Monetary Policy Models

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1 Some History

I have written several papers for BPEA (2002; 1996; 1982) looking at the relation of multiple equation quantitative economic models to the process of monetary policy making. 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. Economists 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 the 1982 paper was that the regularly recurring task of choos-

ing 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 to use a reliable probability model to evaluate conditional projections, and applied the methods to a 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 projection.

Central bank modelers by then had the idea that, to get academic respect, they should build rational expectations into their models. The VAR modeling style displayed in the 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 they become the central modeling tool for policy discussions.

Fourteen years later, I co-authored with Eric Leeper and Tao Zha another paper on monetary policy models. By then, a substantial literature modeling monetary policy and its effects with *structural* VAR's 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 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.

The 1996 surveyed the SVAR literature and suggested by example how SVAR's 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, though, and it made no connection to what was actually going on in central bank modeling, which was not SVAR based. SVAR's and VAR's were used as auxiliary tools in many central banks, but they nowhere became the central focus for policy discussion.

In 2002 I visited four central banks and interviewed research staff, plus 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 *BPEA* paper that came out of the interviews, I criticized this new generation of central bank models, but I also criticized the academic econometric and macro 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. It is the only way to allow clear discussion of uncertain contingencies in a way relevant to decision-making. At the end of the paper I pointed to some promising developments, including a paper out of the ECB (Smets and Wouters, 2003) that demonstrated the feasibility of constructing a monetary policy dynamic stochastic general equilibrium (DSGE) model and producing a distribution over the uncertain values of the model parameters (a uniquely Bayesian notion).

It is perhaps worth restating what it means to say that these models have aban-

doned any claim to being probability models of the data. The 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. In the Federal Reserve board's FRBUS model, for example, there is a standard single-equation specification applied to many variables that assumes no within-quarter interactions among variables. There are nonetheless some contemporaneous cross-variable effects (e.g. current income on non-durable consumption) that imply joint determination of blocks of variables, and this is ignored in 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 not examined. The main publication (Harrison, Nikolov, Quinn, Ramsay, Scott, and Thomas, 2005) describing BEQM, the new Bank of England model, 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 (Alvarez-Lois, Harrison, Piscitelli, and Scott, 2005) suggests how BEQM could be treated as a probability model for the data, but does not actually do so.

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 Haavelmo (1944): Economic models will make forecast errors; unless they are probability models, we have no objective way to assess their accuracy, compare them to other models, or improve them based on experience. The other is decision-theoretic: We estimate these models to use them as aids to decision-making; policy-makers need to weigh

the implications of the model against their own knowledge and judgment; 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 non-probability 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, decision-makers can make choices without an elaborate probability framework. But when reasonably large groups of people, like 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 Smets and 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 policy-making — combination of information in the data with uncertain judgmental information from decision-makers. 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. While these advantages of a Bayesian approach to inference have long been understood, in recent years computational hardware and algorithmic designs have progressed to make applying the approach much easier. (A somewhat more expansive discussion of the advantages of explicit Bayesian models for monetary policy appears in my (2007) paper.)

2 Today

The Bank of England's *BEQM* model, which was still a research project in 2002, has emerged. It is in the style of the Canadian QPM and the US FRBUS, in that it can run in a "rational expectations" mode for scenario analysis, and also in that it abandons any attempt to be a probability model for the data. But it is probably the last of its breed. The ECB, the US Federal Reserve Board, several regional Feds and the IMF, are among the policy institutions that have active projects to produce Bayesian DSGE's that can be used in the policy process. The Swedish Riksbank has a working Bayesian DSGE model, labeled RAMSES, that they described in 2005 publications and began in 2007 to use as the central model in their regular rounds of policy discussion. RAMSES is fit to 15 data series, has 21 distinct sources of random disturbance, and 51 parameters. While this is not at the scale of the stochastic core of FRBUS, which has about 40 distinct sources of disturbance, it suggests that with its much larger staff and computing resources, it is reasonable to suppose the Fed could apply these methods at the scale of FRBUS. There is a trend, in other words, toward bringing probability modeling and policy modeling back together.

There are some fundamentals 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 policy makers 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. The model-based projections may be used mainly as a starting point for discussion, but they provide a necessary focal point. Policy makers also want to be

sure that 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 an 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, not 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 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 the usual use of historical data on forecast errors. Model-based analysis can answer questions like, "What is the probability of two successive negative quarterly growth rates of GDP in the next year?", or the same question about CPI inflation. Depending on the initial state of the economy, different parts of the model may be 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 condition 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 US Federal Reserve is of course one of the central banks that still has 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 presence of a numerical inflation target. The Federal Reserve has for a long time provided semi-annual projections of inflation and output growth generated from individual forecasts of Federal Open Market Committee (FOMC) members. Chairman 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 made quarterly, over a longer horizon, and with explanations for the diversity of views they represent. Though these projections are still only for annual averages, this brings the Fed'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 will be serving on the FOMC.

Inflation targeting and the associated cycle of forecast revision and preparation and inflation-report-writing is now widespread. The current frontier of controversy is probably the question of whether central banks should publish forecasts of their policy interest rates along with their forecasts of inflation and output. Just a few central banks publish forecasts of their policy rates: New Zealand, Norway, and Sweden. There is a good case for publishing policy rate forecasts. In most situations where strong policy action is called for, the central bank will be planning a non-constant path for the interest rate. If inflation is threatening, the bank will raise the policy rate, expecting to be able to lower it to normal levels when inflation recedes. If it has announced an expected path for rates, there is less danger that a foreseen, normal rate reduction as inflation recedes will be misread as a loss of nerve. In a

recession, 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 so 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, so that the central bank would lose credibility when it did not exactly follow the policy rate path it had projected. In those countries that have started publishing policy rate forecasts, this is not so far a problem. 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. Making the forecasts public helps to discipline the central bank and 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 US, the objection is made that the FOMC is much bigger than the policy boards at the central banks that publish policy rate forecasts. It is said therefore to be impossible for the committee to agree on a path for the policy rate — dozens of numbers, instead of the single number that represents the current policy rate. This objection is probably overdone. Discussion is likely to focus on a few dimensions of variation in the path, mainly more or less rapid increase or decrease in the rate, not on month-by-month find detail of it. The complexity of the decision making is

therefore 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.

3 The rules vs. discretion debate revisited

As I pointed out at the start, part of the reason academic research on policy models shriveled was the notion that only policy "rules" matter, so that the normal business of monetary policy, changing policy variables in reaction to the state of the economy, came to seem to be unimportant.

Figure 1 shows the main fan charts from a recent Riksbank *Monetary Policy Report*. The report includes also 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 4 decimal places, or
- Experience with reports like the Riksbank semi-annual 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 into policy actions. But the one specified indirectly is more understandable, and more likely to retain credibility

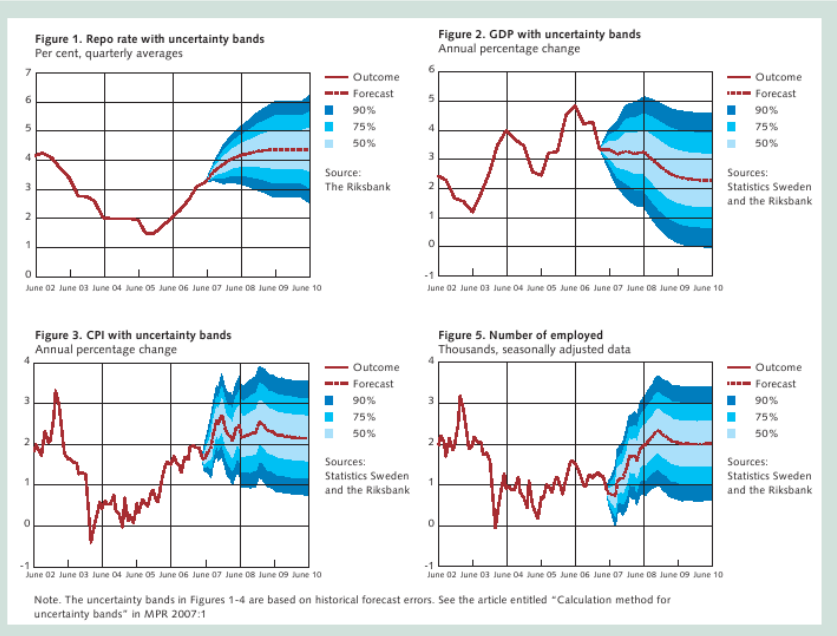

Figure 1: Main fan charts from the Riksbank's *Monetary Policy Report* 2007:1

when unusual conditions arise. The process of making policy projections, discussing them, choosing among them, and explaining the choice, is probably resulting in a more stable and credible policy "rule" than the k% rules or fixed reaction functions economists usually think of when they talk of a "policy rule".

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.

4 Future Prospects

Though the new style of DSGE models fit to data are 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 and/or wage stickiness from a small bag of

stylized New Keynesian tricks. These are "micro-founded" specifications that do begin with optimizing theory, but 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 with different ways of accounting for observed sluggishness in macroeconomic behavior, though it seems likely that different models of inertia will fit the data about equally well despite having different implications about welfare effects of monetary policy. Some of the criticism of empirical DSGE's 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 Hau and Rey (2003) suggesting that this might be the case. 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 DSGE's 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 DSGE. This approach, though, leaves us with no principles to guide assumptions about the properties of the "error", and furthermore leaves policy actions modeled as affecting unobserved model variables, not the actual variables that the public and policy-makers care about.

A more useful, though similarly motivated, idea is to use the DSGE 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. Ingram and Whiteman (1994) proposed a similar idea, applied to non-structural VAR's, a long time ago. DelNegro, Schorfheide, Smets, and Wouters (2006) have made the idea practical for SVAR's and for models at the scale of the recent empirical DSGE's. This approach seems promising and deserving of extension.

So there's 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. While there is some academic attention to these issues, there seems to be less than warranted by their importance and inherent interest. It may be the field requires more investment in technique and more teamwork than most, making it relatively less attractive to the non-tenured 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 is therefore important. Sustaining it may require developing new institutions of cooperation between academics and central banks.

5 Conclusion

Monetary policy institutions in which policy makers and modelers regularly interact, using probability and decision-theoretic language, seem for the time being to be spreading. I've 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, though.

- The Academy could lose interest, stop producing people who are trained well in inference and data analysis as well as 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 don't

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.

We'll see.

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