Thirty Five Years of Model Building for Monetary Policy Evaluation: Breakthroughs, Dark Ages, and a Renaissance

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One of the most important advances in monetary policy analysis in the past three decades has been the development and use of economy-wide econometric models that combine forward-looking rational expectations and sticky prices or wages. Such models are so commonplace now that the idea hardly deserves comment and indeed the structural models presented at this conference are no exception. But no such models existed at the time that the *Econometrics of Price Determination Conference* was held thirty five years ago. The paper by Robert Lucas (1972a) at that conference presented a rational expectations model, but it had perfectly flexible prices—neither time dependent price-setting, as in the future staggered contract models, nor state dependent price-setting, as in the future menu cost models. Other papers at the 1970 conference—still reflecting what was common in econometric macro models at the time—focused on backward-looking models of the wage-price dynamics featuring inflexible markups from wages to prices and adaptive expectations. Expectations of inflation, important for price determination following the Friedman-Phelps hypothesis, were therefore very slow to change unlike in the rational expectations models.

And those two separate strands continued to develop in parallel for years following the conference. One strand was formed by the follow-ups to the Lucas

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conference paper, including his “Expectations and the Neutrality of Money” paper (1972b), his Lucas critique paper (1976), and the policy ineffectiveness paper of Thomas Sargent and Neil Wallace (1975). The other strand was formed by the research work on price adjustment models for policy at the Federal Reserve Board—including the work estimating the wage-price block of the MPS model.

Like the gap between desired and actual decision variables in an (S, s) model, the gap between actual and desired models for monetary policy evaluation was growing. And as the gap grew, the need to close it in some way in order to do monetary policy analysis became clearer and clearer. What exactly was this need? First the Lucas critique presented a convincing case to many researchers that conventional policy analysis was flawed; the critique offered an alternative method of analysis using rational expectations to evaluate monetary policy rules, though most researchers seemed to ignore or misunderstand that alternative, or to think that the alternative was unattractive or too difficult to follow in practice.

Second, monetary policy evaluation required a realistic model of how monetary policy impacted inflation and the real economy, and that required introducing some form of inflexibility in price formation into the rational expectations models, which was not captured either by the rational expectations models with perfectly flexible prices or by the traditional econometric models with backward-looking adaptive expectations. That the form of monetary policy rules did not even matter in the existing rational expectations models, as shown by Sargent and Wallace (1975), made it virtually impossible to evaluate alternative monetary policy rules in those models as the Lucas critique had

suggested one should do.[2] And that the traditional models did not have forward-looking expectations made them highly susceptible to the Lucas critique.

Breakthroughs

With this pent up demand for economy-wide models combining sticky prices and rational expectations it is perhaps understandable that several breakthroughs occurred simultaneously. The paper by Fischer (1977) and the paper coauthored by Phelps (1977) and myself were some of the first responses. Gray's (1976) paper was another. That the editors of the *Journal of Political Economy* (including editor Robert Lucas) decided to publish the Fischer and the Phelps-Taylor papers back-to-back in the same issue in 1977 was an indication that many people saw the need to bring the strands together and were adjusting their research accordingly, trying to be one of the first to do so in healthy academic competition. These papers did combine some aspect of sticky prices with rational expectations. By assuming that prices or wages were set one period in advance they thwarted the monetary policy ineffectiveness proposition. More importantly they opened the possibility that the econometric policy evaluation techniques suggested by Lucas might be used in practice for monetary policy analysis.

But those first generation models were pretty crude, especially when you look back after three decades, and they could not come close to explaining the aggregate dynamics of inflation and output that the researchers in the other strand of work in traditional econometric models were pursuing. Because the models jumped back fully to the flexible wage-price equilibrium in one period, they could not generate much

[2] Another task was to introduce learning into the rational expectations models, which was a path that I first went down before finding how difficult monetary policy evaluation was with learning compared to the simple rational expectations assumption.

momentum. In fact, I remember trying to bring versions of the Phelps-Taylor model to the data and soon realized that something more realistic about price setting or wage setting was needed in order to increase the degree of persistence which was virtually nil in those early models.

That was what led me to the staggered wage setting model (Taylor, 1979) and my 1979 *Econometrica* paper, which, as Ben McCallum stated in his 1999 Homer Jones lecture, "demonstrated that these [econometric policy evaluation] techniques are entirely feasible." In other words you could do practical monetary policy analysis with forward-looking rational expectations. I was able to take the model in that 1979 paper to the data, calculate the optimal rule for the money supply using the methodology that Lucas had suggested—it was effectively a "Taylor rule," though for the money supply—and derived a trade off between the variability of inflation and output as an alternative to the old Phillips curve tradeoff.

A slew of papers in this area where written at this time trying to make things more realistic or more securely founded on economic principles. Julio Rotemberg (1982) showed how a firm's price adjustment decisions could be derived with monopolistic competition assumptions. Guillermo Calvo (1982) introduced his ingenious geometric random version of staggered price setting. Ray Fair and I (1983) worked on an algorithm that was needed to solve non-linear rational expectations models. Simulations with the models were performed and showed that the cost of disinflation was less than in the conventional models though more than in the Sargent-Wallace models. Other researchers used began using microeconomic data on union wage contracts to calibrate staggered wage setting models.

A Dark Age

But after this flurry of work in the late 1970s and early 1980s, a sort of "dark age" for this type of modeling began to set in. Ben McCallum (1999) discussed this phenomenon in his review lecture, and from the perspective of the history of economic thought, it is an interesting phenomenon. As he put it, there was "a long period during which there was a great falling off in the volume of sophisticated yet practical monetary policy analysis."

McCallum (1999) attributes this period in part to a misunderstanding of the Lucas critique by many researchers, namely that it was taken as a *negative* statement that policy analysis could not be done rather than a *positive* statement of how to do it. Hence, researchers in central banks did not immediately take to these new models and the traditional models of the Econometrics of Price Determination conference continued in use with little in the way of alternatives. I think McCallum is probably right, but if you actually read the Lucas critique paper, it is not hard to see that the clear methodology illustrated in his examples could be applied to other examples, including monetary policy evaluation with sticky prices and rational expectations. Another explanation given by McCallum for the dark ages for this kind of research was the take-off of research on real business cycle models which absorbed a lot of time and effort by macroeconomists who might otherwise have been applying the Lucas policy methodology to monetary policy. Of course, the real business cycle development was very important in its own right, but it did take emphasis away from new models for monetary policy evaluation.

There were of course, “keepers of the flame of the rational expectations models with sticky prices” during these dark ages, and for them it was probably not so dark. After all, inflation was plummeting in the United States and the cost, while high, was closer to the estimates of the rational expectations models with sticky prices than to the traditional models backward looking models. Economists at central banks continued to develop models of wage and price dynamics as it was their job to do, and they gradually began to incorporate rational expectations into the models. Svensson (1986) and Blanchard and Kiyotaki (1987) began to build *economy-wide* models with money and with slow price adjustment based on monopolistic competition. West (1988) showed that the unit root implied by many real business cycle models also was implied by the staggered contract models with rational expectations. At Stanford I worked, along with a number of outstanding graduate students (including many who are still very active in this area such as Joe Gagnon, Pete Klenow, Andrew Levin, Ellen McGrattan), on a large scale multi-country model with rational expectations and sticky wages and prices; this work was motivated by the increased globalization of the world’s financial markets and the need to address international monetary policy questions. And McCallum (1988) stressed the importance of robustness in econometric policy evaluation by doing such evaluations with several models.

A Renaissance

This all began to change in the late 1980s and the early 1990s. The renewed interest in policy evaluation models with sticky prices and rational expectations can be traced, I believe, to open economy macroeconomic issues and, in particular, to the spread

of work on developing larger scale international monetary policy models, such as the IMF Multimod model developed by Paul Masson, the Federal Reserve Board's MX3 model developed by Joseph Gagnon and Ralph Tryon, and the multi-country econometric model at Stanford that I previously mentioned. The reason that the renewed interest was originally focused on international models was that one could simply not econometrically analyze key international monetary policy issues like exchange rate policy without combining rational expectations and sticky prices or wages. Questions of fixed exchange rates versus flexible exchange rates are at their heart "regime issues," highly suitable for the new econometric evaluation approach. The Dornbusch (1976) paper on exchange rate over-shooting illustrated how to modernize the small open economy Mundell-Fleming by incorporating both sticky prices and forward-looking expectations. The international econometric model builders were following that same approach in a large economy multi-country setting.

Since evaluating exchange rate policy was naturally a question of evaluating regimes, one had no choice but to specify a policy rule for monetary policy; all the international models assumed something close to perfect capital mobility which meant that the exchange rate regime implied a monetary policy regime. Hence many policy people began looking at alternative monetary policy rules and they used these new international models with sticky prices and rational expectations to do so. The collection of papers in the book by Bryant, Hooper, and Mann (1993) is the best example of this research; it was from this econometric policy evaluation work that I gleaned a "model consensus" monetary policy rule, which later came to be called the Taylor rule.

Now I believe that this new focus on monetary policy rules in practical policy making in the 1990s—as evidenced, for example, by references in speeches to monetary policy rules by members of the Federal Reserve Board and in articles in central bank publications—in turn brought an even greater increased interest in monetary policy evaluation models with rational expectations and newer forms of sticky prices. In any case many papers were written about such models in the 1990s. The staggered price or wage setting assumption came under further scrutiny, with Ball (1994) and Fuhrer and Moore (1995) pointing out that these models—originally designed to give more persistence than the first generation models—themselves did not seem to provide enough persistence. Fuhrer and Moore proposed modifications of the staggered price setting models which were incorporated into modeling at the Federal Reserve, thereby paving the way for the use of rational expectations and more forward looking models in policy making.

With the research on policy rules in full force in the 1990s, many researchers began to try to build better models to evaluate monetary policy. A major advance occurred when the techniques of the real business cycle school began to show their influence in these policy evaluation models. King and Wolman (1999) and Chari, Kehoe, and McGrattan (2000) are excellent examples that demonstrate this influence; in these models staggered contracts and rational expectations are embedded in an otherwise real business cycle framework. The real business cycle techniques have the promise of improving identification of structural policy parameters (down to utility and production functions), though their empirical forms are very similar to those derived by West (1988); they still have many of the characteristics of the earlier models used for policy evaluation

and tend to yield similar results in policy simulations. The smaller versions of these models have been boiled down to three equations, and it is these three equation models that people now frequently refer to as the New Keynesian models, though that term has also been used to describe the entire class of models I am reviewing here.

In sum, I think that this short review makes it clear that tremendous progress has been made in developing econometric models for policy evaluation during the past three decades. One can get a simple measure of this progress by comparing the 1970 *Econometrics of Price Determination* conference with "post renaissance" conferences. For example, McCallum (1999) refers to the 1998 NBER conference and the 1998 Riksbank conference on monetary policy rules. Another measure of progress can be seen by comparing Woodford's (2003) book *Interest and Prices* with Don Patinkin's (1965) book *Money, Interest, and Prices*. Like the 1970 *Econometrics of Price Determination*, Patikin's book had no economy-wide models with both rational expectations and sticky prices and wages. In Woodford's book this type of model is the main model used for policy evaluation.

Further Progress in Model Development

While there has been plenty of progress to celebrate there is much more to do. The most attractive thing about the most recent models is that they can be estimated with microeconomic data, which helps to resolve identification issues inherent in aggregate data. The way that Golosov and Lucas (2005) use the data put together by Klenow, and Kryvtsov (2005) is a wonderful example. One of the interesting findings from Klenow and Kryvtsov is that variations in the number of prices changed each period are not a

major source of variance in inflation fluctuations compared with the size of the price changes. We already knew that the staggered price setting models of Calvo (1982) have this property since there is no variation in the average number of prices changed each period. Surprisingly this is also true in (S,s) models, because, after a large shock, say to the money supply, the increased number of large changes in prices is roughly offset by the smaller number of small changes in prices. Resolving this "observational equivalence" will require a more detailed look at the Klenow and Kryvtsov micro data.

If I had to give a list of criticisms of the recent work, it would start with the frequent abstraction from wage rigidities. There are important exceptions including work at the Federal Reserve Board by Erceg, Henderson, and Levin (2000), but the approach of the more recent Golosov-Lucas paper with perfectly flexible wages is more common. In my view, wage determination is still a source of inflation dynamics, though not in the same rigid ways as the models in the Econometrics of Price Determination Conference. While the (S,s) model makes sense for prices it does not seem accurate for the timing of most wage changes. I think that the study of wage determination needs to be put on the front research burner again. A very important task for future research is an exploration of microeconomic wage data in the BLS's Employment Cost Index, comparable to what Klenow and Kryvtsov (2005) are doing with the CPI. My guess is that the characteristics of the micro wage data will look much different from the price data. Moreover, the micro wage date will permit one to discriminate between different staggered wage setting models. As Levin, Onatski, Williams, and Williams (2005) show, the shape of the distribution of wage contracts in staggered wage setting models matters significantly for monetary policy.

Econometric policy evaluation models are still too rarely used for forecasting. In my view one of the best tests of a model is its ability to forecast, but most forecasting models are still of the reduced form variety, and only loosely connected with structural models, as the Stock and Watson paper at this conference illustrates. Efforts to use the models for forecasting—this will probably require more details for the simple three equation models—would be very worthwhile.

Impact on Actual Policy

What has been the impact on policy of the development of policy evaluation models described here? One might ask if it was influential in building the consensus that led to the end of the Great Inflation. Perhaps the finding that inflation could be reduced with less disruption than traditional models suggested helped reduce some policymakers' reluctance to take the steps to end inflation, but it seems to me that people supported Paul Volcker's disinflation efforts mainly because they were fed up with inflation.

Perhaps more relevant have been the simulations of alternative monetary policy rules. Clearly it would not have been possible to even consider such rules systematically if were it not for these models, but the impact of the policy rule research itself is more difficult to prove. Policy rule research has of course been useful in comparing policy in different countries and in different time periods, or to characterize good policy versus bad policy. It has enabled central bank staff's to work with interest rates as an instrument and to do so with an analytical framework. It has shown that certain monetary policy principles are important, such as reacting in a pre-emptive fashion to forecasted increases in inflation. It has shown that targeting asset prices can lead to poor results. And it has

been helpful anytime forward-looking expectations are important, as in formulating a communication policy during the recent "considerable period" or "measured pace" phases. It is hard to imagine these types of policy issues being analyzed rigorously without the models with rational expectations and sticky prices and wages.

Future Policy Issues

There are many important policy issues which these types of models or their successors can be called on to analyze in the future. Understanding the reason for the recent reduction in pass–through of exchange rates and energy prices is important for monetary policy; the models that incorporate sticky prices, market power, and rational expectations, such as Golosov and Lucas (2005), can be helpful here. Determining when appropriate "deviations" from policy rules should start and when they should end is another important practical topic. Finding better ways to incorporate asset prices into policy formulation is yet another. Nailing down the key reasons among the many conjectures for the recent improvements in output volatility is also important for future policy; in my Homer Jones Lecture (Taylor, 1998) I argued it was improvements in monetary policy, but many other explanations have been raised.

But whether the future policy issues are on this list of policy examples or not, it is important to keep future monetary research focused on policy issues. I think this brief history of econometric policy model development clearly illustrates that economic research is most exciting and productive when it is policy-driven.

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