5-1974


Allan H. Meltzer
Carnegie Mellon University, am05@andrew.cmu.edu

Follow this and additional works at: http://repository.cmu.edu/tepper
Part of the Economic Policy Commons, and the Industrial Organization Commons

Published In

This Conference Proceeding is brought to you for free and open access by Research Showcase @ CMU. It has been accepted for inclusion in Tepper School of Business by an authorized administrator of Research Showcase @ CMU. For more information, please contact research-showcase@andrew.cmu.edu.
Econometric model building of the type represented in the Duesenberry-Bosworth paper was at best an attempt to see whether systems of, say more than 25 equations, add much to our knowledge of the economy and the response to policy changes. They have not, I believe, at least to this point, and I am not optimistic about a change in the next few years. Instead of commenting on the details of the particular models presented at this session, I will explain some of the reasoning that brings me to that conclusion. Before doing so, let me make clear that my comments are not directed at either "theory" or empirical science. There is very little of either in current econometric practice. I view my comments as a criticism of large models, models of twenty-five or more equations, not as a criticism of modelling or testing economic hypotheses.

One way of judging large models is by the accuracy of their forecasts. One recent study compares the forecasts from several models to the average of informed opinion represented by the ASA-NBER judgmental forecasts. The results show that if the forecaster adjusts the model's output to reflect his judgment about what is or is not likely, the average forecast from the model is almost as accurate as the average ASA-NBER forecast on those items for which comparisons have been made. 1 If the econometric forecaster relied solely on the model, his predictions were poorer than judgmental forecasts during the sample period.

The reason most often advanced to defend the frequent adjustment of models and their forecasts is that unanticipated and unprogrammed events—such as strikes, wars, or sudden changes in weather or in government policy—have consequences that differ so much from case to case, or are so poorly understood, that the judgments of informed observers are likely to be more accurate than the judgments rendered by large, econometric models. For sub-units or sub-aggregates, the problem is multiplied many times. New rules, new regulations, or tax changes, and new opportunities that induce firms and households to make substantial adjustments occur frequently.

Many of the changes that are of great importance to sub-units are inconsequential for aggregates. Let me illustrate. Suppose the government adopts a policy that changes some relative price or subset of relative prices. There are, of course, countless policies

* Maurice Falk, Professor of Economics & Social Science, Graduate School of Industrial Administration, Carnegie-Mellon University.

Much of the work on which these comments are based has been carried out jointly with Karl Brunner and supported by grants from the National Science Foundation.

of this kind, and both the policies and the implementation change frequently. Some familiar propositions tell us that market participants respond by searching for substitutes, that the instantaneous effect of the policy differs from the long-run effect, that the adjustment to the policy proceeds through a series of stages until, at last, money prices and market valuations fully reflect the capitalized value of the change in expected return or cost. If we assume that the length of run is positively related to length of time, we can describe the process with an econometric model, perhaps using dummy variables in the equation or equations where the effect of the policy change is believed to be large.

Let the government remove and later reintroduce the restriction. Regulation Q, the investment tax credit or price and wage guidelines or controls are familiar, recent examples applicable to the period of the Pierce-Thomson and Duesenberry-Bosworth models. Is the expected response to the reintroduction of any of these policies the same as the initial response? In a recent paper, my colleague Robert Lucas has shown that, frequently, the responses will differ. Learning occurs, so the parameters estimated when the policies were not in force often do not provide useful knowledge about the future effects of the policy, and the use of dummy variables to allow for each previous occurrence does not, in general, provide knowledge useful for predicting the effects of reintroduction.

One result of using large scale econometric models is to overstate the importance of particular institutional arrangements. Learning and adjustment to regulations often carries with it the development of new institutions or new market arrangements. Regulation Q and the interest equalization tax accelerated the development of the Euro-dollar market and overseas branches of U.S. banks. Expansion and contraction of the commercial paper market is a consequence of a particular set of regulations. One can find numerous examples in the history of markets to illustrate the point.

Substitution between money, bonds, capital, or claims to capital, and current consumption in response to relative prices is much more certain than the particular way in which substitution occurs. The general conclusion that I draw from our experience with large econometric models of the financial sector is that allocative details carry very little information to aid the forecaster or policymaker interested in aggregates. I will not argue the general proposition here or make the general case for small models. A good, specific case has been made in a recent study done at the Federal Reserve Bank of New York. The study shows that each of three single equation models of the money stock predict monthly or quarterly changes in money with lower mean and lower standard deviation than the Pierce-Thomson monthly model. Further, the same conclusion is reached if the single equation models are restricted to information that is available at the time of the forecast and the Pierce-Thomson model is permitted to run with the actual time series.

The builder of a large model is often unresponsive to criticism of this kind. The ability to predict changes in a particular aggregate is not the only purpose of large models. Policymakers are said to be concerned about allocative detail because they wish to change the allocation of resources (or the distribution of income) by changing lending and borrowing, or the flow of funds in the Duesenberry-Bosworth model. Housing is one of the sectors that policymakers talk most about. An earlier version of the Duesenberry-Bosworth model makes clear that one of the reasons for developing disaggregated models of the financial sector is to trace the effect of financial allocation on housing.

Simulations carried out using the model, however, show a very different result. A $1 billion increase in unborrowed reserves—an aggregative policy—has considerably more effect on housing starts than $1 billion of FNMA mortgage purchases, an allocative policy. Within six months of the increase in unborrowed reserves, residential construction rises by more than $1 billion. After eighteen months, residential construction increases by more than $3 billion or almost 14% at the levels used in the simulation. FNMA purchases, on the other hand, have very little current or lasting effect on housing. The peak response to the $1 billion purchase is less than $1 billion and occurs within six months. Eighteen months after the purchase, residential construction is lower than before the purchase. Within a year of the purchase, few of the listed components of GNP, including construction, have changed by as much as one-half billion dollars. After two years, there are no such changes.\footnote{4}

This is not a surprising result or one that can be attributed to some defect of the model. Despite the words authors sometimes use to describe their results, many large econometric models show that mortgage policy has very little effect on housing.\footnote{5} The particular version of the Bosworth-Duesenberry model that I read goes a bit further than most. Their results show that FNMA purchases have no lasting effect on the mortgage stock. A $1 billion purchase produces a peak increase in household mortgages of $800 million followed by a steady decline through increasingly negative values. $1 billion of unborrowed reserves induces substantially greater, more enduring and not entirely plausible increases in mortgage borrowing.

The point to be emphasized is not the specific result but two more general conclusions. First, policies to reallocate financial resources have very little aggregative effect in a model constructed to bring out those effects. Second, policies to increase housing or other components of real expenditure by changing the mix of financial assets and liabilities have not been shown to have these effects even by the relatively weak test of simulation.

I would like to close my discussion of the Duesenberry-Bosworth paper with some questions. One of the most puzzling features of many large econometric models, including the Bosworth-Duesenberry model, is the considerable attention directed at details of financial markets. In contrast, there is a noticeable absence of interest in markets for real assets. The first version of the Duesenberry-Bosworth paper mentions common stocks once, I believe, and lumps them in a residual category. Other stocks of real assets fare no better. Does this mean that attempts to shift from real to financial assets are expected to have little effect? Or, does it mean that such adjustments occur much more quickly than adjustments of financial assets and liabilities?

In my view, fluctuations in prices of existing assets relative to the prices of current output are a much neglected feature of financial markets. A proper statement of the relation between nominal and real rates of interest would, I believe, weight the expected return to real capital per unit of real capital, e, by the ratio of the price of output to the price of existing assets, p/P. The real rate of interest is the product of these terms, and the nominal rate, i, is the sum of the real rate and the anticipated rate of inflation, \( \pi \).

\[
i = \frac{p}{P} e + \pi.
\]


Discussion

Much attention has been directed to the effect of price anticipations on market interest rates. Much more attention is directed by the builders of large models to the determination of nominal rates for many different financial assets and liabilities. Practically no interest is directed to the fluctuations in real rates of return produced by fluctuations in the relative prices of assets and output. Does this misplaced emphasis help to explain the failure of large econometric models to predict more accurately than judgmental forecasters?

Let me conclude with a few comments on the Pierce-Thomson paper. I am pleased to see some of the changes that have been made in the monthly model during the last few years. The shift in emphasis toward money and away from disaggregation is useful. The Pierce-Thomson paper agrees with much of my discussion about the net cost of modelling the details of financial structure. They argue, correctly I believe, that most reported market interest rates are highly correlated, so it is not useful to forecast each of the rates on financial assets. This is a move away from flow of funds with its emphasis on the details of financial structure.

The Pierce-Thomson paper continues to suggest that the demand for nominal money predicts the stock of money. This is true only partially and only to the extent that the Federal Reserve focuses on control of interest rates. It has no consequence or implication for the quality or performance of the monthly model.

Recognition that disaggregation is costly should be followed by a reallocation of resources from collection and processing of data on the flow of funds to improved statistics on the money stock. This year, the growth rate of the money stock will be closer, in my opinion, to 7% than to the 5-1/2% currently reported. Errors in counting the money stock are in part the consequence of insufficient expenditure on monetary statistics and reliance on complex institutional arrangements. A reallocation of resources from flow of funds statistics to statistics on money and to the definition of money is overdue. Better statistics on money will improve control of money and improve monetary policy. The expenditure can be justified as a net social benefit. Additional benefits will be obtained if better statistics on money are accompanied by better models to control money. I look forward to the next version of the Pierce-Thomson model with the hope that the demand for money will be joined by a supply equation dependent on the monetary base.