CHRISTOPHER A. SIMS
Princeton University

Monetary Policy Models

Some History

I HAVE WRITTEN SEVERAL Brookings Papers looking at the relation of multiple-equation economic models to the process of monetary policymaking.[1] When the first of these papers was written, the impact of the rational expectations critique in undermining academic interest in quantitative modeling for monetary policy was apparent. Many, maybe most, economists took the Lucas critique to imply that the month-to-month business of choosing monetary policy actions in the light of current information was trivial or irrelevant. Economists were thought to have nothing useful to say about it. They were supposed to contemplate only the choice of policy "rules," which were modeled as functions mapping the state of the economy into policy actions.

The main point of that paper was that the regularly recurring task of choosing policy actions was neither easy nor unimportant, and indeed that there is no other form of policy choice—"rules," if they can be changed, are themselves policy actions. The paper suggested methods for using a reliable probability model to evaluate conditional projections and applied these methods to a vector autoregression (VAR) model to determine that the then-current policy projections by the Council of Economic Advisers made no sense. But it provided little constructive criticism of the models then in use for policy projections.

1. Sims (1982, 2002); Leeper, Sims, and Zha (1996).

Central bank modelers by then had the idea that, to get academic respect, they should build rational expectations into their models. However, the VAR modeling style displayed in my 1982 paper provided few hooks on which to hang rational expectations. Some central banks and regional Federal Reserve banks estimated VAR forecasting models, but nowhere did such models become the central modeling tool for policy discussions.

Fourteen years later I co-wrote with Eric Leeper and Tao Zha another Brookings Paper on monetary policy models.[2] By then a substantial literature modeling monetary policy and its effects with *structural* VARs (SVARs) had arisen. The robust findings of that literature were that

—Monetary policy in most countries and periods is best modeled as an interest rate–setting rule.

—Most variation in monetary policy instruments consists of systematic reaction to the state of the economy. Random disturbances to monetary policy existed, but they explained little of the observed business cycle variation.

—Output responds with a lag, and prices with an even longer lag, to monetary policy actions. The shapes of these estimated responses conditioned policy discussion and were used as calibration targets by non-VAR modelers.

Our paper surveyed the SVAR literature and suggested by example how SVARs could be expanded to a scale closer to that of central bank policy models. The paper still provided no hooks on which to hang Lucas critique repellent, however, and it made no connection to what was actually going on in central bank modeling, which was not SVAR based. SVARs and VARs were used as auxiliary tools in many central banks, but nowhere had they become the central focus for policy discussion.

In 2002, I visited four central banks and interviewed research staff as well as a few policy board members. The banks' models, which were in regular use as part of the month-by-month routine of preparing forecasts and "scenario" analysis, were incorporating Lucas critique repellent, but at the expense of abandoning any claim to being probability models of the data. In the Brookings Paper that came out of those interviews,[3] I criticized this new generation of central bank models, but I also criticized the aca-

2. Leeper, Sims, and Zha (1996).
3. Sims (2002).

demic econometric and macroeconomic literature, which took no interest in policy modeling and had little guidance to offer policy modelers. The paper argued that getting back to credible probability models was feasible and important, and indeed the only way to allow clear discussion of uncertain contingencies in a way relevant to decisionmaking. At the end of the paper I pointed to some promising developments, including a paper by economists at the European Central Bank that demonstrated the feasibility of constructing a dynamic stochastic general equilibrium (DSGE) model for monetary policy and producing a distribution over the uncertain values of the model parameters (a uniquely Bayesian notion).[4]

It is perhaps worth restating what it means to say that the models currently in use have abandoned any claim to being probability models of the data. These models are presented with coefficients estimated from data and with standard errors on these estimates. But the estimates are obtained one equation at a time, or in some cases for small subsystems of two or three equations. The Federal Reserve Board's FRBUS model, for example, applies a standard single-equation specification to many variables that assumes no within-quarter interactions among variables. There are nonetheless some contemporaneous cross-variable effects (for example, of current income on nondurables consumption) that imply joint determination of blocks of variables, and this is ignored in the estimation. The model does imply a joint distribution for the time-series data, but this distribution is not calculated (at least in published descriptions of the model that I have been able to find), and its match to the data is not examined. The main publication describing BEQM, the new Bank of England quarterly model,[5] does not indicate how the model could imply a distribution for the data; it postulates an unobservable "core," to which observable variables are related by estimated equations, but the probability model for the core is not laid out. A subsequent paper suggests how BEQM could be treated as a probability model for the data but does not actually do so.[6]

There are two related reasons why it is important that policy models be probability models. One is scientific and was laid out years ago by Trygve Haavelmo:[7] Economic models will make forecast errors; unless they are probability models, one has no objective way to assess their accuracy,

4. Smets and Wouters (2003).
5. Harrison and others (2005).
6. Alvarez-Lois and others (2005).
7. Haavelmo (1944).

compare them with other models, or improve them based on experience. The other is decision-theoretic: economists estimate these models in order to use the results as aids to decisionmaking; policymakers need to weigh the implications of the model against their own knowledge and judgment, and this will necessarily involve weighting model results by their reliability. A probability model provides its own characterization of its reliability.

Of course, monetary policy has been made without the aid of probability models, and nonprobability models have been constructed and used as accounting frameworks for organizing policy discussion. Just as firms can allocate resources internally without calculating the shadow price of every input at every moment, so decisionmakers can make choices without an elaborate probability framework. But when reasonably large groups of people, such as central bank staffs and policy boards, need to talk about uncertainty and relate large amounts of current and past data to the current state of their uncertainty, the language of probability is the only clear means of communication.

The Bayesian approach to inference pioneered by Frank Smets and Raf Wouters is central to the reintegration of policy modeling with probability-based inference. It recognizes the need for what has to go on daily in data analysis for policymaking: the combination of information in the data with uncertain judgmental information from decisionmakers. It also can deal in a straightforward way with situations where the number of unknown parameters is large, so that some are likely to be poorly pinned down by the data. Although these advantages of a Bayesian approach to inference have long been understood, recent progress in computational hardware and algorithmic designs has made applying the approach much easier.[8]

Today

The Bank of England's BEQM model, which was still a research project in 2002, is now in active use as a policy tool. It is in the style of the Canadian QPM and the U.S. FRBUS models in that it can run in a "rational expectations" mode for scenario analysis, and in that it abandons any attempt to be a probability model for the data. But it is probably the last of

8. A somewhat more expansive discussion of the advantages of explicit Bayesian models for monetary policy appears in Sims (forthcoming).

its breed. The European Central Bank, the Federal Reserve Board, several regional Federal Reserve banks, and the International Monetary Fund are among the policy institutions that have active projects to produce Bayesian DSGEs that can be used in the policy process. The Swedish Riksbank has a working Bayesian DSGE model, labeled RAMSES, which bank staff described in a 2005 publication and began in 2007 to use as the central model in their regular rounds of policy discussion.[9] RAMSES is fit to fifteen data series and has twenty-one distinct sources of random disturbance and fifty-one parameters. Although this is not at the scale of the stochastic core of FRBUS, which has about forty distinct sources of disturbance, it suggests that, with its much larger staff and computing resources, the Federal Reserve should be able to apply these methods at the scale of FRBUS. There is a trend, in other words, toward bringing probability modeling and policy modeling back together.

Certain fundamentals are driving this trend. In 2002, I pointed to the spread of inflation targeting and of transparency in the policy process as generating a demand for probability-based policy models. When policymakers are required, as they are in most inflation-targeting banks, to prepare regular, policy board–approved projections of inflation and business activity, they want model-based projections from their staff. These model-based projections may be used mainly as a starting point for discussion, but they provide a necessary focal point. Policymakers also want to be sure that the projections include error bands, so that readers of the inflation report will expect revisions of forecasts and not treat deviations from forecast paths of expected absolute size as forecast failures. This leads to an appreciation of the value of models that treat parameters explicitly as random and that can be assessed from a historical record of forecast distributions and actual outcomes.

The error bands that are published now, even at the Riksbank, are based primarily on the historical record of the bank's forecast accuracy, rather than the model's internally generated forecast accuracy measures. The central forecasts themselves are not simply those produced by the model but reflect judgmental input from the policy board, so that model-based error bands would necessarily be only a starting point for determining the bands in the published fan charts. Nonetheless, model-based analysis of forecast uncertainty could provide important insights not available from

9. Adolfson and others (2005).

the usual use of historical data on forecast errors. Model-based analysis can answer questions such as, "What is the probability of two successive negative quarterly growth rates of GDP in the next year?" or a similar question about consumer price inflation. Depending on the initial state of the economy, different parts of the model may be especially important for determining forecast uncertainty; model-based error bands can take account of the effects of such variations on uncertainty. And finally, models can put error bands on projections that are conditional on particular policies or future disturbances, which is not possible with a historical approach. It seems likely, therefore, that as experience with the RAMSES model and its properties accumulates, its model-based error measures may play a more central role.

The Federal Reserve is, of course, one of the central banks that still have not formally adopted inflation targeting. From the point of view of the developments described here, however, inflation targeting is important more for the nature of the policy process it induces than for the numerical inflation target itself. The Federal Reserve has for a long time provided semiannual projections of inflation and output growth generated from individual forecasts of Federal Open Market Committee (FOMC) members. The present Federal Reserve chairman, Ben Bernanke, has put more emphasis than did his predecessor on these projections in his congressional testimony, and he recently announced that the projections will now be announced quarterly, over a longer horizon, and with explanations for the diversity of views they represent. Although these projections are still only for annual averages, this brings the Federal Reserve's procedures close to those of inflation-targeting banks. Because the projections will continue to be based on individual FOMC member forecasts, the incentives to produce better, probabilistic models may work their way back to individual regional Federal Reserve banks, whose presidents serve on the FOMC.

Inflation targeting and the associated cycle of forecast revision and preparation and inflation report writing are now widespread. The current frontier of controversy is probably whether central banks should publish forecasts of their policy interest rates along with their forecasts of inflation and output. Just a few central banks—those of New Zealand, Norway, and Sweden—publish their policy rate forecasts today, but there is a good case for publishing them. In most situations where strong policy action is called for, the central bank will be planning a nonconstant path for the policy rate.

If inflation is threatening, the bank will raise the policy rate, expecting to be able to lower it again to normal levels when inflation recedes. If it has announced an expected path for the rate, there is less danger that a foreseen, normal rate reduction as inflation recedes will be misread as a loss of nerve. In a recession, conversely, the bank will lower the policy rate, expecting, and wanting the public to believe, that it will raise rates as recovery proceeds, to preclude inflation. Realistic projections of output growth and inflation will assume such time paths for the policy rate. A full explanation of the forecast, and even more important, of the reasons forecasts are changing in reaction to events, requires that the policy rate path be explicit.

One objection to publishing policy rate forecasts is the possibility that, despite their being presented as fan charts and with explicit warning that they are likely to be revised, the public might mistakenly regard them as fixed commitments, with the result that the central bank loses credibility when it does not exactly follow the policy rate path it has projected. In those countries that have started publishing policy rate forecasts, this has not been a problem so far. The central bank could lose credibility if it changed its policy rate projections without explanation, or with inconsistent or contradictory explanations, but this is exactly the appeal of making policy rate forecasts public. It helps to discipline the central bank and at the same time helps the public distinguish policy changes made as part of a systematic reaction to the state of the economy from any possible erratic component in policy.

In the United States the objection is made that the FOMC, which has many more members than the policy boards at the central banks that publish policy rate forecasts, would be unable to agree on a path for the policy rate, because that implies they would have to agree on dozens of numbers instead of the single number that represents the current policy rate. This objection is probably overstated. Discussion is likely to focus on a few dimensions of variation in the path, mainly whether a given increase or decrease in the rate should be more or less rapid, not on the month-by-month detail. The complexity of the decisionmaking would therefore be only modestly increased. In any case the procedure already in place to develop summaries of the collection of projections from individual FOMC members could easily be extended to include federal funds rate forecasts.

The Rules Versus Discretion Debate Revisited

As I pointed out at the start, part of the reason that academic research on policy models shriveled was the notion that only policy "rules" matter, so that the normal business of monetary policy, namely, changing policy variables in reaction to the state of the economy, came to seem unimportant. But implementation of a rule is a nontrivial task. Particularly nontrivial is the problem of ensuring that the public understands the rule and believes it will be persistent.

Figure 1 shows the main fan charts from a recent *Monetary Policy Report* of the Swedish Riksbank. The report also includes a variety of alternative projections, conditioned on tighter or looser monetary policy and on possible external disturbances, and explains why projections have changed since the previous report. Which is more likely to generate public confidence in and understanding of monetary policy:

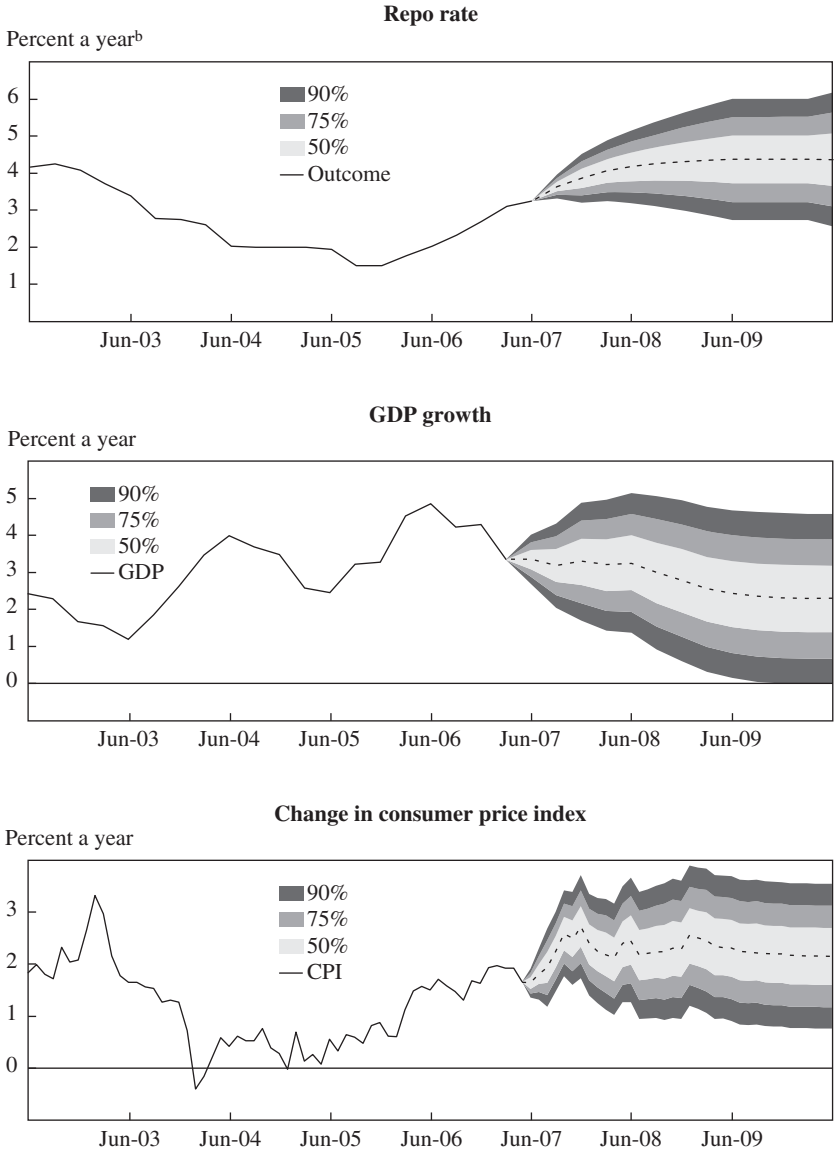
—An announced Taylor rule, with coefficients specified to four decimal places, or

—Experience with reports like the Riksbank thrice-yearly report, explaining current and future policy, showing paths not chosen as well as paths chosen, and explaining how projected paths of policy rates change over time and why they do so?

Formally, both are "rules" mapping current information onto policy actions. But the one specified indirectly seems more understandable and more likely to retain credibility when unusual conditions arise. The process of making policy projections, discussing them, choosing among them, and explaining that choice probably results in a more stable and credible policy "rule" than rules specifying $k\%$ growth in some monetary aggregate or fixed reaction functions of the sort that economists usually have in mind when they talk of policy rules.

The original argument for focusing on policy rules seems to have turned inside out. By being more careful, precise, forward-looking, and transparent about the ordinary business of monetary policy—changing policy variables in reaction to the state of the economy—central banks seem to be coming close to the ideal of implementing a widely understood and stable policy rule.

Figure 1. Main Forecast Charts from the Swedish Riksbank's *Monetary Policy Report*[a]



Source: Sveriges Riksbank, *Monetary Policy Report* 2007:2 (June), using data from Sveriges Riksbank and Statistics Sweden.
a. Solid lines indicate actual outcomes; dashed lines and shading indicate forecasts with associated uncertainty bands.
b. Data are quarterly averages.

Future Prospects

Although the new style of DSGE models fit to data is a major step forward, there is plenty of room for further progress. The modeling technology that has now been widely mastered deals with *linearized* models. For long-run projections, or for analysis of situations where the lower bound on nominal interest rates could be binding, for example, accounting for nonlinearities could be important. Linearized models cannot explain time-varying risk premia, which are essential if the model is to incorporate a rich set of asset markets. Methods for inference in nonlinear DSGE models are for now many times more burdensome computationally and not practical for real-time policy modeling, but they are an important research frontier.

Central bank modelers, having seen a specification that in some sense works, are imitating each other, but some of the most common elements of the models are questionable. The models generate price or wage stickiness, or both, from a small bag of stylized New Keynesian tricks. These are "micro-founded" specifications that do begin with optimizing theory, but they cannot be connected realistically to micro data. To fit well, the models have to introduce a lot of inertia—"habit formation" in consumption and adjustment costs in investment, for example. Many economists regard these models of inertia as thinly disguised empirical adjustments that are not plausible as actual descriptions of tastes or technology. It would be good to see some competition among models that use different ways of accounting for observed sluggishness in macroeconomic behavior, although it seems likely that different models of inertia will fit the data about equally well despite having different implications about the welfare effects of monetary policy. Some of the criticism of empirical DSGEs on this score fails to recognize that using a model that does not account for observed sluggishness is not an option.

The models in most cases include adjustments to some optimization conditions, most prominently the uncovered interest parity (UIP) condition. For an open economy like Sweden, the UIP condition is crucial to the model dynamics. RAMSES makes one of these ad hoc adjustments. It improves the model's performance in various dimensions, but it raises questions about the model's interpretation. Deviations from UIP are treated as affected by interest rates but as unrelated to other parts of the economy. If the deviations were in fact "risk premia" (which is what they are called), one might expect that they would increase at the same time that other risk

premia do. There is even some work by Harald Hau and Hélène Rey suggesting that this might be the case.[10] Ignoring the cross-dependence would be inaccurate, but including it creates a major channel of impact of monetary policy that runs outside the general equilibrium modeling framework.

The ad hoc inertia and UIP shocks may be symptomatic of a broader problem: models in this style, in which aggregates are treated as if they were single homogeneous commodities and all agents optimize continuously and perfectly, may in these empirical DSGEs be reaching the limits of their range of usefulness. It might be better to recognize that the stories the models tell about optimizing agents are only useful approximations. One common way to do this is to treat the observed data as the DSGE model variables plus "error" that is unrelated to the economically interpretable disturbances that drive the model. This approach, however, leaves one with no principles to guide assumptions about the properties of the "error," and furthermore it leaves policy actions modeled as affecting unobserved model variables, not the actual variables that the public and policymakers care about.

A more useful, although similarly motivated, idea is to use the DSGE model as a way to generate probabilistic restrictions on the behavior of a model in the structural VAR style. The VAR-style model applies to the actual data, and its equations and disturbances have behavioral interpretations. But a prior distribution pulls the VAR-style model toward the shape of the linearized DSGE model, which has similarly interpreted equations and disturbances. In this setting the DSGE is freed from having to match in detail the inertial aspects of short-run behavior, while at the same time it can pull the VAR-style model toward reasonable long-run behavior. Beth Ingram and Charles Whiteman proposed a similar idea, applied to nonstructural VARs, a long time ago.[11] Marco Del Negro and coauthors have made the idea practical for SVARs and for models at the scale of the recent empirical DSGEs.[12] This approach seems promising and deserving of extension.

So there is plenty of difficult and interesting research ahead in this area. But the intellectual sociology that has in the past led academic macroeconomists to find reasons not to be interested in policy-oriented research is still in operation. Some academic attention has been paid to these issues, but it seems to be less than what is warranted by their importance and

10. Hau and Rey (2003).
11. Ingram and Whiteman (1994).
12. Del Negro and others (2007).

inherent interest. It may be that the field requires more investment in technique and more teamwork than most, making it relatively less attractive to the nontenured researcher needing a "home run" paper. Central bank research staffs work under time pressure and have to focus mainly on known technology. They also may develop vested interests in the methods and models they know. Interest and criticism from outsiders are therefore important. Sustaining them may require developing new institutions of cooperation between academics and central banks.

Conclusion

Monetary policy institutions in which policymakers and modelers regularly interact, using probability and decision-theoretic language, seem for the time being to be spreading. I have explained that there are reasons to think this way of doing policy really is better, and therefore might persist and spread further. This is not inevitable, however, for several reasons:

—Economics faculties could lose interest and stop producing people who are well trained in inference and data analysis as well as in economics.

—A central bank at the forefront of the new methods could produce, or be blamed for, a bad macroeconomic outcome.

—A lot of decisions are made under uncertainty every day by people who do not even know what a probability is. It could turn out that having a highly trained staff who can discuss uncertainty in probability language is not enough help to be worth the expense.

Whether any of these will prevent the further evolution of new institutions and new models for monetary policymaking remains to be seen.

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General Discussion

Lawrence Summers agreed that transparency about monetary policy decisions is superior to a fixed mathematical feedback rule, but he thought that neither approach is the best available. He worried that there might be substantial benefits from a central bank not demonstrating "extreme fallibility," which is almost inevitable if the bank is too open about its forecasts. For example, if the Federal Reserve's projections of its own intentions differ significantly from market expectations, that might imply a lack of credibility on the central bank's part. It could increase its credibility by following through on its own projections, but this would come at the expense of enacting optimal policy on a moment-by-moment basis. Sims replied that he did not propose straightjacketing future discretion: projections and policies should certainly be revised, but these revisions should be somewhat predictable.

Gregory Mankiw added that the academic literature sides with Sims at the moment. If the chairman of the Federal Reserve has a certain view of the future, there is no benefit to keeping it secret, according to the current consensus. Mankiw thought it would be challenging to develop a model in which the Federal Reserve's economic or policy outlook must be kept from the public in order to yield optimal outcomes.

However, Mankiw argued, some practical considerations warn against the use of large models as advocated by Sims. In the United States, monetary policy is determined by a committee, not an individual, and it is much more difficult for a committee to agree on a vector of policy variables than on a single variable. Sims conceded that the Federal Reserve's policymaking committee is much larger than its Swedish counterpart, which consists of four people, and thus policy is more difficult to coordinate here than in Sweden. Still, he did not think the quantity of variables to be discussed

would be problematic: Discussions tend to focus on just two variables, the level of the interest rate and the slope of its trajectory.

William Nordhaus noted that the Federal Reserve's approach to monetary policy has become much more transparent over time, much like the approach in Scandinavian countries. Still, he recalled the many questions that people raised about the Federal Reserve's actions in relation to the asset price bubble around 2000, noting that just because the central bank has been competently managed in recent decades, there is no reason to assume this will be true forever. Requiring the Federal Reserve to explain its actions may prevent future catastrophic behavior. He suggested that it may not be necessary to announce the entire expected future path of the funds rate, but there is still room for improvement.

Sims agreed that a complete move to the Swedish model might not be realistic, but his emphasis on the Riksbank was intended to make the position Nordhaus had just described appear comparatively centrist. Benjamin Friedman added that Norway's population and GDP are approximately identical to Alabama's. He did not think it obvious that a country like the United States, whose central bank sets the price of instruments that are crucial to markets all over the world, has the latitude to do what a country with the same modest influence in world financial systems as Norway can.