Eastern Economic Journal, Volume XV, No. 4, October–December 1989

"Developments and Prospects in Macroeconometric Modeling": A Comment

Guy V.G. Stevens*

It is a privilege to have been present when the father of American econometric modeling looked backward—and forward—over the field he was instrumental in founding.

Klein divides his survey of the development and prospects in macroeconometric model building into six major topics: statistical methods and techniques; the general question of model size; the modeling of expectations; the special features of models for developing and centrally planned economies; the study of economic policy; and new developments. I shall concentrate my comments on the first three of these topics, with some attention to the last.

STATISTICAL METHOD AND TECHNIQUE

Lawrence Klein's discussion of the impact of statistical methods—as opposed to developments in economic theory—is fascinating, surprising, and instructive. The drama revolves around the application of simultaneous equation methods to the estimation of macroeconometric models. Here was a body of research that identified a potentially severe problem with the application of ordinary least squares methods to the estimation of economic relationships and, moreover, produced the techniques for apparently remedying the situation. Hopes were high at the Cowles Commission and other centers that the application of these techniques would lead to a new day in model accuracy, not to mention the controlability of the economy. As it turns out, according to Klein's fascinating story, simultaneous equation methods made a rather small contribution over the next forty years. Why was this? Certainly not because any of the results were found to be wrong. Rather, a number of causes led to this conclusion: the cost of using full information, and even limited information methods in an environment where data change and re-estimation must be done frequently; the relative devaluation of the importance of the asymptotic property of consistency and the empirical finding that for most sectors the bias introduced by using ordinary least squares was no more than for consistent methods. The benefit/cost ratio of this innovation turned out to be less than one.[1] A final point to emphasize is that this benefit/cost calculation could only be made by confronting the alternatives with the data over a fairly long period of time.

What did turn out to be important? First, better data and more data; second, new developments in the estimation of lag distributions, ie. "the time shape of economic reactions." Other improvements included stochastic simulation, improved techniques of model comparison

*Senior Economist, Division of International Finance, Board of Governors of the Federal Reserve System. I would like to thank many colleagues in the Divisions of International Finance and Research and Statistics for their helpful discussions. This paper represents the views of the author and, of course, should not be interpreted as reflecting the views of the Board of Governors of the Federal Reserve System or the members of its staff.

and error analysis and, of course, the continuing revolution in computer speed and capacity which allowed disaggregation along many dimensions.

ISSUES OF THEORY AND MODEL STRUCTURE

Most economists are genuinely surprised to learn Klein's conclusion that the use of simultaneous equation methods did not turn out to be an unmitigated benefit. On the other hand, with regard to the key issues of theory and model structure that Klein discusses, it was clear to most from the beginning that the benefit/cost calculation could go either way. Here I will concentrate on the questions of model size and expectation formation.

Model Size

Virtually anyone who has built a macroeconometric model should, and probably would conclude that aggregation in it was carried too far. Hardly any existing aggregation over goods or agents can be defended on the basis of conditions or tests for aggregability.[2] In models that include significant foreign sectors, as virtually all models do, the question of the degree of country aggregation in the "rest of the world" complicates the problem many times over.[3]

But there are costs of disaggregation and Klein's discussion of both sides of the equation is to be recommended. Recommended also is an appreciation of Klein's final position on this issue, informed by forty years of experience, that "we should choose the largest possible system for which a good data base exists, that is manageable with prevailing human resources, and that can project the principal macroeconomic magnitudes as well as the best models of any size can. . ." This conclusion is based on the view that small or "parsimonious" models have tended to break down (in forecasting ability) over the years. As well, I think it reflects his goal to have a model that is not just designed to fight the last war, but one that can handle a rich variety of changes in potential policies and exogenous variables.

Let us not forget that Lawrence Klein was once a "small model man." His first effort in 1950 comprised only 12 estimated equations. But given the resources and computer power, the direction he has gone is clear.

As one who has participated in an effort to build a multicountry model that was "small" in comparison with Klein's LINK system (but large in terms of the total number of equations), I am glad he is still out there on point. Although the team building the Fed's Multicountry Model felt a smaller system than LINK was required at that time to develop a multilateral capital flow and exchange rate sector, we were always painfully aware of the injustices we were perpetrating in terms of the over-aggregation of countries and markets.[4] Not only did we borrow heavily from the LINK system, the existence of this much more disaggregated system provided us with a standard to measure the effects of our (erroneous) aggregation. Because of its size and the institutional arrangements surrounding it, the LINK model may not be the best vehicle to test the initial usefulness of certain system-wide proposals, such as the endogenous exchange rates developed for the Multicountry Model or the alternative expectation mechanisms discussed below. However, it is to be hoped, and has been the case in the past, that the fruitful products of smaller, experimental models will eventually be incorporated into more disaggregated models like Project LINK.

Rational or Model Consistent Expectations

In one sense—not emphasized by Professor Klein—there may be a parallel between the fates of simultaneous estimation methods and "rational," forward-looking, or model-consistent expectations as applied to macroeconometric models. Application of the latter to econometric models involves the same instrumental variable or maximum likelihood methods that have been found so costly in the application of simultaneous equation methods to the estimation of macroeconometric models. Whatever the benefits of model-consistent expectations, it is quite possible that its contribution to the goal of ex ante or ex poste forecasting, that Klein sees as paramount, will not exceed the costs. However, John Taylor, in an excellent assessment of the state of the art circa 1988, tends to think otherwise, except for the applicability of maximum likelihood methods. In any case, Klein is right to say that we have yet to see forecasting results that support the superiority—or, even, parity—of these models.

In a second sense there is no parallel between the fate of simultaneous equation methods and the possible future of model consistent expectations. As noted by Klein and others, and unlike the assessment of simultaneous equation methods, there are many reasons to object to this approach on theoretical or empirical grounds:

(1) the assumption in all applied rational expectation models that all agents agree on a common model—the econometric model being used—for the formation of (common) expectations seems far-fetched;

(2) learning mechanisms are excluded in virtually all empirical modes; where such procedures are included, there is no guarantee of convergence to the true model (see, e.g. Pesaran, 1987, 46–48);

(3) where agents with heterogeneous expectations exist in the same market, the paths of the endogenous variables may diverge from the rational expectations path in both the short and long run (De Long et al.);

(4) particularly for models incorporating expectations of future values of endogenous variables, the conditions for identification may be impossible to fulfill (Pesaran 1987, 156–61); in such cases, even the relatively simple instrumental variables estimators will be unuseable;

(5) models incorporating model-consistent expectations may ignore potentially useful information available from the direct measurement of expectations.

All of the above, to say the least, suggest a poor performance for rational expectation models in what Klein calls the "sternest" test-forecasting. One must recognize, of course, that forecasting accuracy may be supplemented or even rejected as a goal by some researchers. McCallum implicitly takes this position when he argues that doing simulations that lead to comparisons of alternative policy rules "is what economics is primarily about, and has been since the time of Ricardo." If one interprets such an activity as primarily the comparison of the long-run effects of policy shifts that are known with certainty, this capability is likely to have little to do with forecasting ability; moreover, the expectation mechanisms of models that do forecast well may not be well suited for attacking this sort of question. I, personally, do not value McCallum's goal nearly as highly as he seems to; however, his comment underlines the likelihood that not all goals can be achieved with a single econometric model.

Space limitations prevent me from doing justice to a number of areas covered by Klein that merit the reader's attention, particularly his discussion of policy applications, the special features of models for developing countries and centrally planned economies, and many of his recommendations for the future. Among these latter, let me note the following; the combination of forecasts; the use of high frequency data; the use of models with variable or stochastic coefficients (these latter two, I might add, pioneered by my colleagues at the Federal Reserve Board); the further use of game and control theory; and, of course, the continued comparison, along many dimensions, of the forecasting and simulation properties of alternative models.

We are grateful to Lawrence Klein for sharing with us his unique perspective on the past, present and future of the important science of econemetric model building. His is a perspective that has been shaped by his manifold contributions to the field and by his own personal approach to economic research. I interpret this approach or style as a thrust for the widest possible explanation of economic phenomena—breathtakingly wide at times—combined with a very high level of theoretical and econometric sophistication. But at all times it has also meant keeping close to the data—always giving the data the chance to refute or modify our hypotheses, but also doing such mundane tasks as keeping meticulous records of forecasts and trying to improve the database. His approach has done much to make economics a science.

NOTES

1. This statement, of course, applies only to the application of simultaneous estimation methods to large econometric models.
2. See e.g. Green, 1964.
3. For a fairly detailed discussion of aggregation conditions as applied to aggregation across countries, see Stevens et al. 1984, chapter 2.
4. The Fed's Multicountry Model in 1984 had five countries and an aggregate "rest of the world." The model was limited to one composite good per country, a very limited treatment of oil and raw materials, and no breakdown of fixed investment into any of its components. See Stevens, ibid.

REFERENCES

De Long, J. Bradford, Andrei Shleifer, Lawrence H. Summers, and Robert J. Waldmann. "The Economic Consequences of Noise Traders." Discussion Paper #1348, Harvard Institute of Economic Research, Harvard University, 1987.

Green, H.A. John. *Aggregation in Economic Analysis.* Princeton: Princeton University Press, 1964.

Klein, Lawrence R. *Economic Fluctuations in the United States 1921–1941.* New York: Wiley, 1950.

McCallum, Bennett T. "Comment," in *Empirical Macroeconomics for Interdependent Economies,* edited by Ralph C. Bryant, Dale W. Henderson, Gerald Holtham, Peter Hooper and Steven A. Symansky. Washington, D.C.: Brookings, 1988.

Pesaran, M. Hashem. *The Limits to Rational Expectations.* Oxford: Basil Blackwell, 1987.

Stevens, Guy V.G., Richard B. Berner, Peter B. Clark, Ernesto Hernandez-Cata, Howard J. Howe, and Sung Y. Kwack. *The U.S. Economy in an Interdependent World.* Washington, D.C.: Federal Reserve Board, 1984.

Taylor, John B. "The Treatment of Expectations in Large Multicountry Econometric Models," in *Empirical Macroeconomics for Interdependent Economies,* edited by Ralph C. Bryant, Dale W. Henderson, Gerald Holtham, Peter Hooper and Steven A. Symansky. Washington, D.C.: Brookings, 1988.