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The State of Macro Is Sad (Wonkish)

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Olivier Blanchard has a characteristically informed, lucid essay on the role of DSGE models in macroeconomics, in which he accurately describes the problems with these models but – again characteristically – tries to make peace with both sides, calling for reform of this dominant paradigm rather than tossing the whole thing. I understand his motivations. But what strikes me is just how sad a portrait he offers of the state of macroeconomic theory.

Here's how I would approach the issue: by asking how we know that a modeling approach is truly useful. The answer, I'd suggest, is that we look for surprising successful predictions. General relativity got its big boost when light did, in fact, bend as predicted. The theory of a natural rate of unemployment got a big boost when the Phillips curve turned into clockwise spirals, as predicted, during the stagflation of the 1970s.

So has there been anything like that in recent years? Yes: economists who knew and still took seriously good old-fashioned Hicksian IS-LM type analysis made some strong predictions after the financial crisis that were very much at odds with what lay commentators, and quite a few economists, were saying. They – OK, we – declared that with interest rates near zero massive increases in the monetary base would not cause high inflation, that large budget deficits would not drive interest rates up or crowd out private investment, and that fiscal multipliers would be positive, in fact more than one, and would be considerably larger than estimates based on non-liquidity-trap episodes suggested.

And all of that came to pass. Those of us who knew our Hicks, directly or indirectly, seem to have had a real advantage over those who didn't.

Can you say anything comparable about DSGE? Were there any interesting predictions from DSGE models that were validated by events? If there were, I'm not aware of it.

Yet even while failing to offer any measurable gains in insight, DSGE had the effect of crowding out the stuff that actually did work. Olivier writes:

> I have found, for example, that I could often, as a discussant, summarize the findings of a DSGE paper in a simple graph. I had learned something from the formal model, but I was able (and allowed as the discussant) to present the basic insight more simply than the author of the paper. The DSGE and the ad hoc models were complements, not substitutes.

Um, no – he notes that he was allowed to present the basic insight more simply only because he was the discussant, but that the author of the paper wasn't allowed to do the same thing. That's DSGE substituting for, in fact, preventing the ad hoc approach. And most macro papers aren't published along with insightful discussions by Olivier Blanchard! There is a real loss and cost here.

So what is the gain from this style of modeling? Olivier offers some awfully weak tea:

> DSGE models can fulfill an important need in macroeconomics, that of offering a core structure around which to build and organize discussions.

Really? That's the point of a paradigm that has taken over the field? It sounds, by the way, exactly like the defenses I heard of academic Marxism when I was young: never mind whether it's right, it provides a framework.

Now, I don't know how to reform all of this. There is a huge amount of sunk intellectual capital in this modeling approach. But at the very least we should admit to ourselves how very sad the whole story has become.