Referee report on Roberto Tamborini's paper 'The "credit-cost channel" of monetary policy. A theoretical assessment'

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The paper brings together two types of financial market frictions, working-capital and credit-cost channel, in a otherwise stylized model in order to gauge the ability of these assumptions to reproduced three empirical observations. In particular, the author highlights the inability of the majority of existing DSGE models in reproducing a negative correlation between the policy rate and real wages and profits.

In my view the paper is an interesting reading, although not always as clear as the reader might wish.

In the following I highlight some of the problems

- The summary of the existing results is problematic. Since the CEE (EER, 1997) a number of DSGE models have been developed which include financial market frictions (e.g. Christensen, Ian, Corrigan, Paul, Mendicino, Caterina, and Nishiyama, Shin-Ichi: An Estimated Open-Economy General Equilibrium Model with Housing Investment and Financial Frictions, Bank of Canada, August 2007, Mimeo; CHRISTIANO, L.J., TRABANDT, M., and WALENTIN, K.: Introducing Financial Frictions and Unemployment into a Small Open Economy Model, Riks Bank, Mimeo). These models have the potential to reproduce the facts accounted for by the authors' model (my experiments with the New-Area-Wide model of the ECB including housing and financial market frictions shows indeed that the correlation is negative (parameters at the posterior mode)).
  In particular, the comment that these models "... require sticky prices as a *sine qua non* condition for monetary policy to have real effects" is imprecise: most of these models have nominal financing contracts so that monetary policy has real effects even without sticky prices (and also without limited participation: which, by the way, to my knowledge is not modelled in the current version of the Christiano, Motto Rostagno model cited by the author).
  Most of these models build in a supply-side effect of monetary policy to the extent that firms face higher costs of production. In this view the statement in the first paragraph of page 4 is imprecise.

- In the abstract one reads that results will be produced "... with no recouorse to non-competitive hypotheses and frictions". I'm not so sure. Financial market frictions exist in the proposed models. And some of them are quite dramatic (e.g. the cash constraint on households and their timing constraint). It is difficult to judge which frictions are more frictions than others. Maybe the point was simply on nominal rigidities.

- When, on page 5, the author comments on the "deprecable, albeit diffuse, malpractice" of taking models to the data too quickly, he stresses only one (problematic) aspect of current research. Nevertheless, he understates the fact that the problem faced by modellers is often to come up with assumptions that can be taken to the data. One drawback of stylized models is that it is not easy to take them to the data and hence to judge them in a broader perspective: a problem, I think, the paper at hand would have.

- One problem I have with the current set-up of the paper is that inflation next period seems to be always known by all agents. This might not be the case, but the presentation of the model does not help the reader. For example at page 8, it is stated that $E_t\left(\widetilde{P}_{j,t+1}\right) = P_{t+1}$ "... where P_{t+1} will be the actual price index". I take from this that $\widetilde{P}_{j,t+1} = P_{t+1} + i.i.d.$ and that P_{t+1} is known at time t. Furthermore, as the preferences of the households for the different products is not really clear, I don't see why households need to buy any good i as opposed to good j if, ex-post, $\widetilde{P}_i > \widetilde{P}_j$. Related to this point is also my question about profits: it seems to me that they are always zero ex-post (as firms are homogeneous ex ante and as the idiosyncratic shocks seem to sum-up to zero). Is this correct? (If not, what is exactly P_{t+1}?). If so, there is no way the model can explain on of the stylized facts deemed important, i.e. the negative correlation of profits and policy rate.

- At page 11 it is stated that "without loss of generality we can set the net revenue from defaulting to zero". This is not very intuitive. Is the "generality" to be interpreted as broad qualitative results? I would imagine that if the default cost is zero (so net revenue is the full value of the firm) the premium asked by the bank should be lower. This fact would be important for the quantitative implications of the model.

- On a more deeper level, looking at the model summary at page 13, I would like to know in which respect this model is different from a model with working-capital assumption and the "Financial Accelerator" (as the Christiano-Motto-Rostagno model has). If there is no difference in essence, the contribution of this paper would be a bit blurred. In this case (and at this stage it seems to me so) the paper would require some re-marketing and the focus should shift to "inspecting the mechanism" rather than proposing a new modelling strategy.

- Coming to the Policy Implications (section 4), the second paragraph does not seem to be a conclusion one would derive from the model. It is certainly an open question, but it should be addressed in light of a model and not simply as a bonus (see the recent paper by Curdia and Woodford, for example).
- There are a few typos in the paper (e.g. title of section 2 and incomplete sentence in footnote 2)