

# Discussion of “Macroeconomic Propagation under Different Regulatory Regimes: Evidence from an Estimated DSGE Model of the Euro Area”

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It is obvious to any reader that the paper by Matthieu Darracq Pariès, Christoffer Kok Sørensen, and Diego Rodriguez-Palenzuela (this issue) represents an enormous amount of careful and detailed research. There is an old saying normally applied to theoretical papers in economics—that they are akin to a sausage, and that while the final product may be very good, one does not necessarily want to know what went into it. But perhaps the reverse is true for this type of paper. The research itself is perhaps as important as the results; what one gains from actually conducting this type of research may dominate what one learns from only considering the results. The other way to state this is that it is very difficult to convey everything that has been learnt from this type of research in the form of a standard written paper.

The paper develops a dynamic stochastic general equilibrium (DSGE) model with a large number of constituent elements. What are the main ingredients of the paper? The basic model has patient households that end up as savers and impatient ones that are the early consumers, and households may form habits; it has monopolistic unions and differentiated labor inputs; and it has entrepreneurs that produce residential goods (using capital, labor, and land) or non-residential goods (using capital and labor). There is a monopolistically competitive retail sector that sells to a perfectly competitive retail sector with a continuum of differentiated goods. The

banking sector has three types of institutions, including a wholesale branch (assumed to be competitive), a (monopolistically competitive) deposit branch, and (competitive) lending branches.

The financial sector is the focus of much of the action in the paper. Loans are one period and, in different versions of the model, are at fixed or have state-contingent rates of interest. Loans are subject to strategic default, which in turn depends on a technology to seize the assets of the borrower. Loans to households are subject to a loan-to-value type restriction, and households may or may not be constrained in their borrowing decisions. Interest rates (as per prices) are set using Calvo-staggered contracts such that monetary policy has sluggish effects. Banks are subject to either Basel I or Basel II type minimum capital regulations. Finally, with respect to the government sector, the fiscal authorities finance expenditure with lump-sum taxes, and monetary policy is set using a type of Taylor rule.

If these are the main ingredients of the model, how is the dish then cooked? There are six types of uncertainty in the model: technology shocks (general, housing, non-housing, labor, and public expenditure), preference shocks (consumption and housing), price shocks (to price markups and bank spreads), idiosyncratic shocks (to households and entrepreneurs), bank capital shocks, and monetary policy shocks. The model has over 100 parameters. Many of these are fixed using the relevant literature to determine reasonable values, but some fifteen are estimated by calibrating the model to euro-area quarterly data.

The results of the paper are illustrated in a variety of ways including a set of impulse responses. The stated aim of the authors is to illustrate the role of financial frictions and also to consider the role of monetary policy and how monetary policy interacts with the different bank regulatory regimes (Basel I or Basel II and Basel III's anticyclical capital requirements). There are 20 impulse responses for each shock analyzed, and some 15 shocks are analyzed—so 300 impulse responses. In each graph there are 4 lines (baseline model, a version with high bank capital, another with imperfect interest rate pass-through, and finally a case with a predetermined interest rate), so some 1,200 lines to consider. In this rather brief discussion I will certainly not attempt to be comprehensive!

Indeed there is clearly a lot going on and much to consider and to discuss. There are so many ingredients mixed together that it reminds me of some comments Italian friends made to me regarding two Italian dishes—namely, polpetta and pasticcio al forno. The former, polpetta, is a tasty dish, and many ingredients can be thrown in, including the occasional leftover. It can be mixed up without too much care being taken, and it still tastes very good. Pasticcio al forno, however, is very different. It has many specific ingredients and they must be treated and added with extreme care and delicacy. Reading this paper, one wonders, is this polpetta or is it pasticcio al forno? I will come back to this question below.

In fact, given the complexity of the model, the authors do a reasonably good job in providing some simple intuitions regarding the basic results of the paper. For example, a positive technology shock leads to a lower interest rate and greater activity through an accelerator-type effect. A negative shock to households increases default probability, increases interest rates, and dampens economic activity, although this is attenuated through monetary policy.

However, having said this, the paper is hard to follow at times. The art of DSGE modeling has taken different routes. A route favored by some academics has been to attempt to employ these tools to see what model features might explain particular features of the data. A good example is the paper by Aguiar and Gopinath (2007), who claim that introducing a stochastic growth term can capture the sort of economic fluctuations found particularly in emerging economies. One might speculate, given the recent financial crisis, that this argument could be more relevant to industrialized economies. An aim of this strand of the literature is to explain the data with a parsimonious model to help our understanding of what might be needed (i.e., the minimum required) to be able to characterize specific aspects of the data under consideration. Chang and Fernandez (2010), in an interesting recent paper, develop a model that encompasses Aguiar and Gopinath (2007), as it has the stochastic growth term but also a particular financial friction. They claim that the latter actually dominates in terms of more closely explaining the fluctuations in emerging economy data. This example shows how this strand of the literature is advancing by attempting to delineate improved model features that might capture aspects of the data more accurately.

A second route of DSGE modeling puts less emphasis on parsimony and more on explaining as many aspects of the data as possible. This is the route which tends to be favored by central banks that need to understand the wider impact of their actions on various parts of the economy. This paper is firmly within this strand of the literature. As already mentioned, the ingredients are both plentiful and complex.

A potential problem with this modeling choice is that one is then not sure which parts of the model are really doing the trick in terms of capturing different features of the data. For example, considering the impulse response graphs, I am struck by the apparent relatively small differences between the versions of the model with and without the collateral constraints. The authors allude to a Kiyotaki and Moore (1997) type credit cycle, where higher asset prices might relieve the constraint while a steep fall in asset prices would imply that the constraint binds and force banks to curtail lending with further negative consequences for the economy and asset prices. It would be of interest to know whether the apparent relatively small effects are simply due to the fact that this feature of the model is not really needed, given the EU 15 data used to calibrate the model, or to some other reason.

A second avenue worthy of further analysis is how the various financial frictions in the model interact. In an interesting recent paper, Martin and Taddie (2010) develop a model with two financial frictions and claim that their interaction amplifies the effects. Two frictions may then be worth more than their sum in terms of explaining economic fluctuations. The complexity of the model employed in this paper may not allow for such a clean theoretical analysis, but carefully chosen simulations could allow for exploration of this idea. As it stands, there is little in the paper on how the various model features interact.

The simulations on the effects of Basel II are of considerable interest. The conclusion is that Basel II (relative to Basel I) would have increased GDP volatility by some 5 percent, keeping monetary policy constant, although the authors also note a higher impact of shocks to the loan book due to the higher risk weights on riskier loans and banks will need to recapitalize more frequently. All in all, however, the effects of Basel II on volatility would have been quite small over the period.

An interesting finding is the interaction between macroprudential policies such as Basel III's anticyclical bank capital requirements and monetary policy. The authors find strong support for macroprudential policies (to minimize a loss function over growth, inflation, and interest rate volatility), and when macroprudential policies are operating, they conclude that monetary policy should not respond to asset prices or to credit. But they also state that the optimal rule would be difficult to implement in practice, as bank leverage would then become very volatile (4.8 times the baseline). In fact, bank leverage becomes quite volatile in the Basel II simulations (2.3 times the baseline) and GDP volatility becomes more volatile. Basel II then appears to introduce "bad" bank leverage volatility, whereas the macroprudential rule may produce "good" bank leverage volatility.

However, it is not entirely clear what is going on here. If macroprudential policy reduces economic fluctuations, one might expect bank leverage to become less volatile and not more. Banks should increase leverage less in the good times and reduce it less in the bad. This then begs the questions, what are banks actually doing in this model and is leverage volatility really a useful metric and a potential problem? The authors appear to be able to live with a Basel II simulation with leverage volatility being 2.3 times the baseline, but there is no convincing argument why 4.8 times the baseline is a problem. And if 2.3 times the baseline is okay, then why do the authors then feel the need to restrict leverage volatility all the way back to the baseline volatility for the case of macroprudential policy with restrictions on bank leverage changes?

I would also like to make a couple of more general points regarding DSGE modeling and macroprudential policies. In general, such models have actually found rather little impact for macroprudential policies including anticyclical capital regulations, and this paper in general reinforces this view. There may be at least two reasons for this. The first is that, on average, banks tend to hold buffers over actual capital requirements, and indeed those buffers are surely endogenous with respect to economic volatility. This is most clearly seen in the case of emerging markets. Take Latin America as an example. Broadly speaking, capital requirements around the continent are close to Brazil's 11 percent of assets at risk, substantially

higher than Basel's recommended 8 percent minimum. But banks actually hold around 16 percent of capital in relation to assets at risk in the region.<sup>1</sup> Tier 1 ratios are also substantially higher than requirements and higher than in most G10 countries.

In this paper, banks target 11 percent of capital to assets at risk and are subject to a quadratic loss function that ensures that capital stays above the 8 percent minimum virtually always. In a recent paper I have written with coauthors (Aliaga-Diaz, Olivero, and Powell 2011), banks are forward looking and again are subject to a quadratic loss function if banks hit requirements and we calibrate this model to fit Latin American bank capital buffers. Repullo and Suarez (2008) develop a model taking a slightly different approach, as banks in their model hold buffers to ensure they can take advantage of profitable opportunities if they arise.

The fact that banks endogenously hold such buffers, which may then vary over lending cycles, dampens the effect of banks' procyclicality and will then also reduce the impact of anticyclical macroprudential policies. Hence calibrating these models to actual average bank capital buffers tends to lower the apparent value of macroprudential policies. One issue with this is that bank capital buffers vary across financial systems that tend to be as strong as their weakest link. To date, DSGE modeling has not captured well how weaker financial institutions and contagion, through financial contracts or investor reactions, may affect the behavior of all financial institutions and hence the real economy.

A second point relates to the interaction between the real economy, finance, and asset prices, which is critical to an understanding of credit cycles. To date, economic models (including DSGE models) have not captured the size of asset price movements, both up and down. In recent work, Cesa-Bianchi, Rebucci, and Powell (2011), using a global-VAR model, show that the majority of crises (stock market, currency, and banking)—including, for example, all U.S. stock market crises—are preceded by periods of "exuberance," defined as asset price booms that are not explained by economic fundamentals.

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<sup>1</sup>See the International Monetary Fund's *Global Financial Stability Report* for data on bank capital as a percentage of assets at risk.

The point is that, to date, it is not clear that the current state of DSGE modeling really captures the types of asset price booms and busts that may be partly driving credit cycles. In turn, this may then mean that they do not find the critical role for macroprudential policies that many have in mind. It seems an odd comment regarding this paper, but perhaps there remains a missing ingredient.

To conclude, this is a very interesting paper with a great deal going on. I asked the question earlier whether it might be polpette or pasticcio al forno, and my answer is that in the end it is impossible to tell. This is in fact my main criticism of the paper as it stands. What I mean by this is that it is hard to tell which is the critical ingredient or ingredients and which aspects of the model are truly driving the results obtained. The paper, and the large amount of research conducted behind the paper, has significantly pushed forward the art of DSGE modeling into characterizing financial frictions and attempting to understand their impacts on the real economy. The paper includes many ingredients and in fact opens up several new avenues for future work. The authors may wish to focus on particular aspects of the model and simulations to attempt to understand more precisely what is actually driving specific results. This would then provide useful results to direct future work in the area, and I certainly look forward to reading future papers from the authors in this direction.

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