Report on "DSGE Models and Central Banks"

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Researchers in policymaking institutions have put significant effort into developing a new generation of macro models with more rigorous microfoundations. The paper reviews challenges that these dynamic stochastic general equilibrium (DSGE) models face, with a particular emphasis on problems related to their use at central banks. Reviews and summaries like this one are much needed since it is important to carefully appraise what has actually been achieved in the science to avoid mistakes that occurred in the past. However, I fear that the paper only provides a useful study for a more limited audience than the title might suggest. Under the assumption that the intended reader is someone looking for an introduction and overview of the academic issues, the presentation is sometimes a bit too detailed without much added value compared to the sources cited. On the other hand, if the intended reader is someone looking for an overview of the policy issues in general and the interaction of DSGE models in the policy process in particular (i.e. practical implementation issues) the review comes out as too short and without much of a guidance for the reader. The paper mainly discusses issues related to academic oriented problems such as identification problems and misspecification (of course with exceptions). This may be a natural division since many of the research problems need to be addresses before policy questions can come into question. Nontheless, it is an unfortunate imbalance between the sections since central banks use DSGE models in two equally important areas: research and policy analysis (the ECB has e.g. one division dealing with research namely DG Economic Research and one division dealing with more direct policy issues namely DG Economics).

Furthermore, it is often difficult to understand the point of reference or benchmark for comparison when discussing modeling problems. This is particularly important in section 2 and 3 where modeling challenges and data issues are discussed. It is hard to understand whether the issues are specific to DSGE models or if the issue at hand also applies more broadly. If the purpose of using a DSGE is to be able to do counterfactual policy analysis, the fact that it forecasts worse than e.g. a vector autoregression estimated with Bayesian techniques (BVAR) is not very interesting. The benchmark for comparison in this case - the BVAR - does not provide an alternative framework for the task at hand.

However, this unclear point of reference or benchmark for comparison and the emphasis on the academic aspects of DSGE modeling is a somewhat problematic aspect of the review. DSGE models are designed to mimic only certain aspects of reality, usually specified moments of observable data. They typically have other implications that are clearly false and lead to their immediate rejection if taken literally. This is shown clealy throughout the review. An exampe is the following:

"Therefore, by excluding a formal modeling of financial markets or financial frictions, the current benchmark DSGE model fails to explain important regularities of the business cycle (thus putting too much weight on, say, monetary policy or productivity shocks). It also excludes any possible analysis of other key policy issues of concern for central banks, such as financial vulnerabilities, illiquidity or the financial systems´ procyclicality. In fact, the weak modeling of financial markets in these models also limit their use for stress testing in financial stability exercises."

Hence, it is easy to refute many of the current DSGEs based on this literal interpretation of the lack of formal modeling of financial markets - many of the implications are clearly false (this, of course, also holds for any type of model). In the same vein it is also easy to refute current DSGEs based on problematic modeling of currency risk premia, fiscal policy and other modeling challanges pointed at in the review. In this sense the review is based on what Geweke (2007) calls a "strong econometric interpretation" of the relation between DSGE models and measured economic behavior. The strong econometric interpretation is not a problem but the review comes out as far too pessimistic and critical when it comes to the use of DSGEs for policy purposes. This is particularly evident in section 6 which offers some conclusions:

"Despite the rapid progress made in recent years, at their current stage of development, these models are not fully ready to accomplish all what is being asked from them. They still fail to have a sufficiently articulated description of the economy, which may be forcing them to downplay or ignore important interactions in the economy."

If one instead would take on a weaker or minimal econometric interpretation of models in general and DSGEs in particular the conclusion may have looked something like this:

"The rapid progress made in recent years has made, at their current stage of development, these models ready to accomplish all what is being asked from them (but not more). Like all models they still fail to have a sufficiently articulated description of the economy, which force them to highlight the most important interactions in the economy for the questions at hand."

All in all, this paper provides a useful summary of these problems and reviews like this one are much needed since it is important to avoid mistakes that occurred in the past. The review offers valid points and the issues and challanges discussed have to be on the agenda of policy institutions which are interested in the usefulness of DSGEs for policy analysis and forecasting. However, the summary is vague on what grounds it evaluates DSGE models and the interaction with their use at central banks. A problem is that the review brings out problems from the academic literature and when interpreting these problems literally the DSGEs fail to deliver. This is not a specific problem of DSGEs however and it is unclear in the review whether problems are specific or more general. It would be nice if the review could provide more details on how the DSGE:s are actually used in central banks. Faust (2008) concludes that "Nobody suggests we follow DSGE models literally or mechanically" (which is a reference - together with Geweke 2008 - that is clearly missing in the survey). The central question is really, which comes from George E. P. Box., with the knowledge that all models are wrong, how wrong do they have to be to not be useful?

References

Faust, J (2008): "DSGE models in a secomd-best world of policy analysis", Mimeo, John Hopkins University, http://e105.org/faustj.

Geweke, J (2007): "Models, Computational Experiments and Reality", Mimeo, University of Iowa.