COMMENTS ON TIME SERIES ANALYSIS AND CAUSAL CONCEPTS IN BUSINESS CYCLE RESEARCH

by

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The papers "The Time Series Approach to Econometric Model Building" by C.W.J. Granger and P.S. Newbold and "Exogeneity and Causal Ordering in Macroeconomic Models" by C. A. Sims embody thoughtful and useful analyses of two very important and controversial issues, namely (1) how to build econometric models and (2) how to interpret causality in econometric models. Since the topics are controversial, the authors will probably not be surprised to learn that readers, including this one, do not agree with all aspects of their papers. Since I favor reverse alphabetical order for obvious reasons, I shall consider Sims' paper first and then turn my attention to the Granger-Newbold paper.

Sims' paper treats various aspects of the Simon, Wold and Wiener-Granger concepts of causality. The point that "...the demonstration that a system like (1) with a Wold ordering [i.e. a "fully recursive" structural form] will fit the data is no evidence at all that the ordering is structural" (p. 13) and what follows have been analyzed in a paper by Basman (1965). That applications of the Wiener-Granger causal ordering can lead to spurious causal interpretations in certain cases is also of great interest. Several years ago

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I discussed a case with Sims relating to money supply changes and stock returns that arose in Cooper's (1971) work in which a Wiener-Granger ordering could be misleading. At low frequencies and at seasonal frequencies, it appears that stock returns lead money changes by a slight margin. It might be tempting to conclude that variation in stock returns "cause" variations in money. A more subtle and economically satisfying interpretation is that low frequency and seasonal changes in money can be predicted and movements of returns result from actions based on these predictions. Thus the lead of variations in stock returns relative to those of money can be compatible with "causation" going from money to returns and not vice versa. This and other cases presented by Sims indicates that the Wold and Wiener-Granger causal concepts have to be approached with a good deal of caution.

Further, while the Simon, Wold and Wiener-Granger causal concepts are ingenious, I would have appreciated seeing some consideration of how these concepts of causality relate to concepts of causality in the philosophical literature. From a reading of Basmann (1963) and of some other items, I have the impression that the Simon, Wold and Wiener-Granger definitions of causal orderings do not coincide with those of the philosophers of science. Basmann (1963, p. 442) provides a definition of the classical scientific notion of causality that appears consistent with the philosophical literature and adequate for most scientific work in economics, namely,

Assume that [the] mechanism under investigation can be isolated from all systematic, i.e. non-random external influences; assume that the mechanism can be started over repeatedly from any definite initial condition. If, every time the mechanism is started up from approximately the same initial condition, it tends to run through approximately the same sequence of events, then the mechanism is said to be causal. (references omitted in the quotation)
Except for explicit mention of probability, this concept of causality seems to be in agreement with Jeffreys' (1957) who writes, "If we can say with high probability that a set of circumstances would be followed by another set, that is enough for our purposes." (p. 190). Also, it is interesting to observe that Jeffreys (1967, pp. 12-13) considers a concept of causal ordering close to that of Simon's and concludes that it is not satisfactory for scientific work because it takes no explicit account of the variation in the data that models fail to explain. It appears necessary to have the concept of causation couched in probabilistic terms.

What the discussion in the above paragraph perhaps implies is that some, or perhaps all, of the concepts of causality discussed by Sims are at variance with certain important concepts of causality in the philosophical literature. In my opinion, this issue deserves a good deal more attention. Further, on p. 5 of Sims' paper it appears to me that an element of circularity is introduced in tying the notion of causality to "intuitive notions of what causal systems "in nature" must be like." Last on causal issues, it appears that systems exhibiting "spurious" Wold or Wiener-Granger causality can be unspuriously causal in relation to the definitions mentioned in the previous paragraph.

On the very important point in Sims' paper (p. 12) regarding lack of criteria for making assumptions that particular variables are exogenous or predetermined, years ago Haavelmo (1947) considered this problem and provided a useful strategy. He had doubts about the exogeneity of an investment variable in a simple Keynesian model. What he did was to construct two slightly broader models that loosen...
assumption in what he considered to be reasonable ways. In Chetty's (1966, 1968) work it was shown that Haavelmo's first model is nested in his third model and that a test of a joint hypothesis about a structural coefficient's value and a disturbance covariance's value could be used to discriminate between the two variants of Haavelmo's model and thus shed light on whether it is appropriate to assume investment exogenous. Similar tests have been put forward by Wu (1973, 1974) with respect to determining whether or not stochastic independent variables in univariate regression models are distributed independently of the disturbance term. Also, in connection with our time series modeling work, Zellner and Palm (1974, 1975) and Zellner (1975), we have emphasized that the assumption that a variable, or set of variables, is exogenous places testable restrictions on a multivariate time series model for these variables and additional endogenous variables. These approaches that involve consideration of reasonable alternative hypotheses about a variable's status and formulation of appropriate test procedures to permit the data to help decide the issue of whether a variable (or set of variables) is exogenous appear very promising.

As regards the Granger-Newbold (GN) paper, "The Time Series Approach to Econometric Model Building," it is easy to agree with many of the reasonable points that they have made in their paper, namely (1) the optimum approach to econometric model building should probably involve the best features of traditional econometric analysis and time series analysis (p. 1), (2) time series techniques can be used to improve methods of model construction (p. 1), (3) econometricians (and, I add, some statisticians, engineers, et al.) have
been somewhat amiss in their analyses of time series data, (4) the
gap between the modeling of a two hundred equation econometric model
and the two-series time series method is a huge one (p. 6), (5) "It is
obviously necessary to severely limit the possible models and this is
generally done by invoking some economic theory, which indicates groups
of explanatory variables and, equally important, suggests variables
that be left out of a structural form equation." (p. 5) and (6) "It
is important to emphasize that we strongly believe that there is no
animosity between classical econometric model builders and time series
modellers." (p. 23).

In part, it is easy for me to agree with the half dozen points
listed above because they are consistent with views that I have put
forward in my own work and in talks since about 1972. I have con-
centrated effort on the production of better econometric model con-
struction techniques by blending traditional econometric and time
series analysis in an approach discussed and applied in Zellner and
Palm (1974,1975) and Zellner (1975). Further applications of the
approach in analyses of small models are provided in Palm (1975),
Evans (1975a,b), Fernandez (1974), and Flosser (1975). That we
have converged independently on the need for better techniques for
determining the forms of models, lag structures for variables, serial
correlation properties of error terms, endogenous-exogenous classifica-
tion of variables, etc., is probably due to the seriousness of the
need. In my own case, this need was dramatically brought to my
attention while interacting with the Federal Reserve-MIT-PENN model
builders in the late 1960s.
In terms of the small models that have been analyzed in the references cited in the previous paragraph we have found that time series techniques have been valuable in developing and checking the empirical implications of various variants of models and in ruling out a number of variants as being inconsistent with the information in the data. This is important since models that are inconsistent with the sample information can obviously lead to poor predictions and control policies. Briefly, our procedure involves the following steps:

1. Use economic theory and other prior information to formulate a tentative linear dynamic structural model in terms of variables that have been transformed, usually by differencing, so that they are stationary.

2. Deduce the forms of the final equations and transfer functions associated with the tentatively entertained structural model. The final equations are in general in the form of a set of highly restricted autoregressive-moving average (ARMA) processes each of which is in the form of processes studied by Box and Jenkins (BJ) and others. Similarly, the transfer functions associated with the structural equation system are highly restricted, as pointed out by Pierce and Mason (1971), with each equation in a form similar to those studied by GN.

3. Using BJ techniques, large sample likelihood ratio tests, Bayesian posterior odds ratios and diagnostic checks determine the forms of the final equations and the transfer functions from the data. Then check to see if the empirically determined final equations and transfer functions are consistent with the implications of the tentatively formulated structural equation system. If so, the
structural equation system can be estimated. If not, the structural equation system has to be reformulated to get consistency with the empirically determined final equations and transfer functions.

It should be noted that in (3) the empirically determined final equations and transfer functions are useful not only in checking the structural equation system's formulation but can also be used to generate predictions and used for control even if the entire structural model's final form is not yet determined. Thus each part of the procedure produces a result that can be used in practice for a particular purpose; that is, the final equations can be used for prediction, the transfer functions for prediction and control and the structural equation system for prediction, control and structural analysis.

In applying the approach described above we have encountered several problems that have relevance for the GN paper and with which GN are probably familiar. First, the problem of determining the forms of final equations and transfer functions when data are limited is a difficult one. While many forms can be rejected as being inconsistent with the information in the data, there is usually difficulty in discriminating finely among several remaining forms. We have augmented BJ procedures by using large sample likelihood ratio tests to discriminate among nested alternatives. Also, we have used posterior odds ratios to discriminate between alternative final equation models in some simple cases (white noise versus first order moving average process and random walk versus stationary and explosive autoregressive processes).

Second, there is the problem of the reliability of large sample estimation and testing procedures when sample information is limited.
Third, if the range of acceptable structural equation systems has been narrowed by the procedures described above, there may be the problem of discriminating among the remaining structural formulations.

Fourth, the problem of seasonality is a thorny one. GN use seasonally adjusted data in the analyses reported in their paper. Zellner and Palm (1975) and Plosser (1975) have indicated that use of seasonally adjusted data can introduce spurious effects, particularly if the seasonal adjustment procedure is inadequate. Use of seasonally unadjusted data involves consideration of how seasonality enters a structural model and use of "seasonal" final equations and transfer functions, problems currently being analyzed by Plosser.

Fifth, as is well known measurement errors in data can vitally affect an analysis.

Sixth, on occasion the problem of transforming variables, say by differencing, to induce stationarity can be a difficult procedure. For example with monthly U.S. data, first differencing the logarithm of the consumer price index appears adequate to induce stationarity before the mid-1960s but not afterward.

Seventh, process parameters' values may not be constant through time.

Eighth, forms of policy-makers' control may change with resultant instability in lag parameters and other features of a model, a point emphasized by Lucas (1973). Analyses using sub-samples of data can be performed that may indicate the possible importance of this problem.

Ninth, the BJ, GN and our approach involve informal methods, informal in the sense that many non-independent pre-tests, etc., are involved in the model building process. It would be desirable to have
these procedures made more "statistically formal." In some cases in which specific alternative models are considered, it seems possible to get a formal model discrimination analysis either through use of likelihood ratio tests or by Bayesian posterior odds ratios.

Tenth, there is the problem of developing further multivariate estimation and testing procedures for sets of final equations, transfer functions and structural equations. While joint estimation techniques that exploit parameter restrictions cutting across equations and correlations of error terms can be expected to yield more precise estimates, it is the case that this potential gain in precision may not be realized if the system is incorrectly specified. Hence it is very important to look at parts of systems carefully before applying multivariate estimation techniques.

While this partial list of problems is a long one, it should be appreciated that these are also problems faced by model-builders employing traditional model building techniques. While time series techniques may not be a complete panacea, I agree with GN that the use of time series techniques in combination with elements of traditional techniques, including simulation experiments, will probably lead to a significant improvement in the quality of econometric models.
REFERENCES


FOOTNOTES

1 See also Evans (1975a). In this paper a small macroeconomic model is formulated that can specialize to a "closed economy" model with money exogenous and the price level and the interest rate endogenous or to an "open economy" model with money endogenous and the price level and the interest rate exogenous. Using time series techniques, Evans employed data to determine which variant of the model is supported by the information in the data. Also, Quenouille (1957) provides interesting analysis of similar problems.

2 The requirement that the model be linear in its parameters may be restrictive in some circumstances. Good procedures for analyzing nonlinear time series models would be welcome but, as far as I know, have not as yet been developed. It is recognized that some work has appeared on estimation procedures for given nonlinear models; however, very little work has appeared on useful procedures for formulating and using data to check the specifying assumptions of nonlinear models.

3 See e.g. Zellner and Palm (1970) and Zellner (1975) for the definition and examples of final equations associated with linear dynamic structural econometric models.

4 The term "model identification" has been used by BJ and others to describe this process of determining the forms of time series models that are consistent with sample and other information. Since identification has a well-defined meaning in the statistical and econometric literature, it is unfortunate that time series workers and engineers use the term in different senses. Contrary to GN's suggestion (p. 12), I believe that time series analysts and engineers, rather than econometricians, should alter their terminology to make it consistent with standard statistical usage.

5 In my opinion, there is a need for additional work on the economic theory of seasonality since seasonal variation accounts for a large fraction of the variance of many economic variables.

6 See Pierce (1975) for a valuable analysis of relative forecasting properties of some dynamic time series models.

7 Here I refer to both "systematic" and "random" measurement errors.

8 I have stated on many occasions that current traditional econometric techniques for treating many of these problems are woefully inadequate or non-existent.