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VECTOR AUTOREGRESSIONS FOR CAUSAL INFERENCE?

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The slippery issues of causal inference have been kept in the econometric closet for over thirty years since Marschak (1953), Koopmans (1953), and Simon (1953) wrote about them. Those who rummaged in that closet in the intervening years no doubt quickly concluded that these were problems better left alone lest they devour our energies with little to show for the effort. The work of Christopher Sims on causal relationships between money and income has reopened these issues by challenging a basic conclusion of the Cowles group that causal inferences require a model and prior notions about causal directions. In addition to Sims, several others have recently begun to rethink these issues. Pratt and Schlaifer (1984) question our understanding of the notion of a "structure." Engle, Hendry, and Richard (1983) attempt to define "exogeneity" and give us new word pairs to mull over: weak, strong, and super exogeneity. From an entirely different perspective Kalman (1982) proposes methods to determine the number of linear relationships in a data set and disdains causal interpretations as prejudice.

The fundamental question that underlies the recent work by Sims and others on vector autoregressions is an old one: can we make causal inferences from nonexperimental data? If yes, what is the program for doing so? The Cowles Commission offered a clear yes to this question and a definite program for producing an answer. The data are assumed to have been generated by a system of simultaneous equations with an adequately long list of "exogenous" variables. The uncertain parameters in the system are then estimated by one of several methods. T.C. Liu's comment that the necessary identifying restrictions are not likely to be available is a

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minor irritant that has not been sufficient to deter stronghearted applied econometricians who have been able to find an abundance of "instruments" from the lengthy list of macro variables.

But the whimsical way that instrument lists are selected, together with an undercurrent of opinion that nothing is truly exogenous, led Sims to suggest an alternative program built around vector autoregressions. This program proceeds "without pretending to have too much a priori economic theory" (Sargent and Sims (1977)) but nonetheless presents causal inferences such as: "imposition of a monetarist rule to make the quantity of money more predictable would have had little real effect" (Sims, 1980b, p. 253), and "The Optimal Control of the Money Supply" (Litterman, 1983).

I take it for granted that vector autoregressions can be very useful for forecasting, as argued by Litterman (1979, 1980). I also take it for granted that vector autoregressions can be useful descriptive devices. Furthermore, with an adequate foundation in economic theory, vector autoregressions can and have been estimated within the framework of the Cowles program. Indeed, much of the work by Sims and others has a foundation in economic theory. The question that I pose in this paper is whether the vector autoregression program is an alternative to the Cowles program for drawing causal inferences. In particular: can causal inferences be made in the context of vector autoregressions without relying on a priori theory? The answer is quite clearly no. If a modification of the system is directly tested, then observed correlations can be used to plan future modifications without reference to a theory. Otherwise, the causal interpretation of observed correlations definitely requires a rather well-defined theoretical structure that can serve as a justification for acting as if the data were generated by a trial modification. I therefore conclude that the vector autoregression program is not an adequate substitute for the Cowles program.

The vector autoregression program is basically a response to rather telling criticisms of the Cowles program. These criticisms remain in effect even if the vector autoregression program is found lacking. The broader question that I therefore consider is whether there is any way at all to draw convincing causal inferences from macroeconomic data. I am skeptical. Macroeconomic fluctuations are thought to be a consequence either of coordination failures or informational inadequacies. The Cowles program seems reasonably well-suited to the study of models with coordination failures (e.g., sticky wages). The program can be criticized for requiring complete commitment to identifying restrictions. In particular, it offers no comment on the practical conundrum facing every data analyst: which variables from an enormous set of alternatives should be used as instruments? I believe that these criticisms can be adequately met only by adopting a Bayesian approach which requires at most a partial commitment to identifying restrictions in the form of a prior distribution. Of course, the shortcoming of the Bayesian approach is that it requires a complete commitment to the choice of prior distribution, but if a sensitivity analysis is performed, that commitment can be weakened to the point of acceptability.

On the other hand, if the sources of macroeconomic fluctuations are informational inadequacies, I doubt whether any version of the Cowles program would allow convincing causal inferences from the currently available data. The reason for this conclusion is that once information is allowed to play a leading role in the macro drama, the phenomenon of transitional expectations has to be acknowledged. Though a theory can make use of an assumption of settled expectations, no credible data analysis can. But no econometric analysis of the normal macro time series can be expected to yield much evidence about the subtle nonlinearities that must be present if transitional expectations are important. To put this differently, it is too optimistic to expect our weak data sets to yield estimates of both the nature of expectation formation and the rest of the system as well. To finish on a positive note, I call, therefore, for an effort devoted to the direct measurement of expectations. The Livingston data set represents a tiny step in that direction. Armed with direct measures of expectations, it is at least conceivable that we can draw causal inferences about macroeconomic phenomena. This suggestion has been greeted with amusement, to which I will respond in kind below.

Parenthetically, it is worth remarking on the extent to which the Lucas critique has captured the imagination of macroeconomists and has had the effect of emphasizing informational inadequacies as the source of macro fluctuations. But the vast majority of the empirical work that adopts Lucas' viewpoint uses the assumption of settled expectations, which I do not think is worth taking seriously at an empirical level. There is, as a result, no convincing data evidence that informational shortcomings are at all important, and it is consequently remarkable how much the profession seems to have moved in response to an untested theoretical suggestion. This episode is an example used by McCloskey (1983b) to demonstrate the inability of the positivistic tradition in economics to explain how opinions are formed. Coase (1982) makes the same point.

As McCloskey (1983a) would expect, it is the rhetoric, not the findings, of the vector autoregression program that has captured the imagination of the profession. The direction of causation and the property of exogeneity are claimed to be empirical issues, in sharp contrast to the Cowles program which takes these as given a priori and untestable. But these remarkable innovations turn out to be entirely definitional. I do not pretend that the choice of definitions is easy, but I believe that the ones I present conform to majority usage. It is my surprising conclusion that economists know very well what they mean when they use the words "exogenous," "structural," and "causal," yet no textbook author has written adequate definitions. The proper definitions are: a parameter is structural if it is invariant under any modification of the system selected from a specified family of modifications. A set of variables x is exogenous for y if the conditional distribution of y given x is invariant under any modification of the system selected from a specified family of modifications, in particular those that alter the path of x. An exogenous variable x is said to cause an endogenous variable y if the conditional distribution of y given a set of exogenous variables depends on x. An endogenous variable y(1) is said to cause another endogenous variable y(2) if the conditional distribution of y(2) depends on a surrogate exogenous variable for y(1).

The word "modification" occurs directly in the definitions for structural and exogenous and indirectly in the definition of causal through the reference to exogenous variables. It is the thread which I believe ought to form the fabric of discussions of causal inference. The Lucas critique of macroeconometric policy advice has had the effect of introducing the analogous word "intervention" into the macro literature. One word or the other ought to permeate all of econometrics.

The features of the sample that will be preserved after aspects of the system are modified cannot be revealed by the data set. Belief in exogeneity (the invariance of a conditional distribution) requires a high degree of conformity between the sampling context and the proposed modification. This conformity is most clear when an experiment is performed which in effect tests the modification. But when only nonexperimental data are available, causal inferences rely on a metaphor of experimental design. Doubt about the metaphor causes doubt about the effect of a modification. A defect of traditional econometrics is that it does not allow doubt about the experimentation metaphor. Some variables must be regarded as exogenous even though no randomization has occurred, and identifying restrictions must certainly be true even though they may in fact be as uncertain as other parameters in the system. A Bayesian approach together with a sensitivity analysis is the proper antidote.

I make one definitional innovation of my own. I will refer to surrogate exogenous variables instead of identifying restrictions or instrumental variables. For example: modifications that control endogenous variables are planned by selecting surrogate exogenous variables that are assumed to behave like the corresponding endogenous variable in the controlled period. In the traditional language, these surrogate exogenous variables are implied by the identifying restrictions.

The last three sections of this paper (sections 5, 6, and 7) contain comments on what I regard as defects in the autoregression program and basically amount to the statement that autoregressions may or may not be causal. A comment is made on the usefulness of tests of "Granger causality" that form an important part of the vector autoregression program. Again I have a suggestion regarding vocabulary. This concept should be called "precedence." What is tested under the heading of Granger causality is whether one variable regularly precedes another. We can all think of contexts in which precedence is suggestive of causation and also contexts in which it is not. In writing this section I could not resist the temptation to refer to Alice's conversation with Humpty Dumpty. The next section is a comment on the suggestion that tests of "Granger causation" are also tests of "exogeneity" and ought to precede every analysis of time series data. This is not the case if exogeneity is defined as above. Nor do I believe that there is a definition of exogeneity that is both useful and testable. It is interesting that this aspect of the vector autoregression program is the one most widely accepted, even by critics such as Gordon and King (1982) and Mishkin (1983). The final section is a critical comment on the causal implications of the variance decomposition that is another element of the vector autoression program. Unfortunately, the model is underidentified and any variance decomposition has a high degree of arbitrariness.

DEFINITIONS: STRUCTURAL PARAMETERS AND EXOGENEITY

One of the great benefits of the research on vector autoregressions is the increased attention given to three important and loosely-defined words: structural, exogenous, and causal. It is difficult in hindsight not to remark on the professional sloppiness that has been evident in the use of these terms. I do not mean to suggest that adequate and generally-accepted definitions have yet emerged, but the debate surrounding them has surely been beneficial.

The adjective "exogenous" seems to have at least three different meanings. The word is often used by economic theorists to refer to variables that are not explained by the theory. For example:

> The variables are classified into *endogenous variables* and *exogenous variables* according to whether the theory is or is not intended to account for their values. (Goldberger, 1964, p. 294)

Applied econometricians, however, use the term to refer to a set of explanatory variables in a least squares regression that produces consistent estimates of the *parameters of interest*. Though I feel that this is the most frequently-used exogeneity concept, I have been unable to find a proper explication in any econometric text. Usually, the theoretical definition is linked to the behavior of the stochastic terms in the equations:

> The statistical counterpart of the preceding (economic theory) argument is that exogenous variables are independent of the random terms of the system or minimally uncorrelated. (Dhrymes, 1970, p. 169)

The shortcoming of this definition is that it makes no explicit commitment to the choice of values for the parameters. Consequently, the random terms are not fully defined, and whether the condition is satisfied or not is impossible to verify. There is, however, a reference in this definition to economic theory which I interpret to be an implicit commitment to the choice of parameter values. Another interpretation of this definition is that there exists *some* choice of parameter values such that this independence condition holds. This interpretation implies the third definition of exogeneity:

> It is a maintained hypothesis underlying the Gauss-Markov theorem and the distribution theory ordinarily applied to generalized least squares estimators that the right-hand-side variables in a regression equation are strictly exogenous, meaning that the expected value of the vector of residuals, conditional on the whole array of right-hand-side variables, is zero. (Sims, 1977, p. 24)

Casual reading of Sims' definition and Dhrymes' definition would not suggest that they are different, but I interpret Dhrymes and most applied econometricians as making an implicit commitment to the choice of parameter values. In contrast, an atheoretical time series approach cannot involve a commitment to the choice of parameter values, and for this reason there is a sharp difference between the traditional definition and the one used by time series analysts. (Incidentally, the real content of Sims' definition occurs in a time series context in which the "whole array of right-hand-side variables" includes past, present, and future values of the variables in the model. More on this below.)

Casual reading of Goldberger's economic-theory definition and Dhrymes' statistical definition suggests that they are very different, but what the theorist is referring to is a thought experiment in which Dhrymes' exogeneity condition is satisfied. For example, it is proper to treat price as exogenous in a theoretical model of the supply side if the model focuses on the behavior of firms and excludes a discussion of the other side of the market. Of course, the theorist's thought experiment that takes price as exogenous may be quite unlike the setting in which a given data set is actually generated, and a data analyst may prefer to make a different assumption, depending on what parameter values are of interest. Incidentally, it is important to understand that the parameter values of interest are usually defined by the theorist's thought experiment in which the price is manipulated independently of the supply equation. These are the parameter values of interest because the data analyst is implicitly studying a modification of the system that would make the determination of price separate from the supply side.

The reader may object that I am reading too much into Dhrymes' definition of exogeneity, but I want to point out that in the traditional simultaneous equations setting, with no intertemporal correlation, the definition of exogeneity is quite meaningless unless there is a commitment to the choice of parameter values. Suppose, as in Cooley and Leroy (1982) and Geweke (1984), that two variables, y and x, come from a bivariate normal population. If you like, you may think of a regression equation with a normal error term and an explanatory variable that is also normally distributed. Then it is possible to write y as a linear function of x and a random variable that is independent of x. And it is also possible to write x as a linear function of y and a random variable that is independent of y. Do we mean to say that *both* y and x are "strictly exogenous"? Or consider Pratt and Schlaifer's (1984) simple model: $y = b^*x + Z$, where b is an uncertain parameter and y, x, and Z are variables, with Z both unobserved and *unnamed*. This same equation could have been written in terms of another unnamed and unobserved variable W: $y = (b+1)^*x + W$, where $W = Z - x$. If, conditional on x, the unnamed random variable Z has expected value equal to zero, it cannot also be the case that the unnamed random variable

W has expectation zero. So, is x "exogenous" or not? It clearly depends on the parameter value.

The closely-related concept of a "structural" equation is defined in an analogous and equally unsatisfactory way. Theil (1971, p. 431) writes "...the original equations are called structural equations because each of them serves to describe part of the structure of the economy." This is an altogether unsatisfactory definition since any equation can be said to satisfy the stated conditions. Pindyck and Rubinfeld (1981, p. 182) offer a more meaningful definition: "Such a model is called a structural model because the form is given from the underlying theory." But, again, what equation is not given by the underlying theory?

An adequate definition of the adjective "structural" has reemerged in response to concerns expressed by Lucas (1976), Sargent (1973), and Sargent and Wallace (1975) about the sensitivity of macroeconometric parameters to changes in policy rules. This has led Sims (1982) to resurrect from Koopmans and Bausch (1959) and from Hurwicz (1962) the definition[1]:

> Definition: A parameter is *structural* if it is invariant under any modification of the system selected from a specified family of modifications.

This definition can and should be extended to the related concept of exogeneity:

> Definition: If the observed conditional distribution of the variable y given a set of variables x is invariant under any modification of the system selected from a specified family of modifications that alter the process generating x, then the variables x are said to be *exogenous* to y.

I trust that it is understood that this exogeneity condition requires the conditional distribution of y given x to be unaffected by the modification, but it leaves open the prospect of altering the marginal distribution of y and the marginal distribution of x. The invariance of the conditional distribution is what allows the planning of modifications on the basis of features of the observed conditional distribution, for example, the re-

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[1]The word "structure" is avoided because it has been employed in too many different contexts to be useful here. The word "structural" on the other hand is used in economics almost exclusively to refer to one equation in a simultaneous system in a way that conforms with the definition presented.

gression of y on x.[2]

Though this definition cannot be found in econometric textbooks, it is the one adopted implicitly by both economic theorists and applied econometricians. Economic theorists define the supply curve in terms of the conditional distribution of quantity given price with price generated separately of the supply determining mechanism. In the thought experiment that defines the supply curve, the conditional distribution of quantity given price is conjectured to be the same regardless of the method by which price is generated. Applied econometricians, on the other hand, would not expect the conditional distribution of quantity given price to remain unaffected by modifications of the mechanism that determines demand, since they realize that unusual events affecting the supply curve in the past would have been partly offset by changes in price, and since they do not expect that this relationship will necessarily be preserved under modification of the demand generating mechanism.

Invariance of a conditional distribution to certain modifications of the system is the essential aspect of the first two exogeneity concepts, one referring to thought experiments and the other referring to actual data. For the sake of completeness, I digress briefly to offer an alternative definition of the third exogeneity concept:

Definition: A subset of variables, x(1), selected from a larger set of variables x = (x(1), x(2)) is said to be *sufficient* for y if the conditional distribution of y given x depends on x(1) but not on x(2).

This may seem initially quite unrelated to Sims' definition, but in fact it is only a slight generalization that involves the whole conditional distribution, not just the conditional means. To give a time series example that restricts attention to the conditional mean, let x(1) refer to a set of variables measured in the same period as y and let the "model" be written

[2]Though the term "intervention" is most common in the macroeconometric literature, I have adopted here the term "modification," since "intervention" suggests an intervening human and since I want to refer more broadly to any hypothetical changes in the system. An economist can predict the effect of an increase in supply on the price, whether the supply increase is a consequence of the selling-off of government stockpiles or a consequence of especially favorable weather. A climatologist predicts the effect of lowering the Rocky Mountains when conjectures are made about the climate in Denver if its elevation were less (Pratt and Schlaifer's 1984 example). Economic historians can discuss the evolution of the United States if the railroad had not been invented (Fogel, 1962), or even if Columbus had not discovered America (McAfee, 1983). All of these will be called modifications in this paper. Paul Romer is thanked for this terminological suggestion.

as $y = E(y|x(1)) + U$, where U is "the error term." Let the other variables $x(2)$ refer to past and future values of $x(1)$. Then the conditional mean of y given x, $E(y|x)$, is not a function of the lagged and future variables $x(2)$ (my definition) if and only if the conditional mean of U given x is zero (Sims' definition). This follows straightforwardly from $E(y|x) = E(E(y|x(1))|x) + E(U|x) = E(y|x(1)) + E(U|x)$, which implies $E(y|x) = E(y|x(1))$ if and only if $E(U|x) = 0$.

Sufficiency is a property of the process that generates the data set. Exogeneity on the other hand depends on the conformity of the given data set and a hypothetical data set generated after a modification of the system. Exogeneity involves a commitment to the choice of parameter values, but sufficiency does not. Suppose that y, x, and z come from a trivariate normal distribution. Then x is sufficient for y if it is possible to write y as a linear function of x plus an error term that is independent of x and z. The coefficients in this linear function are chosen to assure that this independence property holds for the process that generated the data. But x and z are exogenous only if this linear function applies after the modification.

The definition of exogeneity that I have given is close to what Engle, Hendry, and Richard (1983) call "super exogeneity." These authors refer to a variable x as "weakly exogenous" for estimating a parameter, β, if inference about β conditional on x involves no loss of information. If x is weakly exogenous for β and if β is invariant to all interventions affecting the marginal distribution of x, then x is said to be "super exogenous." My most serious objection to these definitions concerns the concept of weak exogeneity. Consider the model:

$$y = \alpha + \beta^{\star} x + U \qquad (1)$$

$$x = \gamma + \beta^{\star} w + V, \qquad (2)$$

where U and V are unobservables that are regarded to be independent random variables with zero means, and where α, β, and γ are uncertain parameters. Note especially that the parameter β is the same in both equations. An intervention that took control of V and thereby altered the marginal distribution of x would leave the conditional distribution of y given x undisturbed, and I would call x exogenous for y. But x is not weakly exogenous according to Engle, Hendry, and Richard since there is information about the structural parameter β in the distribution of x (given w). To put the objection directly, we confound two separate concepts if exogeneity is intertwined with efficient estimation. A second objection is that the definition of super exogeneity refers to all possible interventions that affect the path of x. It seems obvious that some interventions will always exist that alter the path of x and at the same time alter the conditional distribution of y given x. It is necessary therefore to restrict the nature of the interventions. This regrettably leaves the concept of exogeneity rather ambiguous, since the families of possible interventions are rarely explicitly stated.

The definition of structural as "invariant under modification" likewise must be applied with care, since whether a parameter is structural or not depends on the nature of the modification. For example, we sometimes think of tastes and technology as structural parameters, but an injection of a suitable chemical can alter tastes, and various government policies can alter the pace of technological progress. Likewise, the traditional distinction between the "structural" form and the "reduced" form of a simultaneous-equations system makes sense only if you are imagining a modification that alters one of the "structural" equations and leaves all the others unchanged. Such a modification alters the reduced-form parameters but leaves unchanged most of the "structural"-form parameters. But, as pointed out by Sims (1977), there are many hypothetical modifications that can be expected to leave the reduced-form parameters unchanged and these parameters can equally well be regarded as structural.

To put this another way, a linear simultaneous-equations system is a set of linear equations that have an infinity of alternative equivalent representations; which of these representations are structural depends rather subtly on the nature of the modification. Consider again a simple supply-and-demand system. The quantity supplied is assumed to be a linear function of the price. Likewise the quantity demanded. These two equations have a single intersection, usually assumed to be in the positive quadrant. The rhetorical question is: why select these two lines as the ones that define the equilibrium price-quantity couple when any of an infinity of other pairs of lines define the same point? The answer is that the economist can imagine events that would shift "the" supply curve but leave unchanged "the" demand curve. Then the economist can imagine the change in the price-quantity couple induced by this hypothetical shift in supply. Thus among the myriad of structural relations [pairs of linear equations], all of which have the same "reduced form" [price-quantity solution], the economist selects the one that he supposes is useful in the sense of providing answers to a question like "What is the effect of a modification that shifts "the" supply curve but leaves unchanged "the" demand curve?" The conclusion toward which I am leading is that the separate concepts of supply and demand have meaning only in the context of hypothetical modifications.

2. SINGLE EQUATION STRUCTURAL INFERENCE

Is there any reason to suppose that one can predict the effect of a modification from historical data? What makes one think that a parameter is structural, that variables are exogenous? The answers must depend on what is believed to be the degree of conformity between the modification and the sampling process. An experiment, when properly conducted, will by design conform with the modification. But if the sample does not amount to a trial modification, the conformity between the sample and the modification will be in doubt. For this reason, when only nonexperimental data are available, the consequences of modifications must remain speculative.

To illustrate the need for conformity between the sampling process and the modification, suppose that there are two observable variables, y and x, and one unobservable, Z, which necessarily obey the "law"

$$y - \alpha - \beta^{*}x - Z = 0. \tag{3}$$

It appears initially that there are three types of modifications that might influence y:

(M1) After Nature selects Z, a random variable with mean zero, the Modifier selects x. Then Nature selects y to conform with equation (3).

(M2) The Modifier first selects x; then Nature selects $Z = Rx+W$, where R is a fixed parameter and W is an unobservable random variable with mean zero. From these, y is selected in conformance with (3).

(M3) Nature first selects y. Then the Modifier selects x. Based on these, Z is selected to conform with (3).

Note, however, that the modification (M2) in which Nature chooses Z after the intervener chooses x could also be described as a modification of type (M1) with the law written as

$$y - \alpha - (\beta+R)x - W = 0, \tag{4}$$

and with Nature choosing W before the intervener chooses x. If Z, and by implication W, is a potentially observable variable, communication is facilitated if the "law" is always written in terms of one of the variables, say Z, and the distinction between modifications of type (M1) and (M2) is maintained. If, on the other hand, Z refers to a collection of unnamed variables, then it is conceptually clearer to write the "law" as equation (4) with the parameter β+R selected so that W is distributed independently of x. The parameter value, β+W, to which one is thus committed for defining exogeneity is the value that applies in the period of modification. For simplicity, I will proceed as if Z were a named variable, and consequently the distinction between (M1) and (M2) is useful.

Corresponding to these three modifications are three sampling processes:

(S1) After Nature selects Z, the Experimenter selects x and observes y.

(S2) First the Experimenter selects x. Then Nature selects Z = Rx+W, where R is a fixed parameter and W is an unobservable random variable with mean zero. Then the Experimenter observes y satisfying equation (3).

(S3) After Nature has selected y, the Experimenter selects x and observes y.

In order to decide if the data set in front of you tells you anything about the effect of a modification, you must identify the nature of the modification and the nature of the sampling process. Your data set will unambiguously reveal the effect of your proposed modification only if there is a correspondence between the form of modification you imagine and the form of the sampling process. If the sampling process and the modification do not conform, there is no special reason to suppose that the observed relationships in the data will continue to hold after the modification occurs.

Suppose that you are expecting to undertake a modification of type (M1) but have a sample of type (S2). There is then no reason to suppose that the correlation in the sample tells you anything about the effect of your modification, since the apparent influence of x in the sample may have been entirely due to its influence on Z.

A classic example of this is the placebo effect of a drug. Suppose that y represents health, x indicates the administration of a drug, and Z indicates mental condition. In one kind of experiment, patients are randomly sorted into two groups. Each patient in the first group is given a drug and each patient in the second group is given the traditional treatment for the illness. If it is observed that patients who receive the drug have improved health, it is quite proper to infer that other similar administrations of the drug would likewise improve patient health. But it is possible that the administration of the drug causes the patient to expect to feel better, which by itself would improve the patient's condition. What this means in terms of the formal model is that the doctor is choosing the drug dosage x, and Nature is selecting mental condition Z in response. Another form of experiment forces Nature to select Z first, or more accurately, independent of x. Namely, patients are randomly sorted into two groups. Patients in the first group are given a drug. Patients in the second group are given a placebo. In a "double blind" experiment even the administering doctors do not know whether it is the real drug or a placebo. There is no special reason why these two kinds of sampling experiments, S1 and S2, would lead you to the same conclusion regarding the effectiveness of the drug. There are actual examples in which the results are drastically different.

When the sample and the modification do not conform, you do not have the incontrovertible right to assume that the sample and the modification produce similar effects. But you may be able to convince yourself that it does not really matter. Possibly, when Nature chooses Z after x, She does so without regard to x. In the notation above She sets R equal to zero. For example, if after numerous trials with different drugs, the medical profession finds no examples of the placebo effect, it seems proper not to expect a placebo effect in an experiment with a new drug. It is then appropriate to conclude that it does not matter whether Nature chooses mental state (Z) first or second, since either Z does not in fact enter the equation or Nature selects Z without regard to x (the drug administration).

The conformity between sample and modification is most credible when a randomized experiment is performed in which the modification is directly tested. But when only nonexperimental data are available, a metaphor of randomization is used in place of actual experimental design. Though the econometric literature is replete with procedures to deal with inadequacies in the design, such as simultaneity and selectivity bias, all of these methods ultimately make use of the randomization metaphor and therefore do not require new concepts of statistical inference. For example, phrases such as "sampling distributions" that have a relatively clear meaning when experiments can be repeated are used also in contexts such as the analysis of macroeconomic data in which the notion of repeating an experiment stretches the imagination. In such nonexperimental contests it is probably better to reject as inappropriate the frequency interpretation of probability and to adopt instead the personal or Bayesian viewpoint in which the metaphorical nature of probabilities is made more or less explicit.

If the data were not generated in an experimental setting, the sample and a proposed modification can be argued to conform either by appeal to theory or by reference to analogous modifications. Otherwise it is entirely a matter of speculation whether or not features of the sample will be retained after the modification occurs. Usually, it is easy to generate reasons why a sample and a modification will not conform.

Consider again the drug example, but suppose now that the drug is self-administered. If it is observed that patients who self-administer a particular drug (or vitamin) have improved health, is this evidence that the medical profession should prescribe this drug? One reason why not is that there is a placebo effect. Namely, patients who self-administer the drug have such a high degree of faith in the drug that they improve their mental state and thereby improve their health. This is a type of placebo effect that may not be producible by the medical profession, since what is important is whether the patient thinks he is going to improve which is revealed by self-administration of the drug. Thus in this natural experiment, x is chosen in response to Z. The medical profession's modification to control x need not in turn influence Z. This model can be written as

$$\begin{aligned} y &= \beta^\star Z \\ x &= \gamma^\star Z + \sigma^\star w \quad , \\ w &= \theta^\star Z \end{aligned} \tag{5}$$

where y is health, Z is mental condition, x is the administration of the drug, and w is a factor other than Z that contributes to the decision to administer the drug, for example, the doctor's advice. Incidentally, I will use the convention here and elsewhere that variables on the right-hand side of an equals sign are selected before variables on the left, where the word "before" can refer to timing or to information. The system (5) thus defines completely the causal chain, beginning with health Z.

If model (5) applies, Nature chooses Z, the mental condition, and both health y and drugs x follow as a consequence. But the observed correlation between the drug and health does not mean that a modification that altered the drug will have any affect at all on health.

Yet another interpretation of the observed relationship between y and x is that x is chosen in response to y, and the modification of controlling x may have no effect on y. For example, people who are especially healthy may self-administer certain drugs (vitamins) because they think the vitamins contribute to health, while the sickly, having experimented with vitamins without effect, choose not to take them. This model is

$$x = \beta^* y + \gamma^* w \quad , \qquad (6)$$
$$w = \sigma^* y$$

where the doctor's advice w can influence the drug intake x but cannot affect health y.

Statistical arguments based on nonexperimental data rest implicitly on the assumption, or perhaps the hope, that, in the absence of specific evidence to the contrary, samples and modifications necessarily conform. It seems clear from the foregoing discussion that this involves quite a leap of faith. What can be said about the nonexperimental scientist's belief in the conformity of samples and modifications is that, remarkably enough, these logically unfounded inferences are valid with a frequency high enough to encourage the continued reliance on this naive belief. Correlation does not imply causation, but you are better off acting as if it did than in ignoring the nonexperimental data altogether. As a matter of fact, the vast majority of the inferences we make are based on nonexperimental data. A few of these are checked with randomized experiments or trial modifications, but many of our decisions are vitally dependent on evidence from nonexperimental data sets. The existence of experiments and trial modifications is itself evidence of the reliance on nonexperimental data, since often an experiment would not have been performed if there were not adequately convincing evidence that the apparent effect in the nonexperiment could be reproduced in the experiment. Nonetheless there has to be an extra bit of uncertainty attached to modifications planned with only nonexperimental data.

The extra element of uncertainty attaching to causal inferences from nonexperimental data is reduced by appeal to credible theory and by reference to analogous nonexperimental settings that previously had led to successful modifications. In medicine, there have been numerous instances of nonexperimental correlations between health and exposures that have been shown to be causal. This does not prove that the nonexperimental association between coffee and heart disease is causal, but it does give the association more credibility than if there were no analogous settings in which correlations were causal. This finding would be further strengthened by physiological theory that links caffeine with certain heart responses that are suspected to weaken the heart muscles over the long run.

In economics, it is virtually impossible to find analogous settings in which there were successful modifications, since there are so few explicit modifications. Though the body of theory is enormous, there is a surprisingly small core of propositions confidently maintained by a substantial majority of the profession. Credible causal inferences in economics will therefore generally rely on the identification of variables that can be convincingly argued to be exogenous because of the way they were generated. Shiller, in his comments on Sims (1977), refers to Milton Friedman's work on the causal relationship between money and income, which makes use of several exogenous surrogates for money including wars, foreign events, and changes in the Federal Reserve system. These events are thought to precipitate a change in money equivalent to a direct modification and are called by Friedman "quasi-experiments." If the Cowles program is ever to be regarded as successful in macroeconomics, it will surely rely on exogenous variables such as these.

3. THE COWLES PROGRAM FOR DRAWING CAUSAL INFERENCES IN A NONEXPERIMENTAL SETTING

By appeal to the metaphor of experimental design it is possible to use nonexperimental data to plan modifications that alter the path of the exogenous variables. But it became clear early in the history of econometrics that many variables which were of interest because of possible modifications could not also be regarded as exogenous, and the technique of multiple regression which conditioned on these variables could not be credibly employed to predict the effect of the intended modifications on other variables in the system. It is the remarkable accomplishment of the econometric pioneers that they were able to devise procedures that would allow the planning of these modifications that were explicitly acknowledged not to have been tested in the sample. This work culminated at the Cowles Commission and produced a set of procedures that I will call here the Cowles program for causal inference. This program presumes the existence of a set of exogenous variables but, in addition, the program requires the selection of one or more surrogate exogenous variables for each of the endogenous variables. Modifications that directly alter the path of one of the endogenous variables are then planned as if an equivalent modification were to alter the path of the corresponding exogenous surrogate. In the extreme, the Cowles program challenges us always to offer a coherent account of the observed variability of each modifiable variable including a component of variability that is uniquely associated with an exogenous variable which can serve as a surrogate for the proposed modification.

An example is an attempt to determine the effect of price controls on the sales of a commodity by a regression of a time series of sales on price. Suppose that the demand function is:

$$q = \alpha + \beta^{*}p + U \tag{7}$$

where α and β are uncertain fixed parameters, q is the quantity sold, p is the price, and U is an unobservable, unnamed variable. The price p is an exogenous variable if the observed covariability of price and quantity is similar to the covariability that would be induced by price controls. This is not likely, most economists would argue, since in the period before the price control a shift upward of the demand schedule (a positive value of U) would induce a compensating rise in the price of the commodity to ration the available supply. No such covariability between p and U can be expected in the modification period, and consequently the conditional mean of q given p can be expected to change with the modification. In place of the unacceptable metaphor that price is a randomized treatment, the Cowles program would substitute another metaphor. A secondary model is hypothesized that separates the variability of the "treatment" p into a part that is nonrandom and a part that is observable but like a random variable:

$$p = \gamma + \sigma^{*}x + V. \tag{8}$$

The two components of variability of p are: (1) V: an unobservable that is correlated with the unobservable in the primary equation U. (2) x: an observable that is uncorrelated with the unobservable U in the primary equation.[3] The second of these two sources of variability mimics the modification of price controls, since it is by assumption uncorrelated with U. If (8) is inserted into (7) we obtain:

$$q = \alpha + \beta^{*}[\gamma + \sigma^{*}x + V] + U. \tag{9}$$

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[3] If you are worried that q does not appear in this equation, the answer is that this is the reduced-form equation for p.

Thus x has the direct effect σ on p and the indirect effect $\beta^*\sigma$ on q. By observing how x influences price and subsequently quantity, we can therefore infer the effect of direct control of price, β.

In summary, the Cowles program has two steps. First, one must identify a set of exogenous variables. By assumption the conditional distribution of the endogenous variables given the exogenous variables is invariant to modifications that affect the path of the exogenous variables. Secondly, to determine the effect of controlling an endogenous variable, one selects a surrogate exogenous variable (or a set of surrogate exogenous variables). By assumption, the control of these surrogate exogenous variables is equivalent to the control of the endogenous variable. In the more traditional language, this second step is the selection of identifying restrictions.

Although the subject of inference in the context of a simultaneous equations model is more complex mathematically than single-equation econometrics, it relies implicitly on the experimental metaphor for the selection of exogenous variables and for the choice of surrogates, and therefore does not require new concepts of statistical inference. What is unique about the nonexperimental setting generally is that the metaphor of experimental design is usually a subject of intense debate, both personally and publicly. The form of this debate is what I (1978) have called a "specification search" in which many alternative statistical models are used as bases for drawing inferences from the same data set and one or more of the results are selected for reporting purposes. To me, the defect of the Cowles program is that it presupposes a complete commitment to the choice of exogenous variables and to the selection of surrogates when at best only partial commitment is possible. It consequently fails to provide tools that can be used to define and control the ambiguity in the inferences that are a consequence of the doubt about the experimental metaphor. It is this shortcoming that leads Sims (1980a) to complain about the "incredible" identifying restrictions in macroeconometric models and to advocate the vector autoregression program in place of the Cowles program.

The following example will illustrate the problems with the Cowles program. Using a sample of fires occurring in a large city in one year, a statistician discovered that the more firemen sent to the scene of fires, the worse was the "resulting" damage. His recommendation that the city's fire department be abandoned was fortunately not taken seriously. The inference that firemen cause damage would have been appropriate if the allocation of firemen to fires had been random, as it would be in a designed experiment. But because no formal randomization occurred, and because the finding that firemen cause damage conflicts sharply with one's prior beliefs, most of us would interpret the positive correlation between firemen and damage as evidence that a specific nonrandom rule was used to allocate firemen to fires: more firemen were sent to the relatively severe blazes. We are thus led to reject the metaphor that firemen were randomly assigned to fires.

The first metaphor having been rejected as inappropriate, the nonexperimental scientist seeks another. Possibly the next step would refer to a model such as:

$$d = P + \beta n \tag{10}$$

$$n = \gamma P + \sigma z \ ,$$

where d = property damage, n = number of firemen, P = the unobservable potential property damage, z = the firefighting capacity of the district, and β, γ and σ are uncertain parameters. The first equation implies that the actual damage is equal to the potential damage offset by a function of the number of firemen. The second equation describes the number of firemen who are dispatched to fight a fire as a function of the potential severity of the fire and the firefighting capacity of the city. If the econometrician had a perfect measure of the potential P, then he ought to be controlling for P when he regresses d on n. Without that control, the regression of d on n involves a misspecification error, and he may find d and n to be positively correlated because, historically, the largest numbers of firefighters are sent to fight the worst fires.

The more direct and more common way to deal with the correlation between P and n is to find measures of the covariate P, and to form an estimate of β by a multiple regression of d on n and measures of P. If the covariate P is measured with sufficient accuracy, the metaphor that n is a randomized "treatment" may be apt for most people. Possibly they would regard the only remaining source of randomness to be measurement errors in d. But because there are likely to be various ways that potential damage can be sensibly measured or proxied, and because the estimate of β may change greatly depending on how P is measured, the inference about β is likely to be ambiguous. Consequently, the nonexperimental scientist requires tools for identifying, controlling, and communicating that ambiguity.

The other approach for dealing with the correlation between P and n is to treat P as an unobservable and to use firefighting capacity z as a surrogate for the modification that controls n. This is what I refer to as the Cowles program. As will be seen, this, too, leads to serious ambiguity in the inferences and requires the same kinds of tools which presently do not exist. The Cowles data analysis begins with estimates of the reduced form of the system in which the dependent variable d and the endogenous "treatment" n are solved as functions of the exogenous variables:

$$d = \beta\sigma z + (1+\beta\gamma)P \tag{11}$$

$$n = \sigma z + \gamma P.$$

In this form it seems clear that a regression of d on z yields an estimate of $\beta\sigma$, a regression of n on z yields an estimate of σ, and their ratio allows us to recover β, the effect of an additional firefighter on the damage. But do we really get estimates of $\beta\sigma$ and σ from these regressions? That depends on whether z and P are uncorrelated. To put this differently: Can we comfortably act as if z were selected without regard for P and therefore would serve as a surrogate for the modification? It is easy to think of reasons why not. The firefighting capacity variable z refers to specific districts within the city. It seems sensible to expect that districts which are subject to the greatest risk will have the largest firefighting capacity. Thus in thinking more about this problem, we come to doubt that z is a suitably exogenous variable after all, but a new candidate has popped up: the property value of the city. So, on we go in our quest for the mythical Exogenous variable.

One can imagine the consternation of the city Planning Department that hired a sequence of econometricians to review the work of their predecessors. The first one said firefighters cause damage. The second said the first had erred in assuming that the number of firefighters is exogenous and used the firefighting capacity as an instrument. The third claimed the second erred in assuming that capacity is exogenous and used property value as an instrument. The fourth was worried because property owners try to locate near fire houses and argued that property value is not exogenous. He used a dummy variable for a strike of firefighters. The fifth said he had reason to believe that the firefighters struck districts where the potential damage was greatest, and he suggested....

We can also imagine the consternation of the Federal Reserve Board which hired a sequence of econometricians to advise on monetary policy.

The problem with the Cowles program is that it requires a complete commitment to the selection of both exogenous variables and identifying restrictions, when economists are willing only to make partial commitments. In fact, T.C. Liu (1960) nags us that no variable in a macroeconomic setting is properly regarded as exogenous. It is the "incredible" selection of instruments that induces Sims (1980a) to argue for another research program built around vector autoregressions. He takes the identifying restrictions to be simplifications rather than beliefs. My initial instincts would have been to treat them as partial beliefs, that is, to assume the existence of prior distributions for these parameters, concentrated in the neighborhood of zero. What seems at fault in the Cowles program is the assumption of sharp identifying restrictions, when at best only probabilistic ones are available. It is interesting that Liu's (1960) criticism, if read carefully, amounts only to the statement that data analysts do not use whatever prior information they may have in a proper manner. For example:

> The criterion used by Klein and Goldberger for the selection or rejection of explanatory variables throws a great deal of light on the principles that have guided them in specifying the structure. These principles are, in fact, responsible for the overidentification of the Klein-Goldberger structural relationships. Essentially, a relevant explanatory variable is excluded if inclusion would result in a significantly unreasonable magnitude for its own coefficient or for other structural coefficients.... The result so obtained, however, is anything but an estimate of the structure (Liu, 1960, p. 858).

This quotation cries out for a Bayesian response. The procedure Liu criticizes is what I (1978) have called an interpretive search which is intended to pool the data information with other relevant but ill-defined information. A Bayesian analysis is a formally proper method of pooling. If Klein and Goldberger are prepared to commit themselves to probabilistic statements about what are unreasonable magnitudes for the parameters, exact identification is not achieved but a form of partial identification is. In that event, the prior distribution and the posterior distribution will be different, and there is useful information in the data set even though the model is underidentified and cannot be consistently estimated.

The problem with the Bayesian solution to this dilemma is that no particular prior distribution is credible. This is properly treated with a sensitivity analysis that identifies which inferences are adequately insensitive to the choice of prior and which are not. Sensitivity analyses for single-equation problems are discussed in Leamer and Leonard (1983) and Leamer (1983). Unfortunately, the Bayesian analysis of simultaneous equations is complicated enough given a single prior. It does not seem likely that a useful sensitivity analysis for simultaneous equations estimation is in the immediate future.

4. CAUSAL INTERPRETATIONS OF ANTICIPATED ACTIONS

Many would argue that the decisive monkey wrench was thrown into the macroeconometric contraption by Lucas (1972), who introduced a special kind of placebo effect into macroeconomic modelling.[4] The organism under study is assumed to form expectations of future values of the stimulus and to make anticipatory responses. The ability to anticipate and to make creative responses is what distinguishes biological from physical systems and is what makes biological systems much more difficult to understand. In order to make my comments on this as clear as possible, I shall refer to a specific model. Macroeconomic models with particular market-clearing assumptions stimulate unwanted emotional and intellectual responses, and I therefore will base my comments on a botanical model.

I ask you to suppose that, roughly speaking, plants grow in such a way that the root structure makes up approximately a constant proportion of the mass of the plant. In dry climates both the foliage and the root structure are small; in wet climates they are both large. I ask you also to suppose that when water is unusually abundant, the plant allocates more of its growing energy to its roots in an effort to accumulate and store water against the possibility of a shortfall in the future. Conversely, if the season is unusually dry, the plant concentrates its growing energy on foliage to collect the surprisingly abundant sunlight. A model of this form is written formally as

$$y = \alpha + \beta\, E(x) + \gamma(x - E(x)), \tag{12}$$

[4]See Shiller (1978) and McCallum (1982) for insightful reviews.

where y is the proportion of the plant that the roots comprise, x is rainfall, and E(x) is the normal or "expected value" of rainfall, a measure of the climate. The sharp neutrality hypothesis that y is the same in all climates is $\beta = 0$. A value of γ in excess of zero implies relatively rapid root growth in unusually wet periods.

There are two questions that arise in the context of such a model. What kinds of modifications are possible? How should observed data be employed to direct these modifications? These questions are made difficult by the fact that modifications which affect x are likely also to affect E(x). To continue the plant example, it can be expected that in the short run an irrigation system is regarded by the plant as inducing a discrepancy between x and E(x), and the construction of an irrigation system can be expected to induce root growth by the amount γ times the increased water due to irrigation. Over time, however, an evolutionary process will allow the plant to form revised expectations and not to waste its energy on root growth since the supply of water is plentiful. In the very long run, when expectations adapt, the root mass as a proportion of the total plant may be unaffected by the irrigation system. A cactus becomes a palm. Formally speaking, this is the Neutrality Hypothesis $\beta = 0$.

Except in the highly unlikely situation when evolutionary forces work with such rapidity that expectations adapt instantaneously to changes in regimes, successful modifications will be possible in the short run. Though this seems obvious, it is not at all obvious how data collected in the context of one "regime" can be used to predict the effect of a modification which involves a regime shift. Instinctively, we might jump to the conclusion that the only way to answer this question of how the organism behaves in the transition from one state of settled expectations to another is to study actual transitions. Without such experience, there simply is no relevant evidence. There is a certain truth to this statement, but it ignores the fact that even in the context of a fixed environment there are sequences of observations (droughts, for example) that are suggestive of regime changes and that ought to generate transitional behavior by the plant. If the plant is unable or unwilling to respond to these observations in a way that is suggestive of a change in expectations E(x), and if the plant is able to observe only the water at its roots and the sunshine on its leaves, then it is proper to infer that the plant's expectations will not adjust, at least over the length of time of the unusual sequences of observations. To put this more directly, the effect of a new irrigation system can be predicted by studying the past behavior of the plant during sequences of unusually rainy seasons.

A formal statistical model that allows transitional behavior appends to (12) a model of transitional expectations. For example, it may be assumed that x(t) is normally distributed with mean M(t) and variance s, and it may be further assumed that the mean M(t) is equal to the previous mean M(t-1) with probability p; but with small probability 1-p a regime change occurs and M(t) is drawn from a normal population with mean M(t-1) and variance w. If you are sufficiently skilled at algebra, you can use these assumptions to compute E(x) as a function of the sequence of previous x values, which can be inserted into (12) to express y as a complex function of previous sequences of x values. The result is a complete causal model for y that describes how root growth responds to water supply, whether the water comes from the rain or from irrigation ditches. It is a model that can be estimated by an adequately skilled econometrician.[5]

This seems to me to conform to Sims' position stated in his Brookings paper (p. 117) that "Not only are reduced forms possible, they are essential" and (p. 120) "Of course, a fully accurate model would be quite nonlinear." The bridge between such statements and linear vector autoregressions seems shaky to me. Sims is more hopeful that linearity might be sensible:

> One might hope, though, that it could be well approximated by a linear model with unknown stochastically varying coefficients. Since there is no particular reason to suppose the economy is well characterized by a linear model with fixed coefficients even in the absence of complicated probability mechanisms for policy formation, it is not clear that the problem of estimating the response of the economy to policy is fundamentally more difficult in the presence of such persistent oscillation in the policy mechanism than in its absence. (Sims, 1982, p. 120)

I tend to disagree with this for several reasons. First, the statement encourages disregard for what seems to be an extremely important point that even if the model were approximately linear during all of recorded history, the nonlinearities that are present can be expected to play

[5]See Townsend (1983), Taylor (1975), and Decanio (1979) for transitional models in which agents learn the parameters of a fixed-policy regime. See Flood and Garber (1980b, 1983) for models with transitions between regimes.

an important role when there is a modification even modestly unlike the recorded history. If there are unusual periods in recorded history similar to the proposed modification, the prediction of the effect of the modification should concentrate on periods that mimic the modification. If you do use a linear model you simply will not be alert to such episodes, and your policy prescriptions should be heavily discounted if the proposed modification deviates in any significant way from the historical average behavior. To express this another way, the adequacy of a linear approximation for a nonlinear model is an empirical issue that is inappropriately and altogether ignored if linearity is simply imposed.

The second reason for my disagreement with this justification of vector autoregressions is that it deflects macroeconometrics from what I regard to be one of its essential functions, namely, drawing inferences about expectation formation. This is not limited simply to issues of nonlinearities, since there may well be other sources of information that will affect expectations. A plant endowed by nature with keen perception would notice the irrigation ditches and would suspect that a regime change has taken place. If the investment in the form of irrigation ditches were regarded as a credible commitment to a regime change, the plant would respond much more rapidly than if the only evidence of a regime change came through the water amounts at its roots. Likewise, shifts of power in the central government, international agreements on exchange-rate regimes, etc., may suggest changes in the policy regime and bring about more rapid adjustments in expectation formation than would be justified by the time series alone. It is this point that justifies the comment above that there is a certain logical validity to the proposition that it is impossible to predict the effect of an irrigation system if it has never been tried before, since who knows how a smart plant will respond to the construction it observes around it. Thus the complicated nonlinear model of the sort alluded to above seems at a logical level just as deficient as a linear model, since both presuppose one special and unlikely form of expectation formation.

It seems quite clear that if we are to make sense of the historical record and draw useful inferences about the effect of future modifications, we simply have to study expectation formation seriously. The rational expectations revolution has been widely praised for reminding us of this, though it has also been generally criticized for substituting one unlikely form of expectation formation for another. Before Lucas, macroeconometrics had treated x as a serially uncorrelated random variable. Now, if the

Lucas prescription is followed, it is as though x were a serially correlated random variable with a structure known to all participants in the economy except the econometrician. See, for example, the theoretical econometrics of Wallis (1980) and Hansen and Sargent (1980) and the applications of Barro (1977) and Mishkin (1982).[6] Frankly, if I had to choose between these two unlikely models of expectation formation, I would have difficulty and might well opt for the old-style macroeconometrics.

What we really need is a serious study of expectation formation that adequately deals with transitional behavior. If this were done in the traditional econometric manner, we would write down various models of expectation formation and test to see which is most favored by the data. In effect what this requires is the formulation of a very general and highly complex nonlinear model.[7] But estimation of subtle details of a complex nonlinear model is a heavy burden for most macro time series to carry, since the amount of noise in the system disguises whatever nonlinearities may be there. For this reason, real progress will probably require either (1) the study of historical episodes when it seems clear for exceptional reasons that expectations changed in a known direction as in Flood and Garber's (1980b) analysis of the German hyperinflation, or (2) an experimental approach as in Plott and Sunder (1980), or (3) the direct measurement of expectations. Of these three, I lean toward efforts to measure expectations directly.

The Keynesian revolution served as the central intellectual force behind the large expenditures that the government now makes to collect data on various macroeconomic time series. The benefits from this data collection in terms of improved macro economic control may be argued to be either great or small but are certainly diminished by the lack of information about expectations. It would be a substantial triumph of the rational expectations revolution if the government were to divert a significant amount of money to the measurement and study of macroeconomic expectations. Such an effort could tap the large literature coming from the border between psychology and decision theory about the measurement of expectations (e.g., Winkler (1967)) and about actual decision-making under

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[6]Exceptions are Grossman (1975) and the references in footnote 5.

[7]More recent work by Doan, Litterman, and Sims (1983) allows for stochastic drift in the parameters which is a model with formal similarities to the one just described. Whether it is adequate to approximate transitional expectations is speculative.

uncertainty (e.g., Tversky and Kahneman (1974)). This study should not be limited to expectations about a single period in the future, nor should it involve only means of distributions. My guess is that prospects for learning are an important source of business fluctuations, since agents will postpone investment commitments when the future receipt of important information is anticipated. In order to study learning prospects, it is necessary to elicit joint probabalistic judgments about at least two future periods.

This suggestion may evoke a cartoon image of a squirrel busily gathering nuts, trailed by a persistent and pestering Leamer asking if winter weather is expected. Squirrels, of course, rarely have the time for interviews and, when they do submit, their responses are quite irrelevant either because they have no incentive to reveal the truth or, perhaps more importantly, they do not even understand the urges that compel them to store away the nuts. Likely as not, they merely reply "Because I enjoy it." The profits-versus-sales debate has left the profession with the opinion that people are like squirrels, whose expectations might be inferred from the size of the pile of nuts that are stored but certainly not from interviews. I am convinced that humans are a bit more introspective than squirrels, and I think that there is at least a possibility that a few people will be able and willing to reveal their expectations in a meaningful way, especially if expectations about the behavior of the central government play a decisive role in the economy. In any case, this strikes me as the only way to save econometrics from the attack of the Lucas critique other than an outright dismissal of the theoretical logic.

Perhaps an example will make my point most clearly. Imagine an econometrician who is employed to plan an investment tax credit using data from a previous period. If the previous credit had been expected to be temporary, then investors would have hurried their decisions and an immediate response would have been observed. On the other hand, if the credit had been expected to be permanent, the response of investors would have been relatively slow. An econometrician with no direct data on expectations is unable to determine if the observed data represent a small but rapid response or a large but slow response. It is inconceivable to me that he would not be better off if he had interview evidence on the expected permanence of the credit. After all, more data are surely better than less.

5. GRANGER CAUSALITY

It should be clear from the preceding three sections that causal inference from nonexperimental data is a decidely subtle business, particularly in contexts in which human psychology is involved. Certainly these are not matters that are wisely turned over to computers or to their human extensions, statisticians, since a causal inference is not a simple matter of the size of statistical correlations. Nonetheless, the interpretation of statistical correlations can be such a mind-boggling activity that we all yearn for a computer program that will do it for us. Granger (1969) and Sims (1972) seem to have provided one.

Surely the most seductive feature of the vector autoregressive program is the test for the direction of causation. It is not at all surprising that these tests have a large audience in the macroeconomic community, since debate about the meaning of statistical tests has often floundered on issues concerning the direction of causation, and the Cowles program forces the researcher either to impose his own a priori notion of causation or to impose enough other restrictions in a simultaneous-equation setting so that the direction of causation (the path in the words of Sewall Wright, 1964) could be inferred from observed correlations. Though the Cowles simultaneous-equation approach is easy enough at a mathematical level, it is distinctly uncomfortable in application since it requires what seem like entirely incredible assumptions. This is Sims' (1980a) main argument for "atheoretical" macroeconometrics, a centerpiece of which is a test for "causation."

But what has become known as Granger causality has the deficiency suggested by its name - it is a specialized concept that is only incidentally related to causality as most of us use the term. The literature contains several suggestions of alternative words that might more accurately describe the concept (temporally interrelated, temporally prior, etc.). I like to use the word "precedence." What is tested under the rubric of "Granger causality tests" is whether, after accounting for various other influences, one variable regularly precedes another. It is altogether clear that precedence is not sufficient for causality. Weather forecasts regularly precede the weather, but few of us take this as evidence that the forecasts "cause" the weather.

I strongly object to the use of the words "Granger causality" when "precedence" or its equivalent more accurately communicates the concept. It seems to me that it is distinctly inappropriate to take control of the

English language for one's own purposes. Granger and Newbold only partially agree:

> Possibly *cause* is too strong a term, or one too emotionally laden, to be used. A better term might be *temporally related*, but since cause is such a simple term we shall continue to use it. (Granger and Newbold, 1977, p. 225)

Granger later abandons caution altogether:

> Provided I define what I personally mean by causation, I can use the term. I could, if I so wish, replace the word cause throughout my lecture by some other words, such as 'oshkosh' or 'snerd', but what would be gained? It is like saying that whenever I use x, you would prefer me to use z. (Granger, 1980)

Does this remind you of Alice's conversation with Humpty Dumpty?: (Lewis Carroll, 1872)

> "... There's glory for you!"
>
> "I don't know what you mean by 'glory,'" Alice said.
>
> Humpty Dumpty smiled contemptuously. "Of course you don't - till I tell you. I meant 'there's a nice knock-down argument for you!'"
>
> "But 'glory' doesn't mean 'a nice knock-down argument,'" Alice objected.
>
> "When *I* use a word," Humpty Dumpty said, in a rather scornful tone, "it means just what I choose it to mean - neither more nor less."
>
> "The question is," said Alice, "whether you *can* make words mean so many different things."
>
> "The question is," said Humpty Dumpty, "which is to be master - that's all."
>
> Alice was too much puzzled to say anything...

I, like Alice, find myself puzzled. I detect a certain lack of concern for the human capital that we have invested in our language. If I were to continue in that tradition I would propose that we henceforth refer to this notion of precedence by the wordpair: fool's causation. This substitutes a loaded word "fool" for the neutral "Granger" just as "causation" has replaced the neutral "precedence." Moreover, "fool" is decidely simpler than "Granger" - it contains only four letters, one of which is repeated - and, like "cause," it is rather difficult to define precisely. One man's fool is another man's genius. My definition of a "fool" would be a friend of mine living in San Diego.

There is a certain Alice-in-Wonderland character to the discussion of Granger causation that extends beyond the usurpation of language. It is indeed a frightful sight to observe economists tiptoeing into the edges of the quagmire of philosophy. Nonetheless, since a test of precedence is a cornerstone of the vector autoregressive program, I now feel compelled to get my own feet dewy, but no more. I will make comments on three alternative definitions of a causal relationship:

(1) A variable x is said to cause y if an optimal conditional prediction of y depends on both the history of x and the history of z, where z refers to a list of other variables. (Granger, 1969)

(2) A variable x is said to cause y if the relationship is predictable according to a law (Feigl, 1953), introduced into the debate by Zellner, (1978).

(3) An exogenous variable x is said to cause an endogenous variable y, if the conditional distribution of y given a set of exogenous variables depends on x. An endogenous variable, y_1, is said to cause another endogenous variable y_2 if the conditional distribution of y_2 given a set of exogenous variables depends on a surrogate exogenous variable for y_1. The surrogate may be either real or imagined.

The defect of Granger's definition 1 is that it risks leading us to the conclusion that weathermen cause the weather. Generally speaking we have managed to escape the primitive belief that precedence and causation are identical, but the empirical work that has been sparked by Granger and Sims seems to rest precipitously on the edge of the *post hoc, ego propter hoc* fallacy. Not so for the weatherman example, it may be replied, since the set of control variables z ought to include all the data that the weatherman uses. Given all that information, the weather forecast x becomes unnecessary for the prediction of the weather, and consequently the weatherman does not "cause" the weather. This gets to the nub of the matter: what variables z ought to be controlled for? Granger (1980) has suggested that z should represent "all the knowledge in the universe available at that time" except the history of x. I should have thought that at least we might restrict ourselves to earthly knowledge. In practice, one selects a shorter list of variables. If one is interested in whether money "causes" income, no other variables are necessary (Sims (1972)), or it is necessary to include unemployment, wages, domestic prices, and import prices (Sims (1980a)), or it is necessary to include interest rates, domestic prices, federal expenditures, and federal revenues (Sims (1982)), or it is necessary to include prices and credit (Friedman (1982)), or it is necessary to include ...

Are the other variables selected at random? Is their choice less whimsical than the choice of instruments in the Cowles program? Should we control for money in the weather-forecasting equation? "No," "probably not," and "of course not" are the answers. The variables that we select are based on a priori notions about the causal structure. It is thought that the weatherman passively collects information useful for the prediction, and money is rather unlikely to be one of the variables. The point that I am trying to make here is that a theoretical structure is required to direct the selection of the variables. It is clearly a matter for theoretical speculation whether money is more like a weather forecast or more like a low-pressure system to the west, the latter but not the former relationship being causal. No matter how diligently you study the time series on money and income, you cannot answer that question. It must be a matter of professional embarrassment that there are so many articles discussing whether x Granger causes y without an adequate reference to a theoretical structure.

However, it is sometimes argued that tests of precedence should be treated as summary statistics like means and standard errors. Summaries are designed to give a reader a sense of the data that is unencumbered by theoretical notions that may or may not be shared by the reader. An excellent example that could serve as a model for time series studies is Ashenfelter and Card (1982), who first establish the time series "facts" about wages, prices, unemployment, and interest rates, and then study the extent to which competing models can explain these facts. The general problem with this viewpoint is that a decision must still be made concerning the variables to be studied. There are vast numbers of F-tests of precedence corresponding to different sets of control variables that could be reported. Which ones should be? Again, some theoretical structure is required. Clearly, Ashenfelter and Card (1982) have in mind a very specific set of models which serves as a foundation for the choice of variables. Sargent, in his reply to critics in *New Methods of Business Cycle Research* (1977) seems to advocate taking all variables two at a time, since this is unlikely to uncover precedence relations that do not exist for higher dimensional models. But Sims (1972) found money to precede income, yet he (1980b) included interest rates and found the opposite.

Feigl's definition of causality (predictability according to a law) is a step in the right direction, since it refers to a body of knowledge that is used to interpret a relationship of precedence in terms of causality. The belief that weathermen do not cause the weather even though the forecasts routinely precede the weather rests on our beliefs about the conditions that determine the weather, about the extent to which humans can alter those conditions, and about the behavior of the weathermen.

The third definition of causality is implicit in the Cowles program and is the one that applied econometricians routinely use, rightly so I would argue. This definition makes a distinction between causes of endogenous variables and causes of exogenous variables. An exogenous variable x is a cause of an endogenous variable y if modifications that alter the path of x alter the distribution of y. This definition will not do for endogenous causes, since the modification that alters the path of an endogenous variable has to be more fully defined. It can be fully defined by referring to a surrogate exogenous variable. For example, weather forecasts do not cause the weather because weather forecasts are endogenous and because surrogate exogenous variables such as the weatherman's health do not enter the conditional distribution of the weather. That is to say, a modification implicit in the statement "weather forecasts do not cause the weather" might be coercion of the weatherman at gunpoint to make a favorable forecast. A surrogate exogenous variable for this modification is the weatherman's health which we suppose could alter his tendency to make favorable forecasts in the same way that a gun might. If the weather is unaffected by the weatherman's health, then the proposed modification will also leave the weather unaffected. Another modification that could alter the weather forecasts would be the seeding of clouds which would affect the cloud coverage. But cloud coverage is not a proper surrogate since it has a direct affect on the weather. Incidentally, in underidentified systems there may be no surrogate exogenous variables corresponding to an endogenous variable of interest. In that case, a fictitious exogenous variable can be added to the system to serve as a surrogate. The weatherman's health is an example.

TESTS OF EXOGENEITY

Sims in several places argues that the test for precedence is also a test for exogeneity. For example:

> An implication of Theorem 2 is that many commonly applied dis-

tributed lag estimation techniques are valid only if causality runs one way from independent to dependent variable.... Hence in principle a large proportion of econometric studies involving distributed lags should include a preliminary test for direction of causality. (Sims, 1972, p. 545)

The main conclusions of the paper were summarized in the introduction. I repeat them more briefly here: In time-series regression it is possible to test the assumption that the right-hand side variable is exogenous; thus the choice of 'direction of regression' need not be made entirely on a priori grounds. (Sims, 1972, p. 550)

Either I entirely disagree with these statements, or I do not understand them. In order to comment on them and also to make clear a distinction between precedence and causality, I will assume that a simple demand-and-supply model generates the data. In this setting there can be no third variable effect, and the dual notions of precedence and causality come as close as they can be. There still remains an important difference between causality and precedence unless it is assumed that there is no contemporaneous feedback, that is to say, no simultaneity problem. Moreover, the hypothesis of exogeneity as I understand the term is not testable.[8]

The model that will be used is the following autoregressive supply and demand model:

Demand $$p = \beta_d q + \gamma_{dp} p' + \gamma_{dq} q' + U_d \tag{13}$$

Supply $$q = \beta_s p + \gamma_{sp} p' + \gamma_{sq} q' + U_s \tag{14}$$

where p and q refer to price and quantity, p' and q' refer to price and quantity one period earlier, and U_d and U_s are serially uncorrelated random variables with zero means, variances V_d and V_s, and covariance V_{ds}. The decision to put p on the left-hand side of the demand equation and q on the left of the supply equation is an immaterial normalization chosen entirely for notational convenience. A government takeover of the producers and the subsequent fixing of the sequence of quantities supplied is the modification that I will consider. It is assumed that this modification leaves the path of the random variable U_d unchanged. Thus, after the modifi-

[8]Jacobs, Leamer, and Ward (1979) offer a similar discussion.

cation, the generation of p is fully described by the demand equation. Before the modification the system can be described in terms of the reduced form: $(p,q) = a*\gamma*(p',q') + a*U$, where a is the matrix

$$a = \begin{bmatrix} 1 & -\beta_d \\ -\beta_s & 1 \end{bmatrix}^{-1} = \begin{bmatrix} 1 & \beta_d \\ \beta_s & 1 \end{bmatrix} /(1-\beta_d\beta_s) \tag{15}$$

Using this reduced form it is possible to solve for the conditional mean of p given p', q' and q as

$$E(p|p',q',q) = A*p' + B*q' + D*q \tag{16}$$

where

$$A = [\gamma_{dp} + \beta_d\gamma_{sp} - D(\beta_s\gamma_{dp} + \gamma_{sp})]/E$$

$$B = [\gamma_{dq} + \beta_d\gamma_{sq} - D(\beta_s\gamma_{dq} + \gamma_{sq})]/E$$

$$C = [V_d\beta_s + V_s\beta_d + V_{ds}(1+\beta_d\beta_s)]$$

$$D = C/[V_s + 2\beta_sV_{ds} + V_d\beta_s^2]$$

$$E = 1-\beta_d\beta_s.$$

Likewise, the conditional variance is $Var(p|p',q',q) = F$, where

$$F = V_d + 2\beta_dV_{ds} + V_s\beta_d^2 - CD \tag{17}$$

The condition for p', q', and q to be a set of exogenous variables for p is that the modification that alters the path of q leaves the conditional distribution of p given p', q', and q unchanged. Because the modified conditional distribution is implied directly by the demand equation, the exogeneity conditions are

$$\gamma_{dp} = A \tag{18}$$

$$\gamma_{dq} = B$$

$$\beta_d = D$$

$V_d = F.$

These exogeneity conditions are rather complicated, but they are implied by the usual exogeneity restrictions:

Hypothesis of Exogeneity :

$$V_{ds} = \beta_s = 0.$$

This exogeneity hypothesis refers to the conditional distribution of p given q, q', and p'. This would be the relevant distribution for designing a control rule if the choice of q can be made contingent on the value of p'. This is the distribution that would ordinarily be studied by econometricians who would implicitly take this exogeneity hypothesis as given when they regressed p on q,q', and p'. But if q must be chosen to control p without knowledge of the history of p, then the relevant distribution conditions on q and its history only. The conditional distribution of p given q and its history is invariant to modification under the stricter exogeneity hypothesis:

Hypothesis of Self-Generation :

$$V_{ds} = \beta_s = \gamma_{sp} = 0.$$

In this case, the q sequence is generated separate from the p sequence. This is not ordinarily an hypothesis of special interest because a control rule can usually condition an p', but I include it here since, as is argued subsequently, if V_{ds} is assumed to be zero, this hypothesis is partially testable, whereas the hypothesis of exogeneity is not.[9]

The government's modification of the system is presumably intended to affect the price of the commodity, but the modification that fixes the path

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[9]The hypothesis of exogeneity identifies the condition required for q,p' and q' to form a set of exogenous variables for p, where I have used my definition of exogeneity. It is the tradition, however, to call, q, p' and q' the "predetermined variables" and to separate the set of predetermined variables into lagged variables (p' and q') and exogenous variables (q).

of q will leave the path of p unaffected under the following condition:[10]

Hypothesis of Noncausality :

$$\beta_d = \gamma_{dq} = 0.$$

Several comments may be made to contrast these hypotheses. The hypothesis of exogeneity addresses the question whether or not the government can plan its modification using the observed conditional distribution of p given p',q', and q. This consequently concerns itself with whether or not in the sample the generation of the q variables is essentially the same as in the period of modification. Specifically, in the sample there can be no feedback to q from p in the current period, but there can be lagged feedback in the sense that γ_{sp} need not be zero. The hypothesis of noncausality on the other hand is concerned with whether or not the modification can have an effect on prices. It consequently deals with the generation of prices, not the generation of quantitites which is the concern of the hypothesis of exogeneity. If this hypothesis of noncausality can be rejected, then the conditional distribution of p given q,q', and p' in the modification period depends on q or q', though if the hypothesis of exogeneity is not also true this conditional distribution will not be the same as the conditional distribution in effect before the modification.

The hypothesis that the quantities do not regularly precede the prices is a restriction on the reduced-form coefficient of the q variable in the p equation:

Hypothesis of Nonprecedence (q not before p) :

$$[\gamma_{dq} + \beta_d\gamma_{sq}]/[1-\beta_d\beta_s] = 0$$

The hypothesis that prices do not regularly precede quantities is a similar restriction on the reduced-form equation for q:

[10]For this example there is no surrogate exogenous variable that plays the role of the modification. A suitable surrogate would be a variable, say x, that entered the supply equation (14) but not the demand equation (15). This x variable enters the conditional distribution of p given p', q', and x if and only if the hypothesis of noncausality is false.

Hypothesis of Nonprecedence (p not before q) :

$$[\gamma_{sp} + \beta_s\gamma_{dp}]/[1-\beta_d\beta_s] = 0.$$

Parenthetically, I trust that it is not necessary for me now to repeat that I am using the word "precede" in place of the wordpair "Granger cause." Note, then, (1) the hypothesis that q does not cause p implies but is not equivalent to the hypothesis that q does not precede ; (2) the hypothesis that q, q', and p' form a set of exogenous variables neither implies nor is implied by the hypothesis that p does not precede q; (3) the hypothesis that q is self-generating implies but is not equivalent to the hypothesis that p does not precede q.

A data analysis of this system begins with the estimation of the reduced form and unscrambles the reduced-form estimates to arrive at estimates of structural coefficients. The hypotheses of nonprecedence are obviously testable because they deal with reduced-form parameters, but neither the hypothesis of noncausality nor the hypothesis of exogeneity nor the hypothesis of self-generation is fully testable because the model is not identified and because these hypotheses are not equivalent to the testable hypotheses of nonprecedence. There are several identifying restrictions that could be imposed. If there were no contemporaneous relationship, that is if $\beta_d = \beta_s = 0$, then the hypothesis of noncausality and the hypothesis of nonprecedence would conform. If $\beta_d = \beta_s = 0$ and if the demand shock and the supply shock are uncorrelated, $V_{ds} = 0$, then the hypothesis of nonprecedence and the hypothesis of self-generation would conform, and the hypothesis of exogeneity would be true by assumption. But without these additional identifying restrictions, neither the hypothesis of noncausality nor the hypothesis of exogeneity nor the hypothesis of self-generation is fully testable.

The hypotheses of noncausality and self-generation are partially testable. If it is discovered that q precedes p, then it must be that either β_d or γ_{dq} is different from zero, and q must cause p. If it is discovered that p precedes q, then either β_s or γ_{sp} must be nonzero, and q cannot be self-generating. However, if it is discovered that q does not precede p, it may still be the case that q causes p. Although the nonprecedence hypothesis $0 = [\gamma_{dq} + \beta_d\gamma_{sq}]/[1-\beta_d\beta_s]$ may seem an unlikely possibility if there were a causal relation with either β_d or γ_{dq} nonzero, Sargent (1978) discusses a control problem in which this restriction is the result of the choice of a price equation to minimize the variance of the quantity equation. Likewise, if it is discovered that p does not precede q, it may still be the case that V_{ds}, β_s, or γ_{sp} is nonzero and q need not be self-generating. (I note that Sargent (1978) and Hansen and Sargent (1980) make use of the (unlikely?) assumption that $V_{ds} = 0$, which makes the hypotheses of noncausality and self-generation equivalent.)

But this statement that it is possible to reject the hypotheses of noncausality and self-generation is implicitly based on the impossible assumption that there are absolutely no misspecifications. In practice, small misspecifications, for example errors in measurement, will cause small bias in all coefficients. Sharp hypotheses as a consequence cannot be sensibly tested with economic data, since if the sample size is large enough they will surely be rejected. An hypothesis that is of interest even if there are certainly small misspecifications is the neighborhood hypothesis that the coefficients are close enough to zero that the observed relationship could be attributed to a slight misspecification. Although the sharp hypothesis of noncausality is subject to test as described above, no neighborhood hypothesis is testable because the model is not identified. By this I mean that neighborhoods around zero for the reduced-form parameters map into unbounded intervals for the structural parameters, and it is possible to have a reduced-form parameter very close to zero and also have the relevant structural parameter arbitrarily far from zero.

I think that this discussion can be focused by commenting on the standard practice of ranking variables by F tests for precedence orderings. It would be traditional when analyzing the supply/demand system to report something like the following: "There is evidence that p Granger causes q but there is no evidence that q Granger causes p." This statement is meant to verbalize and, I suppose, to interpret the finding that the t statistic on p' in the q reduced-form equation exceeds some historically accepted standard, say 2, and the t statistic on q' in the p equation falls short of 2. What I am arguing is that it is totally inappropriate to interpret this rather ambiguous statement in the natural way that modifications controlling price would have a substantial effect on the quantity supplied, whereas modifications controlling quantity would not have much effect on the price paid by purchasers. This simply does not follow.

The same conclusion applies more forcefully to the exogeneity hypothesis: it is not testable even if the model is perfectly specified. If you discover that p precedes q, that is, that $[\gamma_{sp} + \beta_s\gamma_{dp}]/[1-\beta_d\beta_s]$ is not equal to zero, there is no special reason to suppose that either V_{ds} or β_s is nonzero. Thus p precedes q is quite consistent with q exogenous. Likewise, p not preceding q is quite consistent with q not exogenous.

There is no way, without imposing additional restrictions, to test the exogeneity hypothesis in any sense.

7. INNOVATION ACCOUNTING

Another major element in the vector autoregression program is a study of the dynamic response of the estimated system to shocks. Though Sims (1980) seems rather careful not to make causal interpretations of this analysis, most readers are not likely to pick up the subtlety in language and are likely to infer that a particular intervention is being studied, namely, one that eliminated the noise that until then was part of the control variable. For example, when Sims (1980a, p. 24) writes that "money innovations are the main source of variation in all three price variables - wages, prices and import prices," who among us does not have a feeling of indignation toward the Federal Reserve Board, even though several pages earlier all that is claimed is that this is a "descriptive device."

The problem with statements such as "money innovations account for 80% of the variability of prices" made in the context of vector autoregressions is that, first, the number 80% is altogether arbitrary for reasons to be discussed below and, second, this statement has interest only if there were an intervention that eliminated the innovation in money and left the rest of the system unchanged. There is nothing in the vector autoregression program that suggests either is possible.

The arbitrariness in the number reported to be the percentage of the variance in y due to the innovation in x is a consequence of the fact that the residuals in the vector autoregression are correlated and, as a result, the innovation is not well-defined. Consider the first-order model

$$y(t) = B*y(t-1) + U(t), \tag{19}$$

where y is a vector, B is a square matrix of coefficients, and U is a serially uncorrelated sequence of random variables with covariance matrix S. The steady-state variance of y can be found by setting $y(t)=y(t-1)=y$

$$\mathrm{Var}(y) = (I-B)^{-1}S(I-B)^{-1}. \tag{20}$$

If S were a diagonal matrix this variance could be written as a linear function of the variances of each of the elements of U, and it would be possible to decompose the variances of elements of y into parts due to the innovations in each of the series. But when S is not diagonal, covariances in S will contribute to the variance in y and no straightforward decomposition is possible. To deal with this indeterminacy, Sims in effect rewrites the vector autoregression including current levels of the variables:

$$y(t) = C*y(t) + B*y(t-1) + V(t) \tag{21}$$

where C is a matrix of coefficients and V is a vector of innovations with a diagonal covariance matrix D. Then the steady-state variance is $(I-B-C)^{-1}D(I-B-C)^{-1}$. In this form the variance of any of the elements of y is expressed as a linear combination of the variances that form the diagonal elements of D, and the decomposition is allowable. However, values of C and D can be chosen arbitrarily provided only that the systems (19) and (21) are equivalent. This implies that C and D must satisfy the relations $(I-C)^{-1}D(I-C)^{-1} = S$. But otherwise C and D are arbitrary, as is the consequent variance decomposition. In order to choose a particular value for C and D, Sims selects a particular ordering of the variables and given that order requires C to be triangular. In the more familiar language, a causal chain is implicitly assumed. Whether this makes sense surely depends on the context. Incidentally, the arbitrariness of the causal ordering has been subject to much criticism, including Gordon and King (1982), but it needs to be added that the degree of arbitrariness is greater than would be implied by studying all possible orderings since there is no special reason why C should be triangular.

Clearly, some economics is required to place any meaning on these variance decompositions. Parenthetically, I ask if Sims' (1980) recursive ordering makes sense: money to real GNP, to unemployment, to wages, to prices, to import prices. It seems to me that he has in mind a rather traditional macro model, with IS-LM followed by Okun's Law followed by the Phillips curve followed by a price mark-up equation, with import prices thrown in as an afterthought.

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