

## Not All DSGEs Are Created Equal\*

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Olivier Blanchard made a very important point in his January 12 post on macroeconomic models ([Blanchard, 2017](#)): “Different classes of macro models are needed for different tasks.” I agree strongly with this point, and I agree with almost everything else in Blanchard’s post, except I do not think it does justice to DSGE research.

Specifically, I disagree with the following paragraph:

“Take theory models. DSGE modelers, confronted with complex dynamics and the desire to fit the data, have extended the original structure to add, for example, external habit persistence (not just regular, old habit persistence), costs of changing investment (not just costs of changing capital), and indexing of prices (which we do not observe in reality), etc. These changes are entirely ad hoc, do not correspond to any micro evidence, and have made the theoretical structure of the models heavier and more opaque.”

The paragraph makes a blanket statement about DSGE modeling that makes readers think that what Blanchard describes was done everywhere in the DSGE literature. I disagree with that view. There are DSGEs and DSGEs—or, put differently, not all DSGEs are created equal.

I personally do not like the approach of putting ad hoc frictions (and shocks) everywhere in the models to twist and bend them until they fit the data. I do not see much of a difference between that and completely ad hoc non-microfounded modeling. But all those “bells and whistles” were most often added to the models by researchers working on them at policy institutions, including the IMF, under pressure to make results more “empirically relevant.” This required generating a closer match with multiple series of data. The models could then be used for applied policy exercises rather than simple illustration of qualitative results.

In my own work with Matteo Cacciatore, Romain Duval, and Giuseppe Fiori, we did some of that in our papers on structural reforms during recessions or at the zero lower bound ([Cacciatore et al, 2016a](#) and [2016b](#)) precisely because of the IMF’s goals: We needed to make the model “more quantitative” for its results to be used in the April 2016 issue of the *World Economic Outlook* ([IMF, 2016](#)). I think it was useful work, and it helped make important policy points about the risks of doing certain reforms during recessions, or the fact that whether or not the zero

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bound should delay reforms is a more complicated issue than suggested by [Eggertsson, Ferrero, and Raffo \(2014\)](#). [Christine Lagarde](#), [David Lipton](#), and [others](#) at the Fund have been giving policy advice based on the results of those papers (and [on other work I did with Matteo Cacciatore and Giuseppe Fiori](#)) since last spring (sadly, almost always without mentioning that those were *our* results and not just the results of “IMF research”). But I do prefer the cleaner versions of the model, without the “quantitative bells and whistles,” because they deliver the same qualitative results in tighter, more transparent fashion.

From my perspective, when a model (or class of models) performs poorly, the question I am interested in is “what mechanism(s) are we missing?” And with mechanism I mean stuff that is deeper than ad hoc tweaks to the specification of preferences or adjustment costs.

Continuing to use my work as example, this is what motivated me to get into the models with producer dynamics I worked on with Marc Melitz and then also Florin Bilbiie ([Ghironi and Melitz, 2005](#), and [Bilbiie, Ghironi, and Melitz, 2012](#)). I thought the New Keynesian framework was missing something important by always assuming a fixed set of producers and goods in domestic and export markets, and I wanted to learn how that would matter for model predictions, ability to explain facts, and policy. So, I started the agenda with Marc and Florin.

To me, the papers we published in [2005](#) and [2012](#) were about explaining what we can learn from taking market entry/exit seriously in macro models, and doing that in the cleanest models we could write, even if they failed to match all sorts of features of data that are not central to what we were focusing on.<sup>1</sup> The numerical exercises about business cycle moments in my paper with Marc Melitz ([Ghironi and Melitz, 2005](#)) were not what the paper was really about—except serving the purpose of illustrating that the model did at least as well (or poorly) as the standard framework, but with the added benefit of helping us learn something new and interesting about real world dynamics.

I think there is a lot of other DSGE research that did that in the past and focused on key mechanisms and results in very clean models. Much work in New Keynesian open economy macroeconomics comes to mind, with many DSGE papers presenting very clean models with stark results and policy implications, but obviously no close connection with data (for instance, [Benigno and Benigno, 2003](#), [Corsetti and Pesenti, 2001](#), [Devereux and Engel, 2003](#), [Obstfeld and Rogoff, 2000](#), and many others—including more work by these same scholars). Blanchard himself as a paper with Jordi Galí ([Blanchard and Galí, 2010](#)) that seems to me a great example of valuable DSGE work that delivers important results while not seeking a close match with data.

I think there is a lot of work of this type that’s going on now, perhaps more of it in academic departments than policy institutions because we do not face the constant pressure of “quantitative relevance.” The HANK model that Kaplan, Moll, and Violante have been working on is a valuable step toward bringing household heterogeneity into the picture ([Kaplan, Moll, and](#)

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<sup>1</sup> For this reason, the models in those papers abstracted from realistic ingredients such as nominal rigidity, unemployment, the role of monetary policy, or other features of reality. In our first papers, we wanted to focus on the lessons from incorporating producer dynamics in the simplest versions of the basic macro model with monopolistic competition (the foundation of New Keynesian macro) before getting into the analysis of other, very important questions.

[Violante, 2016](#)). Obviously, it is impossible to go as deep into it with pencil and paper as in representative household models, but it is still a much cleaner model than the “kitchen sink” variety. [Dmitriev and Hoddenbagh’s \(2013\)](#) work on fiscal unions; [Farhi, Gopinath, and Itskhoki \(2014\)](#) on fiscal devaluations; [Farhi and Werning’s \(2012\)](#) rewriting of New Keynesian open macro to study capital controls and their own work on fiscal unions ([Farhi and Werning, 2013](#)). These are all examples of (in my opinion) clean, recent DSGE modeling that delivers clear, useful results without seeking to match this and that and this other data series.

Many other examples come to mind: [Bacchetta and van Wincoop’s \(2016\)](#) model of the Great Recession as result of a self-fulfilling global panic; work by [Corsetti et al \(2013\)](#) on the euro area crisis and its implications for macro policy, and [Corsetti et al \(2016\)](#) on exchange rate policy in a Great Recession; Devereux’s papers with various coauthors (for instance, [Cook and Devereux, 2013](#), on macro policy in a world liquidity trap, and [Devereux, Senay, and Sutherland, 2014](#), on financial globalization and monetary policy); [Fujiwara, Kam, and Sunakawa’s \(2016\)](#) on international monetary cooperation; [Ravn and Sterk’s \(2016\)](#) “when-HANK-met-SAM” marriage of [Kaplan, Moll, and Violante \(2016\)](#) with the search-and-matching model of unemployment. [Ozhan \(2016\)](#) uses an adjustment cost, but he does a very nice job of modeling the CEPR “consensus view” on the euro area crisis and disentangling several different channels of shock propagation. Luca Fornaro is doing excellent work, some with Gianluca Benigno on stagnation ([Benigno and Fornaro, 2016](#), and [Fornaro, 2015](#)). And, for the sake of brevity, I am neglecting many people and strands of DSGE work in macro and international macro—most notably, many other contributions to the booming literature on incorporating financial frictions and intermediaries, or to the similarly booming literature at the intersection of international trade and macroeconomics.<sup>2</sup>

So, it seems to me that there is a lot of DSGE literature that did or does what Blanchard says we should be doing: “...accept the fact that theoretical models cannot, and thus should not, fit reality closely. The models should capture what we believe are the macro-essential characteristics of the behavior of firms and people, and not try to capture all relevant dynamics.” But Blanchard’s post does not acknowledge the existence of this large part of the DSGE literature, and that’s what I disagree with. It gives the impression that all DSGE research quickly veered toward the “kitchen sink” approach, and it ends up giving ammunition to those who believe as a matter of dogma that “(a) you can get anything out of DSGE models and (b) assumptions are hopelessly opaque and stylized” (Smith, January 10; see Figure 1 below). It also provides ammunition to the cadre of other bloggers and journalists who have been happily jumping on the bandwagon of those who agree with this quite incorrect position. I do not think this was Blanchard’s intention with his description of DSGE research.

Other than this, I agree with everything Blanchard wrote—especially with his emphasis on the importance of having different modeling tools for different purposes. (I still very much value the lessons we can learn from non-microfounded models of macroeconomic interdependence of the [Canzoneri-Henderson, 1991](#), or [Giavazzi-Pagano, 1988](#), variety!)

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<sup>2</sup> Those who pay attention to ongoing research will also have seen papers that allow for departures from rationality and manage to obtain transparent, intuitive results.



**Figure 1.** Myth and Prejudice

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