

August 5, 2013

John,

Thanks for the comments, they were useful in lots of ways. We — I'm including Mike and Stan here — hope this is part of an ongoing conversation about related issues, so we thought we'd respond in some detail. We've also rewritten the paper to address some of your comments, and will do more once we've given this some thought.

First, let us say that we see the paper contributing to our understanding of the conditions required for identification. It's not about whether these conditions are plausible, or whether the models themselves make sense. We have some interest in both of these other issues, but we thought it best to stick to the first one and give the paper a sharp focus.

We think we learned a few things along the way. We found, for example, that some of our early guesses about how this worked were wrong. We thought we needed a shock in the other equation. The "Gertler example" suggests something of the sort. It turns out to have nothing to do with that. What we need is a restriction on the Taylor rule shock. A zero somewhere would do, or some kind of orthogonality condition. If we go with orthogonality, then it has to be orthogonal to something, and the other shock will work. But any restriction will do. We also thought knowing the yield curve would help. It doesn't, at least with this problem.

More constructively, we found that we need to know something about the shock. Period. We need to observe it, or know enough about it to impose a restriction. This applies to any forward-looking equation with an unobserved shock, including the NK Phillips curve and its cost-push shock (Section 4).

1. This is a personal question not a citation question, but I'm still puzzled whether the solutions you advocate are in the class I discuss or not.

To be honest, we're not sure, you have lots of stuff there.

In the discussion that follows this comment, it seems to us that you're combining two issues: the theory of identification and the plausibility of restrictions. We think it's useful to separate the issues because (i) they're logically distinct and (ii) reasonable people can and will disagree about what is plausible and what is not. It sounds presumptuous, but to use an analogy, we're doing the work of Koopmans, not Liu or Sims, although we find both interesting.

On s_{1t} specifically: In 3.1 it's tied to consumption growth, hence plausibly observable in this model, although that's not our point. More relevant to us: the identification problem has the same form whether this shock is there or not. See the last para of 3.1. In 3.2, it's hard to say what kind of restrictions make sense, which we think is

one of the things Ken Singleton has been saying: that is, what conceivable restrictions would you use in this model? We've wondered the same thing, and come up empty.

Note, too, the large number of people who have estimated affine models and interpreted the short rate equation as a Taylor rule. We haven't rubbed their faces in it, but they're plainly not solving the identification problem.

2. If you can identify the Taylor rule, run just one regression!

It's not a regression in these models, but we have some numerical examples in Appendix B that work through the calculations you need to estimate the Taylor rule.

You mention data here. We know it sounds wimpy, but identification is about whether you can estimate the parameters of a given model, not whether the model is a good one. Maybe that's what you mean by "heroic assumptions," in which case the examples should do the trick.

3. To illustrate the point, consider a simple Taylor rule...

Hmmm... If we understand what you're saying, yes, it's true, we assume agents know the model and see the shocks. So you ask: how do they solve the identification problem? To be glib, we just assume they do. That's the classical approach, and the one followed in the models we're discussing. But we're puzzled by what else we might do. If the agents don't know the model, what do they do? What do they think others are doing? If we're not careful, we get into one of those infinite regress problems: who knows what, what do others think they know, etc. That's an issue for any forward-looking model, not only NK models, and it's not an easy one to deal with.

Do you have something specific in mind? If so, we'd love to hear it.

4. See my long discussion of Giannoni, Rotemberg and Woodford.

You say this to bring up a different issue, but let us remind you of something you said at the EFG meeting in New York when you gave your paper. This is from memory, so the words won't be as clean as yours, but our recollection is that you said something like: NKs like to say their models are "new" and "forward-looking," but when they talk informally it's all Old K language.

Yes, exactly! This shows up most clearly, we think, when they switch back and forth between models and VARs. In the models, things are forward-looking and things react immediately to shocks. In VARs, it's common (standard?) to say that shocks have no current impact on things. In the piece you cite, Giannoni and Woodford are working in the VAR tradition. You quote them on page 600 of your paper: "we assume . . . that a monetary policy shock at date t has no effect on inflation, output or the real wage in that period." That sounds a lot like Sims to us. And yes,

that pretty much does in the NK theory, where the rule can't be estimated precisely because it affects inflation immediately. What you say here sounds exactly right to us: "This is an especially surprising result of a new-Keynesian model because [these variables] are endogenous ... [and] jump in the new-Keynesian equilibrium."

It's easy, we know, to take different approaches in different papers, so maybe it's unfair to compare statements across papers. We were hoping to contribute by providing a clean framework for talking about restrictions so that comparisons are more easily made. Our guess of what's happening is that Sims has some affinity for the Old K thinking, which he argue mimics lots of the stylized facts reasonably well. So that shows up in lots of VAR work. But it's an imperfect fit with NK models, as you note, and mixing the two is likely to generate more confusion than insight.

5. New Keynesian macro.

You don't say this, at least in these words, but there's a question lurking behind that scenes that we sympathize with: namely, is all this NK stuff a waste of our time? Well, we've voted with our own actions and avoided it for most of our careers. But there's value in meeting people halfway, which is what we're trying to do here. We've noticed you doing the same in your blog, and Steve Williamson as well.

We also find, as you do, that a lot of the financial data we use is nominal, so we need to address money and inflation somehow. Some of the old cash-in-advance models were one way of doing this. A Taylor rule is another, and it has the advantage of being simple. Thinking along these lines led us to early versions of your 2011 paper, and we're still (slowly) working our way up the learning curve. Along the way, we thought we might clarify issues as they come up.

In the meantime, let us know if you're ever in New York (Dave and Stan) or Los Angeles (Mike). We should continue this over a cold beer.

Cheers,
Dave, Mike, and Stan