

Panel Discussion – Macroeconomics and Reality, 25 Years Later

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If monetary policy is coherent – and I would never suggest that monetary policy is anything but perfectly coherent – the men and women who make policy must answer three fundamental questions.

- What objectives are policies aimed at achieving?
- What is the economic environment in which the policymaker is operating?
- What choices are appropriate to meeting the objectives given the environment?

I think that it is fair to say – and Chris Sims (2002) has said it -- that the approach to addressing these questions in the Federal Reserve System is not currently much different than what it has been for many decades. At the very least, it has not changed much in the decade and a half I have been involved in the process. (I'm going to address where we stand on all of this from my perspective as an employee of the Federal Reserve, though I am bound to say that the System has yet to endorse my views as their own.)

Objective setting has been the product of a mixed brew of empirical observation, hard-knocks schooling, and plain old-time religion. Of that list, I'd put the second – hard-knocks schooling – at the top. We did the inflation thing. We didn't like it. We flirted with the deflation thing. Didn't think we'd like that.

Not surprisingly perhaps, the exercise of characterizing the economic environment – the conditional forecasting piece, where the conditioning is on both the current state of the economy and a benchmark path for policy – has been more model-based than the objective-setting exercise. Most of you know from Chris' 2002 Brookings review that this is the exercise known as the Greenbook process, whose formal core is the

so-called FRB-US model, which is really just a simple New Keynesian model with a glandular condition.

The Board staff has worked hard to make FRB-US what we wish our policy models to be, that is a work horse model with a story to tell, good forecasting properties, and enough structure to answer the third important question: How would the world be different if the FOMC were to choose a policy path that differs from the benchmark assumed.

The problem is that FRB-US is large, complex, and few of us really understand the precise story that is being told. This is a consequence of the fact that more weight has historically been placed on the short-term forecasting use of the model than on its integrity as a framework for conducting counterfactual analysis. In fact, as Chris has noted, actual Greenbook forecasts represent judgmental assessments informed by FRB-US, supplemented substantially with separate small-scale sectoral models and pure introspection.

So this is still the state of play. To this date, we have yet to adopt, in the Federal Reserve System, an analytical framework that permits us to synthesize the analysis of objectives, forecasting, and counterfactual policy experiments. The good news is that I am convinced we are close to at least partially rectifying this situation, at least in principle.

A Call For Modest Expectations

Several years ago, we asked a group of our colleagues from all over the system to come to Cleveland and participate in a workshop to discuss what models they used in their own policy development process. There were several VAR-related models, no

DSGE-based models. At the time I was shocked about how little impact these models seemed to have in the advice-giving activities of the research departments that nonetheless dutifully continued to produce output from them.

The reason appeared to be that characterizing a benchmark from which policy counterfactuals can be conducted involves *two* necessary conditions. One is confidence that the any model on which these experiments are based have acceptable empirical properties. The VAR-based models that were in use at the time excelled along this dimension. But the second necessary condition is that forecasts be couched in terms of some compelling narrative. Although VAR forecasts might provide reasonable point estimates of, for example, the dynamic path of GDP (including confidence intervals about those estimates), their structures rarely provided the basis for storytelling. As the end product of policy advice is an articulated vision of how and why the economy might be expected to behave under a variety of alternative policy choices, this is a serious shortcoming.

Storytelling, on the other hand, is the strong suit of DSGE models. But if the impact of forecasts from models without tight theoretical structures has been minimal, the impact of DSGE models has been almost nonexistent. Among policymakers, of the two necessary conditions for taking a model seriously, being competitive in a forecasting horse race seems to be the really, really necessary condition.

The work typified by Smets and Wouters (2003), the son of Christiano, Eichenbaum, and Evans (2005), and the descendent of Chris Sims' work dating back to "Macro and Reality," demonstrates, convincingly to me, that DSGE models are finally in the game. It should surprise no one that I am partial to the methodology exemplified by

Christiano, Eichenbaum, and Evans: Using identified VARs to characterize the dynamic facts of the economy, and building theoretical constructs with the insistence that they plausibly match those facts. Smets and Wouters, and Andersson, Linde, Villani, and Vredin (2005), and others have shown that these constructs are not only competitive with VARs in a forecasting sense, they are in some cases actually superior.

But we're not quite there yet. If you go to the ECB website, you will find that the Smets and Wouters paper is featured prominently on its own page, where all of its virtues are itemized. But, at the time I was preparing these remarks, a funny thing happened when I clicked on the link to the Smets-Wouters paper. I was sent to the 2001 paper by Fagan, Henry, and Mestre that describes the Area-Wide-Model that is the ECB's equivalent of FRB-US, as if it to signal that the policy analysis process would only be rested from the cold dead fingers of the large-scale modelers. (I note that the link has since been fixed.)

Fine with me. But perhaps we should be careful what we wish for. Consistent with the observations made by Anders Vredin at this conference, in my time with the Fed I've noticed a powerful desire to comment upon data at a fairly detailed level. This leads to a temptation to add ever more complexity to the model structure, and introduce ad hoc elements that may be relevant only for some narrow question at some particular point in time. My casual impression is that one frustration of the FRB-US folks is that things get stuck in their model that are the econometric equivalent of the human appendix – vestigial remnants that served some purpose long lost to evolutionary history.

Another problem is that the dynamics of the policy briefing game seem to favor forecasting performance over theoretical integrity, an impulse that in the large-scale

modeling world has led to the infamous “add factor” problem. Smets and Wouters appear to have conquered this for now, in part by continuously re-estimating model parameters. To a degree that approach seems perfectly justifiable, but it is a short step from there to more substantial adjustments of the model structure. If this fine marriage of empirical performance and theoretical structure requires large and frequent adjustments in the quantitative specification, then we should begin to wonder if empirical performance isn’t cheating on theory.

There are already aspects of our front-runner DSGE models that are crucial to the empirical performance, but theoretical weak reeds. Mark-up shocks, for example, are quite important for forecasting inflation in the Smets-Wouters model. I presume the same would be true for other models that adopt the Christiano-Eichenbaum-Evans variant of the basic New Keynesian Model. But how much do we know about these shocks? Maybe they represent convenient driving processes for which we have approximately correct interpretations, and whose deeper theoretical source is not of too much concern for the estimation of aggregate GDP growth and inflation. Like technology shocks, perhaps. That would be my take, and if we stop here, fine (especially if it is possible glean some independent sense of the nature of these shocks from independent sources, like micro data). But if we find ourselves going down the road of adding a little structure here and there for the primary purpose of adding another stochastic process to capture this or that nuance of the data, game over.

All of this is to say that I think we are ready to be add DSGE models to the quiver, as one more arrow that we shoot into space every six weeks or so, hoping that we

hit the bullseye. But I don't think we should expect much more. To do so could easily be to build in the very weaknesses that we hope to avoid with a solid DSGE backbone.

When our theoretical structures begin to fail – and I'm going to argue that they will -- let's go back to the VARS and begin the Christiano-Eichenbaum-Evans procedure anew, rather than falling in love with an aging DSGE structure and thinking it will look just great if we pile on enough bling-bling.

Beyond the New Keynesian Framework

Why do I think there is a chance we will ultimately have to abandon, or substantially revisit, the DSGE frameworks that we accept as truth today? For that, let me return to the three steps that I think we are trying to unify: Objective setting, conditional forecasting, and counterfactual experimentation.

For the sake of argument, let me propose that the Fed shares a core objective with almost all other developed country central banks in desiring a rate of inflation that averages somewhere within a band of 1 to 3 percent annual growth in the Consumer Price Index over the course of a business cycle. There is, of course, no formal agreed-upon inflation target for the Fed, but it is no secret that the possibility of adopting one has been discussed by the Federal Open Market Committee. Enough participants have talked about the issue to reveal that 1-3 percent CPI inflation is a reasonable approximation to what a target might look like, and a level that even those who do not favor a formal target would find desirable.

One thing I feel reasonably confident about. This objective was not arrived at by virtue of analysis under the assumptions of the prototype New Keynesian model. Almost without exception this class of models suggests an optimal rate of inflation that is

somewhat negative, zero, or just a little bit positive, depending on the precise details. I don't perceive much of a taste among world central bankers for getting very close to these levels as a matter of practice.

There are, of course, several explanations: Bias in measured price statistics, fear of zero nominal-interest rate constraints, or just plain misguidedness on the part of policymakers. I don't find any of these to be completely compelling. I have written in the past about research that my late friend Bruce Smith was engaged in that made both empirical and theoretical cases for optimal inflation rates bounded away from zero. Bruce's efforts seem to point to optimal rates of inflation in the 1 to 3 percent range that central banks seem to actually adopt. (See, for example, Smith [2003]).

The theoretical core of Bruce's work was models in which adverse selection and financial intermediation take center stage, leading potentially to environments in which Mundell-Tobin type effects may appear at very low rates of inflation, but where credit-rationing kicks in when inflation exceeds some, perhaps quite low, threshold.

There is not much work at the level of Smets and Wouters that takes financial intermediation seriously. In fact, Christiano, Motto, Rostagno (2003) is the only example of which I am aware. I remain open to arguments that the forecasting/counterfactual policy exercises do not necessarily require these sorts of complications. If, for example, there is a range of inflation rates where the credit market imperfections are not relevant, then models that ignore the structure of financial intermediation may be just fine. But I don't think we are in a position to assume it is so just yet.

In fact, this points to what I think is a broader problem. We are close to falling dangerously in love with the basic New Keynesian framework, the sticky price aspects of

it in particular. Here is a simple observation: In the VARS that are identified in the usual ways, inflation wants to drop like a rock in response to a basic technology shock. Models that engineer significant price inertia don't want to let that happen. This is true in Altig, Christiano, Eichenbaum, and Linde (2004). It is true in Smets and Wouters.

There *is* a paper by Rochelle Edge Tom Laubach, and John Williams (2003) that appears to get the technology-shock inflation impulse response correct, but seemingly at the cost of a counterfactual response of prices to a monetary shock. This general problem has been emphasized by the Bank of Portugal's Nuno Alves (2004): The New Keynesian framework seems to have difficulty reconciling both sticky-looking behavior with respect to monetary policy surprises with the very flexible-looking response to technology shocks. One wonders if continually tweaking that framework can bring about a successful resolution.

One final point. In my time at the Fed, I have come to appreciate that most of the really important policy choices have nothing to do with Taylor rules or the like. They have to do with those episodes of financial crisis in which Taylor-like rules are woefully inadequate. Think here October 1987, the period from summer 1997 through the end of 1998, and the aftermath of September 11, 2001.

I have in the past agreed that it is useful to think of the policy choices in those periods as policy shocks. I would still argue that today. But it sure would be helpful if at least some of these events would appear as something more than completely random disturbances. In other words, it would be very useful to have usable measures of what we loosely call "financial market fragility," and more useful still to have a coherent

quantitative model as sophisticated as Smets and Wouters that captures them. They are, after all, buried somewhere in the probability distribution of the data.

Traditional models (such as FRB-US) are, of course, no better than DSGE alternatives on this score, and maybe models like Christiano, Motto, and Rostagno model can lead us where we need to go,. In the final analysis, I conclude that we now have DSGE models that are plausibly every bit as good as the existing competition, if not superior. But we should keep reminding ourselves that this may be a low standard.

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