

“What Makes a Good Model for the Central Bank to Use?”

by David J. Stockton¹

I thought the most useful contribution that I could make to this panel is to describe how the staff at the Federal Reserve Board approach the use of models in the monetary policy process. It will come as no surprise to you that we at the Fed do not think that we have settled on the perfect formula for using economic models in carrying out our mission to provide analysis and guidance on issues of monetary policy. I would like to make a few simple points. First, our core model, FRB/US—a large-scale structural econometric model—plays a very central role in macroeconomic analysis at the Board. Second, that model is not sufficient, and our analysis incorporates a large variety of models that employ many different approaches. And third, we view all of our models as grossly simplistic representations of reality, and they are not used in any mechanical sense for the conduct of policy—at least not without considerable scope for judgmental adjustment.

It is relatively straightforward to answer a simpler question than the one posed for this session. I can answer the question of what the ideal model would be for a central bank to use. An “ideal” model would begin by providing a complete description of the structure of the economy. That description would not be limited to the U.S. economy, but would also include the features of foreign economies that have the potential to feed back onto the performance of the U.S. economy. The model would be linked to the observable world through accurate and timely data and would use estimation techniques capable of delivering well-identified estimates of

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structural parameters. It would include both sensible steady-state properties and accurate estimates of the dynamics that define short-run behavior. The model should be rich enough to embody all of the costs associated with inflation, deviations of output from potential, and with the higher moments associated with these variables. An alert research staff should be busy developing evidence against the model and revising the specification accordingly.

Of course, we will need more than just this structural model to make policy. We are going to need a complete articulation of the objective function of the policymakers satisfying all the usual restrictions. Then with a detailed and accurate model of the economy and with a complete description of preferences of the policymakers, we simply would need to set the policy instrument to maximize the value of the objective function subject to the constraints imposed by the model.

I think one can see some tenuous connection between this idealized model and what actually occurs in the monetary policy process, but it is also easy to see important points of departure. To begin with, we know that we do not have a complete and accurate description of the structure of the global economy. If we did, we would not need conferences like this one. Indeed, we spend a great deal of time thinking about how to use our models when we know with probability one that they are misspecified, but we are uncertain about the nature of the misspecification. Unfortunately, developing evidence against most macroeconomic models is much easier than knowing how to reform them. As a second shortfall from the idealized model, there are significant problems with both the timeliness and accuracy of the data that we are working with and on which we estimate our models.

As a third shortfall, it would be a daunting challenge to develop a complete description of the objective function of the policymakers, especially given the institutional structure of the Federal Reserve System. When all seats on the Board are filled, there are 19 participants involved in FOMC deliberations—twelve of whom are voting members. To make matters even more complicated the composition of the voting members of the Committee changes each year, as voting seats rotate among the bank presidents. Trying to devise a mathematical representation of even one Committee member would be difficult—trying to devise a representation for the 19 Committee participants and their collective behavior would be nearly impossible.

So given these difficulties, what do we do? The research function of the Board, broadly defined, faces a budget constraint, and we allocate our scarce resources in a way that we hope will maximize the return to our efforts—in terms of the quality of our input into the process of monetary-policy decisionmaking. The result of that allocation has us involved in a wide variety of activities. Certainly among the most prominent of those activities, the staff of the Federal Reserve Board has a long history of serious commitment to the development, improvement and use of large-scale structural macroeconomic models. As most of you know, the current version of the model in use at the Board is the so-called FRB/US model—a forward-looking structural model of the U.S. economy with about 60 stochastic equations and 300 identities.

However, only a fraction of the macroeconomists at that the Board are involved directly with the development of our large model. We also devote considerable effort to basic research; while there are many facets to that work, at one level or another, one can think of much of this research as developing evidence against our core model. We also do a considerable amount of work focused on developing strategies for monetary policy to cope with the uncertainty

surrounding our knowledge about the structure of the economy and the problems with the data on which the policymakers must rely. Finally, we see considerable payoff to allocating resources to our major statistical programs, and, on the research front, we have a sizable agenda of issues related to economic measurement.

I would now like to discuss how these activities relate to each other and how they relate to the policymaking process. And I would like to start by mentioning efforts to develop and understand economic data. Good models require good data—that alone would be reason enough to devote effort to economic measurement. There are, of course, statistical approaches that can be implemented to deal with shortcomings in the data, but these are weak substitutes for making progress on measurement.

Another reason for attention to the data is that, given the shortcomings of the econometric models, our principals have considerable interest in our “getting the facts right.” Thus, we devote effort to understanding how the data are constructed and to how different measurements of similar concepts compare with each other. Few economic statistics can be taken as truth, and we are constantly looking for tensions among the economic statistics that might cast some light on competing hypotheses for economic developments. In the end, we also hope our attention to details of the data are important complements to our modeling efforts.

While improving measurement is a prominent activity of the staff, one way or another, economic modeling is the central activity of most macroeconomists at the Board. In that regard, I would say we come very close to Adrian Pagan’s characterization of being “the very model of a modern central bank.” We devote considerable energy to the development and maintenance of a core model—in our case FRB/US. However, a great deal of modeling work takes place outside

that core model, and I strongly subscribe to the “suite of models” approach for reasons that I will discuss in a few moments.

But the core model is central in our policy work. As we see it, there are several advantages to taking seriously efforts to develop the core model. The first is that it plays a key role as the intellectual framework around which the staff organizes its thinking about economic developments. While recognizing all of the attendant uncertainties, we have to take a stand on the structure of the economy. In that regard, the FRB/US model provides an important degree of transparency in our communications with the policymakers and with other system economists. They can examine our model-based analysis and determine whether or not they are persuaded, and if not whether their problems lie in the assumptions going into our model or in the basic characterization of the economy embedded in our model.

On the issue of estimation versus calibration, we have chosen to pursue econometric estimates of structural parameters of our large-scale model. We recognized that taking the estimation approach would require a significant investment of labor, both up front and on an ongoing basis. But we viewed those costs as worth bearing in order to have a model that can be judged against the data by rigorous measures of goodness of fit.

With respect to large versus small, our core model is large. And given the range of questions we are asked by policymakers, it is difficult to envision getting by solely on a much smaller model. To be sure, some questions that we are asked to address are not critical to the mission of the central bank. But I think the reasons for the size and complexity of our model are deeper than that. We and the policymakers are often seeking conformation or contradiction of

our explanation of economic events by looking for evidence in a wide variety of behaviors. Large models can be helpful in identifying shocks and dealing with structural change.

A recent example was the acceleration of labor productivity in the second half of the 1990s. Our univariate models of labor productivity went off track in the late 1990s. But if one were stuck on a desert island with just the residuals of those models, identifying any structural break would have been agonizingly difficult. In the context of FRB/US, we saw a pattern of residuals over time that could be ameliorated if one assumed that structural productivity was higher than we were then estimating: on the supply side, inflation was low relative to resource utilization, but despite low inflation, corporate profits were strong; on the demand side, business investment was increasing rapidly, and so was spending in the household sector on housing and durable goods. Moreover, equity prices were rising sharply. Taken together, a pattern of residuals across multiple equations provided support—though still not confirmation—that an improvement in structural productivity had taken place. So a reasonably rich structure is one way that we can probe the weak spots in our explanation of unfolding events.

Of course, there are some significant shortcomings that can be associated with the use of a large-scale structural macroeconomic model. For one, the very size and complexity that we find useful in answering the wide variety of questions we are asked to analyze and for gauging the reasonableness of model-based economic analysis carries with it some downsides as well. There can be a thin line between the desire for completeness of the model and overfitting the data. One hopes that tendency is curbed somewhat by the fact that those of us who use these models in the forecasting process will be around long enough to pay the penalty in terms of forecasting accuracy of that is imposed by overfitting. But the risk is still there.

I noted with some amusement and sympathy the comment by Adrian Pagan about his admiration and suspicion upon hearing very complicated explanations of perverse results from experiments using large models. If one's model leads frequently to this situation, the benefits of transparency provided by the use a core model will be eroded. I would note that complicated explanations of puzzling model results is not the sole problem of large-scale structural modelers; I have heard strained explanations from modelers of virtually all persuasions. I have also heard many good modelers point out perverse results as reflecting shortcomings of their models.

Another problem with using a structural macroeconometric model estimated on historical data is that it seems sensible to have doubts about the usefulness of such a model in conducting analysis of policy experiments much outside the bounds of past historical experience. Despite our best efforts to separate expectational effects from costs of adjustment in FRB/US, we cannot have great confidence in our success in that effort. For most of the analysis we do, this may not be a significant problem. But I seriously doubt that, for example, the current version of FRB/US would be of much use in predicting economic performance say during a hyperinflation or for that matter a persistent deflation.

So how do we deal with these drawbacks? As I noted earlier, one important way is that we are engaged in a great deal of modeling at the Board that extends well beyond FRB/US. Adrian Pagan speculated that this tendency to look at a "suite of models" was motivated more by a desire to keep the research staff happy than by some deeper analytical purpose. I can assure him that, because keeping a research staff of 120 Ph.D. economists happy is not in the feasible set, there must be other motivations. One motivation for looking at alternative models is that in some aspects of our work—such as forecasting—we take very seriously efforts to probe the

vulnerabilities posed by model uncertainty. Stochastic simulation of the core model, even allowing for parameter uncertainty in addition to intercept uncertainty, just does not provide a complete picture of how the world could evolve differently under either minor or major changes in the specification of key behavioral relationships. Indeed, in part because of our attention to issues of model uncertainty, the baseline forecast produced by the staff at the Board is a judgmental one, in the sense that it is not developed directly from runs of our large-scale model. We look at models both that embed minor tweaks to a similar specification, and we look at models that involve considerable departures from the core model in terms of explanatory variables or underlying theoretical structure. We also are engaged in a constant struggle to determine whether the errors we observe in the core model are a reflection of random noise, structural change, or the possibility that we are working with the wrong model. Other estimation techniques and other models can be used productively to gather evidence on those questions.

We use small-scale econometric models, such as VARs, as a check on the results derived from our large model. To be sure, congruence of results from the two approaches does not necessarily imply accuracy, just as difference does not necessarily signal a problem with the large model. But I think such comparisons can provide useful information about the degree of comfort one can feel with the robustness of the answers provided by large and complex models.

In addition to econometric models, we use small calibrated models to provide guidance on some policy issues where estimated models are likely to be less useful. In recent months, there have been several policy proposals to enact a temporary investment incentive. The temporary nature of the incentive and only partial coverage across different types of capital goods led us to explore a variety of modeling approaches outside of FRB/US. It seemed reasonable to

consult putty-putty models and putty-clay models and to examine the sensitivity of the analysis to assumptions about the service life of different types of capital equipment and to associated movements in the relative price of these types of equipment. Small calibrated models can also be useful when exploring policy experiments that lie well outside the historical experience over the estimation period. It simply is not reasonable to expect one model to deliver all of the analysis that will be necessary for purposes of policy analysis. What constitutes a good model will depend on the questions being asked of it.

Another important use of small models at the Fed is for proto-typing specifications and structures that would be too difficult and expensive to incorporate in FRB/US without a reasonable sense that such alterations are warranted and useful. Those models often play a role in our policy work long before they become formal features of the core model. The work of Jeff Fuhrer and George Moore on inflation modeling nearly a decade ago is one such example.

This raises a more general point. It is quite natural for there almost always to exist a constructive tension between the current specification of the model and the staff's evolving understanding of macroeconomic behavior. I would conjecture that the important structural features of large models, for the most part, change more discretely than our understanding of the economy. So use of models beyond the core models at central banks is, in part, a reflection of the exploration and discovery process.

Let me conclude with a few observations on how models are used in the policy process. We use our large-scale model intensively in the policy process. It is used as an input into the forecasting process. It is used in a variety of "what if" scenarios and to explore the probability and consequences of risks faced by the policymakers. And we use the model to examine

alternative strategies for monetary policy. It is my sense that policymakers take these analyses seriously. But that is a far cry from saying that they accept model results uncritically or without asking for or providing considerable additional information or analysis that lies well outside our large-scale models. I must admit, in providing model-based analysis to monetary policymakers, I have often experienced what I imagine are the mixed emotions of an assistant football coach who calls in a play to the star quarterback, who proceeds to ignore the recommended play but still manages to pass the ball for touchdown. We collectively have a great deal of work to do to improve our models to make them more useful in the policymaking process. One of the most important attributes of a useful model is that it helps us to think clearly about the problem at hand. The work we have heard about over the past two days makes an important contribution to that effort.