

Comments on Sargent and Sims'  
"Business Cycle Modeling Without Pretending  
to Have Too Much *A Priori* Economic Theory "

by L. R. Klein

It is fortunate that Tjalling Koopmans is present at these meetings to tell us what he really meant when he wrote "Measurement Without Theory." It seems to me that Sargent and Sims are misusing Koopmans' arguments for their own purposes. He criticized the National Bureau of Economic Research (NBER) for not using economic theory in their business cycle studies, and I would criticize Sargent-Sims for the same deficiency. Koopmans was also criticizing the National Bureau for not using statistical theory or stochastic specifications in their nonparametric approach to cycle measurement. Sargent-Sims very admirably use deep statistical theory with much stochastic structure in their analysis, but this is no substitute for economic theory. If they do not introduce some more aspects of system structure, both from economic theory and knowledge of economic institutions, for *restricting* the parametric specifications of their models, I am afraid that all is lost. All the problems of collinearity, shortage of degrees of freedom, and structural change will so confound the interpretation of their results that we shall not know what to make of them. In this respect, I find their approach to be disappointingly retrogressive and contrary to the main stream of econometric analysis today.

There are two bothersome misconceptions, either explicit or implied in the Sargent-Sims presentation, and I think that they are basic enough that it is worthwhile taking some time to consider them. One deals with the meaning and interpretation of the Phillips curve and the other, with the available or necessary degrees of freedom for econometric inference.

*The Phillips Curve.* The Phillips curve is not an optimizing relation that should be deduced from some principles of best behavior in either the micro or macro sphere. It is simply a market clearing relation. On the one hand, there are optimizing decisions of households (and trade unions) about labor supply and, on the other hand, optimizing decisions of firms

about labor demand. When employee and employer representatives come to the bargaining table, with all the institutional apparatus that such a process entails, a wage bargain is struck on the basis of labor market and other economy-wide considerations. It is surely an accepted part of our subject's view of the working of markets that wages move in response to excess supply or demand in order to set up a tendency towards restoration of equilibrium. It is just a way of introducing dynamic adjustment processes into the reconciliation of two optimizing decisions, and it is fruitless to look about for some optimizing explanation of the Phillips curve.

Also, the Phillips curve is a structural relationship (or a whole set of them) within the context of a macro model. It is frequently confused with the "trade-off" relationship between price change and unemployment level. But the "trade-off" relation is not a structural relationship in the same sense; it is the "parametric" relationship between two reduced form expressions — one for the rates of change of price and one for unemployment, each of which are endogenous variables in a complete system. We have, as part of the system solution

$$(\Delta p/p)_t = f(\text{initial conditions, exogenous history})$$

$$U_t = g(\text{initial conditions, exogenous history})$$

Since the arguments of  $f$  and  $g$  are the same, there is an implied relation between  $(\Delta p/p)_t$  and  $U_t$ ; this is the trade-off relation between price change and unemployment.

*Degrees of freedom.* It is sometimes felt that the number of degrees of freedom are limited purely by the number of data points. A more revealing way of looking at the problem is to assume that one starts out a model investigation with  $nT$  degrees of freedom, where  $n$  is the number of equations in the system and  $T$  is the number of data points. The product  $nT$  gives the number of stochastic elements in the joint likelihood function. Against these  $nT$  degrees of freedom, the estimation of parameters uses many in the process of statistical inference. Some estimation methods are expensive; some are frugal. Requirements can be worked out for various methods from the most expensive

$$T > m + n \quad (m = \text{number of predetermined variables})$$

in the case of *FIML*, to the most frugal

$$T > m_i + n_i - 1 \quad (m_i = \text{number of coefficients of predetermined variables and } n_i = \text{number of coefficients of endogenous variables in the } i\text{-th equation})$$

for *OLS* or *IV*. The point is that an appropriate method can almost always be found, even for the very large structural models now in use,

and the full dynamic properties can be estimated. The standard approach to model building can always use more degrees of freedom to good advantage. Econometricians hardly ever have as much as they would like, but they do not have too few to make inferences. The situation is not as bad as Sargent-Sims imply. They use degrees of freedom wastefully because they are unwilling to let *a priori* economic analysis do some of their work. Their methods are much too gross and therefore need more observations.

Since Sargent-Sims draw upon the same data as do model builders, they should get broadly similar results. It would be most unusual if different reasonable analyses of the same data — by Sargent-Sims, the NBER, or model builders — came to radically different findings, except for the fact that nonstructural approaches may not be able to tackle certain types of hypothetical analyses. Model builders can do everything that Sargent-Sims do, and then some; with a model, we can analyze a rich variety of alternative policies or other simulations. I can agree with all the numbers and numerical calculations that Sargent-Sims come up with, and the same holds true of John Geweke's paper; but I cannot agree at all with their overly strong conclusions or interpretations. The causality lines that they claim to have established do not make sense to me.

Also, the Sargent-Sims results in the frequency domain should agree with similar results in the time domain. Much contemporary analysis using principal components, especially for data reduction in the case of the first stage of *TSLS*, show that the main economic indexes can be broken into major classes of trends and cycles of differing periodicities. This is all in line with the cumulative history of NBER findings. At a much earlier date, Stone [145] extracted the principal components of the U.S. national income accounts and found that their movement could be accounted for by a trend term, a GNP term, and a  $\Delta$ GNP term. Time domain analysis of the cyclical content of the Wharton Model of the U.S. Economy shows the existence of two superimposed cycles after trend extraction, much in line with Stone's nonparametric investigation. At this level of analysis, I find no contradiction between the work of Sargent-Sims and either the model builders or time series studies in the time domain.

A point raised by several time series analysts, and also by Sargent-Sims, that estimation of economic relationships should pay more attention to the lag structures and the isolation of white noise residuals is well taken. I could find comfortable accommodation with the linear model specification

$$A(L)y_t + B(L)x_t = C(L)e_t$$

where  $y_t$ ,  $x_t$ ,  $e_t$  are  $n$ ,  $m$ ,  $n$  element vectors, respectively. Each equation of a complete system would thus be of the type used by Box-Jenkins in their *ARIMA* analysis. A good procedure might be to specify each

equation individually on the basis of economic theory and other *a priori* information as a static equilibrium relation. It would then be cast in dynamic and stochastic form as a typical equation of the above system.

This specification for the structure of the system is convenient for demonstrating in closed form expressions the difficulties that arise in connection with Sargent-Sims views about causality. The solution to the dynamic system, following E. P. Howrey's expressions, are given as

$$y_t = K\lambda' - \{A(L)\}^{-1}B(L)x_t + \{A(L)\}^{-1}C(L)e_t$$

where  $K$  is a matrix depending on initial conditions and  $\lambda$  is a vector of characteristic roots of the system. The trade-off relation between  $\Delta p/p$  and  $U$  is obtained by associating, in a two-dimensional graph, the solutions for these two elements of  $y_t$  obtained when the  $x_t$  vector is distributed in a particular way. I find, using the Wharton Model, that if  $x_t$  is disturbed through unusual rises in the fuel or food price components (internal or external) or in the exchange rate components that the associated changes in solution values  $\Delta p/p$  and  $U$  are positively related. If, alternatively, the external stimulus comes from a domestic fiscal impact in taxing or public spending, I find an inverse relation between the changes in  $\Delta p/p$  and  $U$ . The important point to be emphasized is that these two different trade-off relations were generated from the same model and that structural Phillips curves were included in the model. The structural analysis of behavior and implied causation is capable of generating quite different gross correlations that are really implied by trade-off relations. Gross correlations, no matter how many lead-lag combinations are analyzed by the Sargent-Sims approach, throw no light on underlying causal patterns or exogeneity. What I have done with respect to analysis of the trade-off relations in considering Phillips curve analysis could be done equally well for the analysis of the relationships between money and income. The Sargent-Sims approach, applied earlier to gross lead-lag correlations between money and nominal income by Sims, fails in the same way to reveal the causal pattern.

Finally, in commenting on the Sargent-Sims paper, I should like to direct attention to their system results in Table 26. These results are so implausible that I find it hard to see how they could have put them before this audience in a serious vein. Prices are not going to follow the course that they have projected. There must be something drastically wrong with their system or approach to have produced this result and many of the others in Tables 26 and 27. The block structure discussed in this Appendix is not unlike the block recursiveness of the coefficient matrix and block diagonality of the covariance matrix introduced by Franklin Fisher to estimate large scale models in mainstream econometric analysis.

In John Geweke's paper there are interesting numerical calculations whose arithmetic should not be criticized, but surely there is no basis for drawing such strong conclusions as



...a model ought to exhibit a reduced form equation in which the wholesale price index is the endogenous variable with factor prices (and not much else) explanatory. This property contradicts the frequent assumption that since wages and prices emerge from a complex set of interactions there is bound to be material feedback between these variables.

I find no evidence in his calculations that would lead to these rigid conclusions about two sets of endogenous variables — wages and factor prices. Also, there are many other variables, not considered at all, that should properly enter a reduced form equation for the wholesale price index.

At the outset of Geweke's paper there is a false distinction posed between nominal and real magnitudes. The proper procedure is to model structural behavior relationships, within the context of a complete system, taking variables as they should appear on the basis of *a priori* considerations — in some cases using price relatives and deflated magnitudes and in others using absolute prices where speculative behavior is indicated. From this complete system, one can derive implied relationships among prices and wages in either real or nominal form. It is simply a case of setting up the appropriate calculations. Solutions for either form can readily be derived from dynamic simulations. One would have to be wedded to gross correlations or unstructured reduced forms to feel that a choice has to be made.

In the section of his paper dealing with prewar and postwar relationships, time series analysts are too prone to seduction, to the idea that structure has changed. Actually, there is more stability in basic economic behavior than unstructured time series analysis might suggest. It is important to sort out rather superficial changes of institutional conditions and input values of exogenous variables from parametric changes in fundamental behavior. The concept of *autonomy* of a relationship introduced at an early stage by Trygve Haavelmo is relevant in this respect. If, as suggested in other studies, the prewar U.K. consumption function remains basically stable after the interruption of World War II and postwar reconstruction, it is hard to think that basic household and firm reactions in less disturbed markets for determining wages and prices would not remain stable.

The complaint among time series analysts that either there is no suitable macro theory, or too many, for coming to a decision about model specification is not the right issue. We have good theories in economics, especially about micro behavior and the functioning of markets. What is needed is a proper aggregation procedure for deriving the implied macro relations. Many specification restrictions are statutory; these are firm. Other restrictions based on institutional knowledge of the functioning of the economy must be used, too. Entirely too much emphasis has been placed, in today's discussion, on zero type restrictions. Other important

restrictions from expenditure systems and optimal factor combinations for efficiency should also be used. These have been important in throwing light on investment incentive tax provisions through specification of investment functions. Such restrictions are likely to be important for cases of large step changes in prices where there has been relatively little sample variability. They also help immensely in getting around problems of identifiability, collinearity, and undersized samples. Generally speaking, they lead to nonlinear systems. The realism of the nonlinear aspects is an important advantage over the usual linear model of pure time series analysis. Without theory and other *a priori* information, we are lost. I wonder why Sargent, Sims, and Geweke are trying to lead us away from the established path that was so long in being prepared?