence. Nancy Cartwright (1989) makes a similar point in an analysis of causal capacities: "If Cs do ever succeed in causing *E*s (by virtue of being C), it must be because they have the capacity to do so. That capacity is something they can be expected to carry with them from situation to situation" (p. 145). Invariance to direct control over its antecedents is a hallmark of a causal relation and a critical part of the scheme of causal inference I will propose later.

Structure of the Causal Field

It might be objected that the project of understanding Simon's account of causality as fundamentally related to Mackie's conditional analysis is misguided, because Mackie's analysis is deterministic while econometrics, toward which Simon's account is directed, is of its nature probabilistic. A first answer is to recall that, despite an implied relevance to econometrics, Simon's account employs only deterministic relations. Still, as we were forced to admit earlier, applications to econometrics require the introduction of random error terms. Mackie's deterministic analysis of causality is compatible with a (possibly) indeterministic world if these random error terms can rightly be impounded in the causal field. This, however, requires a richer notion of the causal field than originally proposed by Anderson or developed by Mackie.

In general, economic quantities that do not command our immediate interest may be impounded in the causal field. But this requires some elaboration. Having rejected the notion that parameters are constants, true constants should be impounded in the causal field and not in the parameter space. Parameters that do not command our immediate concern or that change only infrequently may also be impounded in the causal field. We must, however, be careful. When one of these parameters changes, it may have far-reaching effects on the rest of the causal structure. Such parameters are *boundary conditions*. Any conclusion drawn from a causal ordering is conditional on the values of these parameters.

Whether to create such a boundary condition is a matter of practical judgment. It is clear that very little economics could be done without placing such limits on the range of admissible interventions. Say, for example, that inflation causes interest rates along the lines of the Fisher theorem. This causal ordering is dependent on a background of limited regulation. If the government were to impose regulations similar to the Federal Reserve's regulation Q, which prevented interest rates from moving, the boundary condition would have been violated, and one would not expect the causal order to stand. Similarly, most of the causal orderings that interest us are conditional on a relatively free market economy. Shift to a Soviet-style command economy and many causal orderings would surely change.

In both these examples, it would be possible to move the relevant boundary parameters into the parameter space and to define causal orderings that are invariant to the presence or absence of financial regulation or to the dominant economic system. For some problems this would be an enlightening thing to do. And that is the sense in which it is just a matter of practical judgment. But, given the subject matter of economics, it is not possible to define causal orderings that are invariant to *all* interventions: some parameters will always appear in the causal field; no causal ordering will ever be wholly unconditional.

As well as containing true constants and those parameters that may be regarded as boundary conditions, the causal field may contain *variables* that are not central to our concerns. But what to impound in the causal field is not a matter of free choice. The causal field is a background of standing conditions and, within the boundaries of validity claimed for a causal relation, must be invariant to exercises of controlling the consequent by means of the particular causal relation (INUS condition) of interest. If extreme monetarists, for instance, correctly hold that money causes prices generally to rise and that the mechanisms for setting particular prices can be relegated to the causal field, then their cost-push opponents are wrong to think the partition can be worked the other way.[19] If a variable truly belongs in the field, it must not be caused by any of the variables that command our direct interest.

It is important to notice that variables impounded in the causal field need not be causally irrelevant: cause may run from a field variable to a variable of interest, but not from a variable of interest to a field variable. This is related to the well-known econometric proposition that a variable may be omitted from a regression without bias providing that it is uncorrelated with the remaining regressors. Random error terms, then, capture in part the effects of variables omitted from direct consideration and impounded in the causal field. This does not rule out irreducible random error – i.e., intrinsic indeterminism.

In general, whatever the source of their randomness, the random shocks in causally ordered stochastic systems may be treated as variables. Provided they fulfill the noncausality assumptions required of

19. Contra Cobham (1980).

other variables, they may be impounded in the causal field. Random shocks are described by their moments (mean, variance, etc.). It is possible that these moments are parameters that are subject to direct control. In that case, the random variables may be counted among the variables of interest in our formalization or impounded in the field with their parameters taken to be boundary conditions of the causal ordering. It is also possible that the moments are not parameters but variables, not subject to direct control but causally ordered with other variables.[20] In such a case, the random shocks and their moments must be counted among the variables and not relegated to the causal field.

Once random errors are considered, the causal field can no longer be considered to be fixed.[21] Indeed, once random error terms are introduced, at least some of the variables are no longer causally determined to take precise values. The whole system may be regarded as a probability distribution function. If, when every relevant INUS condition were released from the causal field, this distribution collapses, then the world would be truly deterministic. If the distribution were nondegenerate, the world would be truly indeterministic. The causal ordering, however, would connect only deterministic elements: nonrandom variables, parameters, and the moments of the random variables, which are classified as parameters or variables according to circumstances. The indeterminism apparently inextricably involved in econometrics is, therefore, compatible with Mackie's deterministic analysis. As for Papineau (1985, pp. 70, 71), it is not specific values of consequent variables that are causally determined, but the chances of their taking on values within some range.

Formalization and Generalization

Simon's analysis is compatible, as I have shown, with Mackie's account of causality. But Simon illustrates his analysis with deterministic, linear systems of real, continuous variables. Such a restrictive approach is not necessary. Simon (1953, pp. 34, 35) suggests that the analysis could be extended to nonlinear systems but does not pursue it very far. Mesarovic (1969) extends Simon's notion to very general mappings between variables, which need not even be measurable (see also Katzner, 1983, pp. 118–21). Mesarovic does not go far enough, however, to capture the issues considered in this article or to address some important issues in macroeconometrics. In the Appendix, I modify and extend Mesarovic's formalization so that it explicitly reflects the critically different roles of variables, parameters, and the field in the analysis of causality. Causal order is then defined analogously to Simon's notion of block recursion.

20. This is true for some ARCH models; e.g., Engle et al. (1987).
21. This is consistent with Mackie's (1980, p. 149) point that the causal field is specified broadly and largely negatively so that a causal regularity does not require a precise repetition of circumstances from instance to instance.

It turns out that to cover the case of cross-equation restrictions, such as are often engendered by rational expectations, an additional defining feature of a causal order – nesting of parameters – must be specified. This generalization of Simon clarifies many of the issues highlighted in this article and is useful in applied applications to rational expectations models and other nonlinear models (e.g., Hoover and Sheffrin, 1990). All details are left to the Appendix because its notational complexities are not needed for the main argument.

AN INFERENTIAL SCHEME

So far we have analyzed causality as an order imbedded in the true process that generates economic variables. The central econometric problem is that this process is *unobservable.* At best we have periodic observations on variables and some knowledge of institutions, which may allow us to say which variables are directly controlled and perhaps to identify particular interventions as belonging clearly to one or another part of the economic structure. The rest – the causal structure, the values of particular parameters, and so forth – must be inferred if they are to be known at all. Observational equivalence demonstrates that inferring causal structure from a single regime (i.e., from a single setting of the economic parameters) is hopeless. The question to be addressed is, what can be inferred about causal structure when data come from more than one regime?[22]

An Illustration

Causality is related to invariance. The presence or absence of invariance with respect to particular interventions provides evidence on the direction of causation. Consider a simple example of a data-generating process in which money (M) causes prices (P):

$$\mathrm{P} = a\mathrm{M} + \epsilon \qquad \epsilon \sim N(0, \sigma_\epsilon^2), \tag{9}$$

$$\mathrm{M} = b + \nu \qquad \nu \sim N(0, \sigma_\nu^2), \tag{10}$$

where $N(\cdot,\cdot)$ indicates a normal distribution characterized by its mean and variance, $\mathrm{cov}\,(\epsilon,\nu) = 0$, $\mathrm{E}(\epsilon_t\epsilon_{t-j}) = 0$ and $\mathrm{E}(\nu_t\nu_{t-j}) = 0$, $j = 1,2,\ldots\infty$. Clearly, *M causes P* on Simon's analysis.[23]

22. This question is implicit in some earlier studies of the stability of estimated economic relations across changes in regime: Neftci and Sargent (1978), Miller (1983), and Blanchard (1984). None of these authors, however, casts the problem as one of causal inference.
23. Note that the structure of the errors, particularly the diagonal variance/covariance matrix is really not very restrictive. We can always obtain such a form through a suitable transformation of the variable (cf. Bernanke, 1986, pp. 52, 53).

The reduced forms of equations (9) and (10) are

$$P = ab + av + \epsilon. \tag{11}$$

$$M = b + v. \tag{12}$$

Equations (11) and (12) describe the joint probability distribution of money and prices D(P,M). Elementary statistical theory tells us that such a joint distribution can be partitioned into a conditional distribution and a marginal distribution in two ways:

$$D(P,M) = D(M|P)D(P) = D(P|M)D(M).$$

Standard formulae can be applied to compute these distributions from equations (11) and (12).[24]

$$D(P|M) = N(aM, \sigma_\epsilon^2), \tag{13}$$

$$D(M) = N(b, \sigma_v^2), \tag{14}$$

$$D(M|P) = N([a\sigma_v^2 P + b\sigma_\epsilon^2]/[a^2\sigma_v^2 + \sigma_\epsilon^2], [\sigma_\epsilon^2\sigma_v^2]/[a^2\sigma_v^2 + \sigma_\epsilon^2]), \tag{15}$$

$$D(P) = N(ab, a^2\sigma_v^2 + \sigma_\epsilon^2). \tag{16}$$

The parameters of the price-determination process are a and σ_ϵ^2, and the parameters of the money-determination process are b and σ_v^2. Now suppose that we have some way of assigning interventions not to particular parameters, for we assume that the actual data-generating process cannot be observed, but to one or other of these two processes. For example, suppose that the Federal Reserve changes the conduct of monetary policy, then either b or σ_v^2 changes. In either case, D(M|P) and D(M) will change, as is to be expected; but notice that D(P) will also change and, crucially, that D(P|M) will be invariant. Suppose on the other side that a price control regime is introduced which alters either a or σ_ϵ^2. In either case, D(P|M) and D(P) will change; but notice that D(M|P) will also change and, crucially, that D(M) will be invariant. Because money causes prices in the true underlying data-generating process, the partition D(P|M)D(M) is clearly more stable to well-defined interventions than the partition D(M|P)D(P). If prices had caused money

24. See Lindgren (1976, Ch. 4, sec. 2) and Mood, Graybill, and Boes (1974, Ch. 10, sec 5).

in the data-generating process, these results would of course have been reversed.[25]

This suggests a general strategy for identifying causal orderings. Each of the conditional and marginal distributions in equations (13)–(16) can be interpreted as regression equations. It should be possible to use institutional and historical knowledge to identify periods in which there are probably no important interventions in either the money-determination or the price-determination processes. During such periods the regression equations should all show stable estimated coefficients. If we could then identify periods in which there are interventions clearly associated with the money-determination process and ones clearly associated with the price-determination process, we could check the patterns of relative stability of the alternative partitions and thereby determine with which causal ordering (if either) the data are consistent.

Misspecification of the Causal Field

The inferential scheme suggested here relies implicitly on the notion of the causal field. The random error terms in equations (9) and (10) are taken to be adequate summaries of the influence of field variables. But, as I pointed out earlier, whether a variable may be legitimately impounded in the causal field depends on its causal relationship to the variables which command our immediate interest. This has important consequences for practical causal inference.

In order to use statistical tests of stability to obtain evidence useful in inferring causal ordering, there must be an appropriate relationship between the variables and functional forms used empirically and the underlying data-generating process. The stability tests may be sensitive to the omission of certain regressors (and clearly not every potential regressor can be included). Similarly, we may have failed to transform the variables in such a manner that they correspond to the diagonal variance/covariance matrix of the data-generating process, so that causal relations between variables of interest are, in effect, hidden away in the supposed causal field. Or, equally, because our regressions are dynamically misspecified in a statistical sense, the estimated errors may not have the properties of randomness that would qualify them for inclusion

25. Rational expectations can complicate things. If money causes prices in a model in which agents base expected prices on a rational anticipation of the actions of the monetary authority, what Hendry (1988) calls a "feedforward mechanism," then D(P|M) is no longer invariant with respect to interventions in the money-determination process. It remains true, however, the D(M) is invariant with respect to interventions in the price-determination process. Hendry uses the difference between the case of rational expectations and the case presented in the text as a basis for discriminating between "feedforward" and "feedback" (or rule-of-thumb) mechanisms. Causal direction, however, is implicity taken as given.

in the causal field – e.g., they are not homoscedastic white-noise. These problems may be classified under the rubric "misspecification of the causal field." This terminology should not, however, mislead us. The causal field is an element of the structure of the data-generating process and not of the statistical procedures used to infer that structure. The terminology is useful nonetheless, because it is false assumptions about the causal field that encourage us to use inappropriate statistical procedures.

The possibility of misspecification of the causal field opens up the possibility that improved statistical procedures will force us to reconsider the import of empirical evidence. Any determination of causal ordering is necessarily tentative. This is, however, a general property of empirical inference. What we ultimately would want is a perfect correspondence between the true data-generating process and the statistical model used to gather evidence about it. This is impossible because the true data-generating process is unobservable: even if we had perfect correspondence, we would not know it with certainty. The chief virtue of a statistical model is, nevertheless, its truthlikeness, or verisimilitude with the underlying data-generating process. Although this process might be captured adequately by a theory or model, there do not exist sufficient criteria by which we may judge that we have obtained the final, best model.

We nevertheless do have widely accepted criteria that a truthlike model must necessarily meet, and we have standards by which to judge competing models.[26] Not knowing the actual process, we can nevertheless say that a model cannot resemble it unless its errors are random – that is, unless the part that we cannot explain is, at least provisionally, unexplainable, the model cannot be called truthlike. Typical criteria for randomness are: estimated errors should be white noise (i.e., not correlated with their own past – equivalently, they should have no autocorrelation); errors should be innovations (i.e., not correlated with other variables omitted from the model); and errors should be homoscedastic (i.e., of constant variance). If errors do not possess these properties, then it should be possible to formulate a different model that is better in the sense of having a lower variance and encompassing the first model. (In this case, *encompassing* means providing a basis for calculating what the coefficients and variance of the other model would be without in fact estimating it.)[27] In addition, on weak assumptions, statistical theory leads us to expect errors to be approximately normally distributed.

26. Fuller explanations and defense of these criteria are found in the writings of Hendry and his colleagues; see, e.g., Hendry and Richard (1982), Hendry (1983, 1986), Ericsson and Hendry (1984).
27. On the notion of encompassing, see Hendry and Richard (1982, pp. 16–20).

Theory may also guide econometric observations. At the crudest level, theory suggests potential variables. It also requires that models not produce values outside of the range of possible observations. A consumption function, for instance, must not generate predictions of negative consumption. On a higher plane, we may impose restrictions from economic theory on econometric estimates and test these restrictions against more general models. If they are accepted, then theory aids in understanding the significance of the observation; if not, the observation may suggest what element of the theory is unsatisfactory.

Another requirement is stability of coefficient estimates. Like consistency with theory, this criterion is on a different plane from the need for random errors. The very concept of randomness – unexplainability – justifies it as a necessary condition of verisimilitude. There is no necessary connection between stability and the true data-generating process. Economic reality no doubt changes – perhaps, even so frequently that stability is not to be found. Nevertheless, econometric observations would be practically useless if they were completely unstable. We must, therefore, count on finding some stability and on supplementing econometric observations with other information, say institutional facts, if we are to distinguish between real changes in structure and misspecification of our own statistical models. The criterion of consistency with theory is subject to similar strictures. It is useful only in that it aids interpretation of observations. We must not afford it overarching status. Observations must give grounds for reconsidering theoretical commitments.

CONCLUSION

Practical economists often dismiss philosophical analysis as irrelevant to their day-to-day concerns. Logical, philosophical, and methodological problems nonetheless arise repeatedly. Resolutely looking the other way does not dissolve the problem. In economics, as in other fields, knowing what constitutes a cause and how one might gather evidence relevant to ascertaining causes may have far-reaching practical consequences. Methodologists have unfortunately been content on the whole to deal with conceptual issues only. What I have shown in this article is, first, that a philosophical analysis of causality can be associated with and used to illuminate a more purely economic analysis and, second, that these analyses suggest a practical inferential scheme. Having wended our way through the mazes of logic, we now stand ready to face the mazes of arithmetic and the Minotaur of empirical evidence.

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APPENDIX

A GENERALIZATION OF SIMON'S ANALYSIS OF CAUSATION

Simon's original formalization of the causal relation was restricted to linear systems of equations with independent parameters. He notes that his analysis is easily extended to nonlinear cases (Simon, 1953, p. 34). In fact Mihajlo Mesarovic (1969) suggests an even more extensive generalization in the context of general systems theory.[28] My exposition extends and modifies Mesarovic in two ways. First, the notation is chosen to emphasize the close relation between the formalization and Mackie's conditional analysis of causality. Second, and more substantially, I draw a distinction between parameters and variables different from the usual distinction in the general systems literature. The reason is partly to give an explicit representation of the scope for interventions and partly to allow consideration of the issue of cross-equation restrictions that are important in rational expectations models.

Consider pragmatic economic concepts such as GNP, the marginal propensity to consume, velocity of circulation, or any other so-called variables or parameters of the economic system. Each ranges over some set of potential values. Variables or parameters are then sets, and each instantiation or value of a variable or parameter is an element of one of these sets. Values of variables or parameters may be indexed by time, in which case the variable or parameter is the set of the ordered pairs (the Cartesian product) that assigns each possible value to each possible time.

With respect to any economic system, causal factors can be divided into parameters (P_i; $P^0 = \{P_i\}$, $i = 1, 2, \ldots, n$) and variables, which are divided in turn into outputs (Q_i; $Q^0 = \{Q_i\}$, $i = 1, 2, \ldots, k$) and the field (F_i; $F^0 = \{F_i\}$, $i = 1, 2, \ldots, m$).

Let

$$F = F_1 X F_2 X \ldots X F_m,$$

$$P = P_1 X P_2 \ldots X P_n,$$

and

$$Q = Q_1 X Q_2 X \ldots X Q_k,$$

where "X" indicates the Cartesian product. A causally ordered system (C) can be represented as a mapping

$$C: FXPXQ \longrightarrow Q.$$

28. For a particularly succinct and lucid account of Mesarovic's idea, see Katzner (1983, Ch. 6).

This says that each combination of particular values for the inputs (F, P, and Q) is assigned to some particular values of the outputs (Q).

Let P′ be the Cartesian product of the elements of $P^1 \subseteq P^0$, and let Q′ be the Cartesian product of the elements of $Q^1 \subseteq Q^0$. Then a causally ordered subsystem $C' \subseteq C$ is called *self-contained* if and only if:

(i) C': $FXP'XQ' \rightarrow Q'$

and

(ii) $P^1 \subseteq P^0$, with equality only if $Q^1 = Q^0$.

Condition (i) says that a self-contained subsystem maps from a subset of the systems variables (Q^1) into itself. Condition (ii) says that a self-contained subsystem must in general contain only a subset of the parameters of the original system.

The intersection of self-contained subsystems of C is itself self-contained (Katzner, 1983, pp. 120, 121; Mesarovic, 1969, pp. 101, 102). And since there are a finite number of variables Q_i, there must be *minimal self-contained* subsystems: i.e., self-contained subsystems that do not themselves contain any smaller self-contained subsystems.

The elements of F^0 are *external causes* of the elements of Q^0: each is an INUS condition of the elements of Q^0; although by virtue of being impounded in the causal field, they are not the INUS conditions that command our immediate interest.

Consider a self-contained subsystem of C, C″ with variables Q^2 and parameters P^2, such that $C' \subset C''$. Clearly,

$$C'': FXP'XQ'XP''XQ'' \longrightarrow Q'XQ''$$

and

$$P^1 \subset P^2 \text{ and } P^2 \subseteq P^0, \text{ with equality only if } Q^2 = Q^0.$$

If there is no intervening subsystem C^* with parameters P^* such that

$$C' \subset C^* \subset C'' \text{ and } P^1 \subset P^* \subset P^2,$$

then the elements of Q^1 are *internal causes* of the distinct elements of Q^2.

If C″ contains another distinct self-contained subsystem C‴ with no intervening subsystems, then the elements of Q^3 are also internal causes of the distinct elements of Q^1.

In the system, equations (1)–(4) in the main text, $P^0 = \{p_{ij}\}$, $Q^0 = \{q_i\}$ and $F^0 = \{f_i\}$. Equations (1) and (2) together form a self-contained subsystem: $P^1 = \{p_{11}, p_{21}, p_{22}\} \subset P^0$, and $Q^1 = \{q_1, q_2\}$ is mapped into itself. It is *not* a minimal self-contained subsystem since equation (1) is itself self-contained (and in fact minimal). Because no subsystems intervene between equation (1) and the systems (1) and (2) together, q (internally) causes q_2: it is an INUS condition for it.

The importance of the parameter condition (ii) in the definition of self-containment is illustrated in the following system.

$$\alpha q_5 + \delta = \epsilon \tag{A.1}$$

$$\delta q_6 = \epsilon \tag{A.2}$$

Now $P^0 = \{\alpha, \delta, \epsilon\}$, $Q^0 = \{q_5, q_6\}$ and $F^0 = \varnothing$.

Judged simply by mappings of outputs into outputs, both equations (**A.1**) and (**A.2**) appear to be self-contained. But only (**A.2**) fulfills the condition

$$P^6 = \{\delta, \epsilon\} \subset \{\alpha, \delta, \epsilon\} = P^0.$$

It is self-contained; equation (**A.1**) is not; and the system (**A.1**) and (**A.2**) is. Therefore, q_6 causes q_5: i.e., any choice of q_6 through the choice of parameters of (**A.2**) influences the value of q_5; but holding δ and ϵ constant, the choice of q_5 through the choice of α does not influence q_6.

The interrelationships between equations mediated through parameters rather than or in addition to interrelationships mediated through variables are typical of the cross-equation restrictions that arise naturally in rational expectations models.