

Comment on Gertler, Gilchrist and Natalucci, ‘External Constraints on Monetary Policy and the Financial Accelerator’.

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My deepest apologies for not being able to deliver these remarks in person. I am sure that Phillippe will deliver these comments with elan and will suppress errors in my comments.

This paper is a first class demonstration of the best of modern macroeconomics. We have moved well beyond quasi-religious discussions of the meaning of involuntary unemployment to a detailed examination of the quantitative role of various mechanisms to generate and propagate fluctuations. All the discussion is now about the size of capital’s share, the elasticity of labor supply, the intertemporal elasticity of substitution in consumption, the size of bankruptcy costs and so on. Macroeconomists are miles ahead of our colleagues in other subdisciplines in the profession and I think it useful to use this fine paper as a way of patting ourselves collectively on the back.

But, a discussant’s job is to be ornery about even a fine paper. Let me begin with some comments that are intended to improve the presentation and exposition of the paper.

1) The key shock in the paper is a rise in the foreign interest rate, which the authors correctly think of as a rise in the risk premium. But, this connection can and should be made much more explicit. One suggestion is to think about a country-specific tax on borrowing and lending. With such a tax, the domestic interest rate would have to be replaced by the after-tax interest rate in the model. A change in the tax is then equivalent to a rise in the foreign interest rate. One interpretation of such a tax is the possibility of expropriation of assets by the government. A rise in the expropriation probability could be interpreted as a tax.

2) In a paper that is allegedly about international events, it is surprising to see no reference to the current account or the trade balance. The paper desperately needs to incorporate figures both for the data and the models. My conjecture is that the current

account for the model might well be close to that in the data. The reasoning is simple. Consider a model without any of the frictions emphasized by the authors. In such a model, a rise in the foreign interest rate would lead to an outflow of capital and thus a current account surplus. The frictions in the authors' model do not seem likely to impede this basic force.

3) It is disappointing and surprising that no data on the price level or the inflation rate is reported in this version of the paper. Most of the models in the paper seem to generate a small decline in the inflation rate upon impact. My impression of the data is that it shows a modest rise in inflation following the Korean crisis.

4) I would be useful to report data on consumption as well, as a way of comparing models and data. My understanding of the data is that consumption falls sharply. My conjecture is that consumption in the model changes relatively little. This conjecture is based upon the kind of work that Backus, Kehoe and Kydland and others have done which suggests that international models produce much more consumption smoothing than we see in the data.

Let met turn to more substantive issues.

5) In work with Ellen McGrattan and Pat Kehoe, I have emphasized that with complete markets relative consumptions across countries are determined solely by the real exchange rate. This implication is wildly counterfactual. To take Korea as a simple example, models with conventional measures of risk aversion would imply a change in relative consumption between Korea and the US of the order of 30 to 40 percent following the crisis. Ellen, Pat and I found that incomplete markets did not quantitatively change this central feature of the data. We think that only if we understand this puzzle will be able to make progress on a quantitative analysis of financial crisis. One way of thinking about this result is that Gertler, Gilchrist and Natalucci get a miniscule change in the real exchange rate, compared to the one-hundred percent movement in the real exchange rate seen in the data. They shrug their shoulders at the failure of their model to reproduce the large movements in the real exchange rate. There is no doubt in my mind that observers call the crisis severe precisely

because they saw a gigantic movement in the real exchange rate.

A final misgiving about a central ingredient of this model. This comment really applies to Bernanke, Gertler and Gilchrist, upon which this paper is based. These authors have an economy with risk neutral agents called entrepreneurs and risk averse agents called households. They claim that an optimal contract in the presence of aggregate risk has the return paid by entrepreneurs to be a constant, independent of the current aggregate shock. I have trouble understanding this result. Surely, entrepreneurs should and would provide insurance to households against aggregate shocks. One way of providing such insurance is to provide a high return to households when their income from other sources is low and a low return when their income from other sources is high. My own guess is that if they allowed the return to households to be state contingent, then aggregate shocks would have no effects on the decisions of households and would be absorbed entirely by entrepreneurs. Before we push this intriguing financial accelerator mechanism much further, I think it would be wise to make sure that we get the microeconomics right. It would be hard, but quite feasible, to allow entrepreneurs to be risk averse as well.

My apologies again for not being there in person. But, I did want to say: Mark, Simon and Fabio - you guys have written a fine paper which I liked a lot. And, I hope that my suggestions on exposition and presentation will make the next revision even better.