

Comments: Modern DSGE Models

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Thanks to the organizers for the opportunity to discuss the papers of Marty and Harald.

Before getting into details, I want to say that there has been tremendous progress in the development of dynamic stochastic general equilibrium models over the last 40 years. When I was a graduate student, we were taught IS-LM models. John Taylor tried to push the fast forward button by introducing rational expectations in the Cagan model—but I found that very confusing. Whose expectations were being modeled?

Marty and Harald are University of Minnesota graduates so they might have avoided the painful trip via IS-LM to the modern era. They may have always worked with models that had optimizing households and firms—all forward-looking—and market-clearing prices. As I see it, the big advantage of the transformation was gaining tools that allowed us to do policy analyses and to design better institutions.

Now, let me turn to the papers of Marty and Harald. My take-away is that Marty is very enthusiastic about how things are going and Harald is more cautious, seeing many challenges still ahead. I have only 15 minutes so I cannot summarize all of the achievements laid out by Marty and all of the challenges laid out by Harald. I want to focus on a central question where there is no real consensus—either between them or between many of us out there in the trenches of research. Are DSGE models *useful* for doing policy analysis? Here, I want to emphasize the word *useful*. Many people tell me that DSGE models are *used* by central bankers. That is a low bar for me. The high bar is: are they useful? Can we trust them to make good predictions when considering policy changes or when considering the redesign of the institution. I read Marty's paper as a 'yes' and Harald's paper as a 'maybe' and, when reading for substance, a 'no.' Why is there any disagreement? Interestingly, I think it is for the same reason. Let me try to explain what I mean.

I'll start with Marty and with his own words: “A key challenge was to develop an

empirically plausible version of the New Keynesian model” and, he goes on to say that work he did with Christiano and Evans “met this challenge.” He contrasts this with earlier work of Kydland, Prescott, Long, and Plosser, better known as the “real business cycle” guys, who did not include money and assumed markets were competitive and frictionless. These models were simple, with few shocks, and as a result, missed some key properties of the aggregate data. For example, a key feature they missed was variation in aggregate hours, which were predicted to be much smoother than observed hours.

Another strand of the business cycle literature, which could be thought of as extending the real business cycle models, was the early New Keynesian models. Marty’s assessment of the models prior to Christiano-Eichenbaum-Evans, was they were not empirically plausible, focusing more on qualitative predictions and less on quantitative predictions.

Let me turn next to one of Harald’s challenges—he has many but I think this one is most relevant to what I am describing here. His challenge is that DSGEs are now empirically plausible. These are my words, not his, but hopefully he’ll see where I’m going with this in a minute. As you can see, I have set up the achievement to be the same as the challenge. So, that means there has to be a “but.” But, the empirical fit is due to the introduction of non-micro-founded frictions—Harald’s paper considers financial frictions but there are many other frictions as well—and to non-structural shocks. In effect, the new New Keynesian models are the earlier and simpler ones with wedges added to the first-order conditions. So, that leads us to the main question: Are these models useful for policy analysis?

To give some idea of what we’re talking about, let me consider two quantitatively important shocks in the DSGE models. These are exogenous shocks to wage markups and risk premia. They account for most variation in real activity. I want to show you specifically the impact on two labor market variables: unemployment and hours. As I noted earlier, one of the key critiques of early DSGE models was they missed key properties of the data, especially labor market inputs.

I’ll show two sets of results. First, I’ll show the main results of Gali, Smets, and

Wouters who compute the variation of unemployment that can be attributed to different shocks in the model, which will depend of course on which frictions—and there are a whole host of possible frictions—are included in the model. So, you should think of the exercise as depending on choices of two things: the choice of frictions and the choice of shocks.

There are two scales on this picture. The right hand side corresponds to the unemployment rate which is the black line. The left hand is the percentage points 11.5–4.5 (=7) so it is easy to read off the contributions of each shock. There are 8 shocks included which—when all are turned on—exactly replicate the black line (because some are positive and some are negative). Let me focus on two: wage markup shocks and risk premium shocks. Let's consider 2009 Q1. The unemployment rate is roughly 4 percentage points above the trend level. According to this graph, one percentage point is due to changes in wage markups and 2.5 percentage points are due to changes in the risk premium. So, 3.5 out of 4 for those two shocks.

Next, consider the results of Gust, Herbst, Lopez-Salido, and Smith, who do a state-of-art nonlinear computation with a binding zero lower bound. I'm going to focus on their decomposition of hours. The graph is slightly different. Here I show a time series for predicted hours with only one shock on at a time and then again with all shocks on. There are 5 shocks in the model—not shown are monetary shocks and government spending shocks, which account for the difference. They did not include wage markup shocks. Like Gali, Smets, and Wouters, these guys find that the risk premium shocks contribute the most in the 2008-2009 downturn. I should also note that contributions to monetary shocks and neutral and non-neutral technology shocks are not large, which is relevant for anyone that uses a limited information econometric procedure with only those shocks. It means they are trying to identify the economic impact of shocks that are small.

Now we turn to the 64 million dollar question. Are the shocks to wage markup and risk premia primitive, interpretable and micro-founded shocks, or are they wedges that stand in for unmodeled features of the data? And, if the latter, it matters what these wedges stand in for. The stochastic processes for these shocks are unlikely to be invariant

to changes in the policy environment and hence do not overcome the Lucas critique—which was the whole reason for moving away from ad-hoc IS-LM models.

First, consider the wage markup shocks and/or wedges. Are these shocks just variations in the value of leisure or are they variations in bargaining power of unions. Matters a lot. Risk premium shocks: are they fluctuations in attractiveness of short term government bonds relative to other assets? If so, they are unlikely to be invariant to monetary policy. Some researchers interpret these as shocks to external financing costs. Here, again, a change in monetary policy is likely to impact financial intermediaries and financing costs. I've also seen a third interpretation of these shocks as “capital quality” or “capital uncertainty” shocks, but I really don't know what that would mean vis a vis policy. So, to summarize, it's not clear if we are or are not violating the Lucas critique. We may simply be summarizing the extent of our ignorance.

In my view, one way forward to doing policy analysis is to move beyond these variance decompositions. The exercises have been useful only up to a point. The findings should now be used to identify promising DSGE models with micro-founded frictions and primitive and interpretable shocks. Then, the hard part starts, which is to discipline the analyses with micro evidence. Here, I mean we need to micro-found each “friction” and “shock.”

Finally, as a long-time Minneapolis Fed employee, I would be remiss to end things here. In my view, there is a better way forward. I think there should be more emphasis on designing rules and institutions. I worked Gary Stern who was the President of the Minneapolis Fed and Art Rolnick who was the Director of Research. These guys believed that long-term studies would pay off. And, I believe they have. They help us during crises like the current one in Europe, during policy overhauls like Too Big to Fail, and when considering new rules or policy actions like QE. Principles are first established and then the quantitative work can be built upon a solid bedrock.

That, in my view, is a better way forward.