

The Referee Report on
‘Essays on Monetary Policy and Estimation of DSGE Models’
by Yuliya Rychalovska

The dissertation ‘Essays on Monetary Policy and Estimation of DSGE Models’ by Yuliya Rychalovska consists of two papers. The first paper ‘The Implication of Sectoral Heterogeneity for Monetary Policy and Welfare in a Small Open Economy: A Linear Quadratic Case’ analyzes optimal monetary policy in an open economy. The second paper ‘Bayesian Estimation of DSGE models under adaptive learning: robustness issues’ confronts estimation results for a set of DSGE models for rational expectations and adaptive learning dynamics.

Both papers are nice contributions to the macroeconomic literature. **The dissertation warrants a dissertation defense and an eventual award of a doctorate title.** I also think that Yulia should try to publish her papers.

The rest of the report contains some comments on the two papers. These comments may be used as hints for discussion during the dissertation defense.

Comments on the paper
‘The Implication of Sectoral Heterogeneity for Monetary Policy and Welfare in
a Small Open Economy: A Linear Quadratic Case’

The paper evaluates the optimal monetary policy in an open economy DSGE model, which is calibrated on the Canadian economy. Yuliya compares several possible monetary policy rules and ranks them according to the welfare. I think that this paper is well-written, demonstrates technical skills of the author, and also provides the intuition behind the results. Given the fact, that similar paper have been published by the author (as the CERGE working paper, and as the Czech National Bank working paper), it is not surprising that I find this paper as extremely well written. I would just recommend that the author tries to publish the paper in a good academic journal; I think that Journal of Monetary Economics or Journal of International Economics can be suitable.

To discuss at least one point, I am curious about the robustness of the paper results with respect to the inclusion of shocks to the nominal exchange rate. DSGE models are notoriously known to have troubles in generating the realistic exchange rate volatility relative to the volatility of the macroeconomic aggregates – the so-called ‘excess volatility puzzle’ (Evans and Lyons, 2004, Evans, 2008). My understanding is that the consensus view (Devereux and Engel, 2002) is that modelling of ‘non-fundamental’ exchange rate fluctuations (noise traders,

or exogenous shocks to risk premia) is needed to replicate the volatility of the exchange rate relative to volatility of other macroeconomic variables (output, consumption).

I do not want the author to rewrite the paper and to include this type of shock to her model. But, as a possible hint for discussion during the dissertation defense; Yuliya could discuss her intuition of whether (and possibly how) her results would change if such type of shocks is added to the model (for example, I conjecture that the peg policy need not be so bad, if we allow for non-fundamental exchange rate fluctuations).

Comments of the paper

‘Bayesian Estimation of DSGE models under adaptive learning: robustness issues’

The second paper in Yuliya’s thesis deals with Bayesian estimation of three DSGE models under alternative learning rules (rational expectations versus adaptive learning). The adaptive learning economies differ with respect to the information sets used by agents to form their expectations and by the specification of the initial beliefs. By the comparison of these various specifications, the author tries to achieve robust results about the impact of the expectation specifications on the model likelihoods for various types of the models. I think that this is a nice idea, which can contribute to our understanding of the role of expectation formation both in macroeconomic modeling.

I would like to discuss the set of the following three topics.

First, I would like to discuss the choice of priors. It seems to me that prior distributions for some parameters are rather strict: for example, the learning parameter – *gain*, but also Calvo parameters or habit parameter, have low values of the standard errors. It would be interesting to know how this choice of prior influences the estimation results. I conjecture that especially the learning parameter may be rather influencing, while the prior on Calvo parameters basically helps to shape the posterior. Some discussion of the prior choice during the defense may be useful.

Second, Yuliya uses the MCMC algorithm from Dynare to perform Bayesian estimation of the parameters for 200 000 and 500 000 draws. A comment on properties of the MCMC sample would be useful for two reasons.

- First, large scale DSGE models are nearly non-identified and the likelihood function has many peaks and flat areas. The proper prior can somehow improve the situation, but still the posterior may be multimodal, and therefore the Markov Chain may not properly explore the whole posterior distribution.
- Second, Chib and Ramamurthy (2010) report that for large DSGE models (such as Smets & Wouters model), the autocorrelation function of the MCMC draws (based on the random-walk Metropolis-Hastings algorithm¹) exhibits a significant autocorrelation even after thousands of draws. One may therefore fear that hundred thousands simulation may not be enough to get a precise statistics about the posterior distribution.

¹ I am not a Dynare expert, but I think that Dynare uses the Metropolis-Hastings approach to the MCMC.

I would therefore welcome some discussion of author's assessment of the quality of the MCMC approximation to the posterior with respect to these two objections.

Third, the conclusion of the paper repeats in a nice and parsimonious way the finding of the paper. Still, I miss the Yuliya's own opinions on implications of these results for macroeconomic modeling. Should we abandon the DSGE models with rational expectations for models with adaptive learning (and leave the rational expectation models just for robustness check or academic curiosity)? Would the author recommend that policy institutions should experiment more with adaptive-learning macroeconomic models?

My final inquiry is about other statistics than the likelihood. I appreciate the fact that the marginal likelihood is a standard way of model comparison², but it may be interesting to know something about the errors of unconditional forecast of various model specifications (for example, to know during which periods each particular specification failed or did a good forecasting job). I think by looking at other statistics one can learn a lot. However, this topic need not be discussed during the defense; the author could take it as a possible hint for future research.

Similarly to the first paper, the second paper is publishable, and I think that *Journal of Economic Dynamics and Control* may be a suitable platform.

References:

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² The profession now uses the likelihood-based Bayesian techniques for estimation of DSGE models, and I think that the reason is often technical rather than genuine Bayesian (the simulation-based Bayesian techniques are able to approximate complicated distributions -- but have in mind that a blind application of these techniques may not always lead to good approximation of the posterior, cf. the Chib and Ramamurthy paper quoted above). However, using the results of Chernozhukov and Hong (2003), the simulation based MCMC techniques could be potentially applied to other types of estimators as well, even outside the Bayesian framework.