Comments on Granger-Newbold and Sims

by Robert J. Shiller

The Granger-Newbold paper makes some important criticisms of econometric methodology as it is commonly practiced today. The usual assumption that regression errors are serially uncorrelated or follow a first order autoregression is rarely justified on theoretical grounds in the literature, yet alternative representations are in most cases not even considered. This practice follows as Granger and Newbold stress, from a general lack of interest in the error terms or "innovations" which should be more central to our modeling. Undoubtedly, we will see more efforts in the future to combine conventional econometric modeling procedures with time series methods.

The Granger-Newbold paper first points out that current methodology in regression analysis gives spurious significance tests if errors are not serially uncorrelated or first order autoregressive. This fact has been mentioned often before, but it deserves mentioning again. Their point is most dramatic in their example of a simple regression for which, under the null hypothesis that the slope coefficient is zero, the dependent variable is the error term. Here it is most clear that it is inappropriate to casually add as part of our null hypothesis the assumption that errors are not serially correlated. However, the authors do not make a case that prewhitening the data is better than the conventional approach of estimating higher order autoregressive or moving average representations of the error process for generalized least squares.

In the Monte Carlo results which show the spurious significance tests in Table 1, we find that the null hypothesis is rejected most often when $b = 0$ and $b^* = -.8$. For these parameter values, the independent variable x is a random walk while y is given by

$$y_t = 100 + .2 \sum_{j=1}^{t-1} \eta_{t-j} + \eta_t,\ t = 1, \ldots 50.$$

Thus, y_t is a random walk plus a serially uncorrelated error term. With a sample of 50 observations, the standard deviation of the error term η_t must be of the same scale as the standard deviation of the random walk component. Now, we know that when one random walk is regressed on another (independent) random walk, spurious significance tests often arise, but the Durbin-Watson statistics usually reveal serially correlated errors. Here, however, the additional serially uncorrelated component of y apparently "swamps out" the serially correlated errors.

In Table 2, Granger and Newbold show that even when the Cochrane-Orcutt technique is used, significant results are obtained far too often. Of course, a first order autoregression of the errors will not yield white residuals. Here, however, we are not shown the Durbin-Watson, which might have warned us of this.

I'm not sure that the model building strategy outlined in part 4 of their paper captures the best features of the "two philosophies" compared in their introduction. I would say that conventional econometric modeling is motivated more by prior knowledge coming from economic theory or other information about the underlying process which generated the data in contrast to the time series approach which treats the data as stochastic processes without regard to their origin. The eight steps mentioned here say nothing about how one might use prior knowledge about the economics of the situation. Following these steps may make it difficult to see how one's economic intuition can be brought to bear. For instance, while prewhitening two series (with different whitening filters) loses us nothing, this procedure will convert a simple contemporaneous relation into a complicated distributed lag relation.

In his paper, Sims presents a very illuminating discussion of the concept of causality as he and others have defined it. Of particular interest is his distinction between "causal" relations and "structural" relations and his illustration at the end of the paper of some relations which exhibit "spurious causality," that is, which are causal as here defined but *not* structural. Since the concepts have been implicit in much discussion of the causality literature, it is very helpful to see them set down with such precision here. The paper begins with a very abstract discussion of the terms "causal" and "structural" and then gives a rigorous statement of the more narrow concept of Granger-Sims causality and spurious causality.

Much of the criticism that has been made of the Granger-Sims causality tests relies ultimately on the possibilities of spurious causal relations. Part of the criticism has been due to a confusion of the Granger-Sims definition of causality with the usual English language meaning of causality (which is related more closely to Sims' meaning with regard to structural relations). But there is also a substantial criticism that the kind of causal relations which have been found by the Granger-Sims tests may not be very interesting if there isn't at least some presumption that what we are discovering are structural relations. The question then is, How likely is it that we may observe a model with the Granger-Sims form when the relation is not structural?

Sims shows two approaches toward creating examples of spurious causal ordering. In his first approach, summarized in Theorem 1, he shows that when a variable y_1 is used to control an objective variable y_2 we may observe a spurious causal ordering from y_2 to y_1. There are probably no real world circumstances in which we could seriously believe that a consistent policy of this kind was pursued over the sample period, so this approach is probably not of great interest. His second approach, summarized in Theorems 2 and 3, is much more important since it formalizes what must be on the mind of everyone who reads the causality literature. In this approach the possibility is explored that a spurious causal ordering from y_2 to y_1 may arise if both variables are determined by a third variable z, as shown in (15). In this approach one looks for restrictions on the parameters of (15) which assure a causal ordering from y_2 to y_1, as might happen, for example, if $y_{1_t} = z_{t-1}$ and $y_{2_t} = z_t$. In Theorems 2 and 3 Sims provides alternative sufficient conditions which do not assume v_1 and v_2 zero. The important conclusion that Sims draws here is that these conditions are "unlikely." To the extent that the union of these sets of sufficient conditions represents a set of necessary conditions, we can then conclude it is "unlikely" that a model of the form (15) can explain an observed causal ordering, so that we might be inclined to fall back on the assumption that the relation is structural.

It is at this point, when we try to decide which models are "likely" and which "unlikely," that Sims' paper becomes a paper on economic theory. Although Sims never explicitly discusses any particular application, he clearly thinks that genuine structural Granger-Sims causal orderings are "likely" on *a priori* grounds, and he is willing to conclude that certain coefficient restrictions on economic models are *unlikely* without even discussing the particular model in question. He is probably quite right in applying his intuition to the likelihood of certain restrictions on economic models, but his conclusion may seem unconvincing or puzzling to many readers.

Another limitation of this analysis is that it shows at best only that *exact* spurious causal orderings are unlikely, while all we ever learn from the data is that the restrictions on (3) which define a causal ordering hold approximately. It's not surprising that these restrictions on (3) imply equally narrow restrictions on (15). The question that remains unanswered is, Is there a plausible economic theory which would place restrictions on (15) which assure approximately the restrictions on (1) which define causality?

Our perceptions of which models are likely must also color our interpretation of a test result which rejects the hypothesis that there is a causal ordering from X to Y. Suppose, for example, that Sims had rejected (rather than accepted as he in fact did) the hypothesis that there is a causal ordering from money to income. Do we conclude from this alone that it is incorrect to regress income on current and lagged money, using ordinary least squares in search of a structural relation which tells us how income responds when we control money? The answer

is no. It may still be true that there is an exploitable structural relationship from money to income which can be consistently estimated by ordinary least squares under the assumption that the money variables are predetermined rather than exogenous, as may happen if I am ready to assert that feedback from income to money is not contemporaneous and that errors are serially uncorrelated. Under these (perhaps "unlikely") assumptions we may have found a *structural* relation even though the data fails the Granger-Sims test for a causal ordering from money to income. Thus, the interpretation we would place on the rejected causal ordering depends critically on our prior odds on such a model as opposed to the alternative hypothesis that money is predetermined only if it is exogenous.

One final comment about the meaning of the term "structural." Sims says that we cannot tell from the data alone whether a relation is structural because "being structural is a property of the way we interpret the system as applying to the real world." Still, we should not conclude that we cannot use the data to convince ourselves that a relation is structural nor that we necessarily need a controlled experiment. An example, also from the literature on money and income, will illustrate. Milton Friedman saw in the historical data a number of major movements in the money supply due to wars, foreign events, changes in the management of the Federal Reserve System, etc. Since these events were obviously random shocks applied from outside the economic system, he called them "quasi-controlled experiments." The fact that income did move in the predicted direction will convince most people that there was a structural relation from money to income. What Friedman has done in effect is find dummy variables for shock periods which we believe are exogenous and use them as instruments in estimating a regression of income on current and lagged money. Thus, economics need not suffer from the oft lamented "inability to run a controlled experiment" if we choose instruments which we really believe are exogenous.