Opponent's Report on Dissertation Thesis

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Title of the Thesis:	Essays on monetary policy transmission
Type of Defense:	**DEFENSE**
Date of Pre-Defense:	February 22, 2017
Opponent:	**Univ.-Prof. Dr. Ansgar Belke**

Address the following questions in your report, please:

a) Can you recognize an original contribution of the author?
b) Is the thesis based on relevant references?
c) Is the thesis defendable at your home institution or another respected institution where you gave lectures?
d) Do the results of the thesis allow their publication in a respected economic journal?
e) Are there any additional major comments on what should be improved?
f) What is your overall assessment of the thesis? (a) I recommend the thesis for defense without substantial changes, (b) the thesis can be defended after revision indicated in my comments, (c) not-defendable in this form.

(Note: The report should be at least 2 pages long.)

Let me first state that I very much enjoyed reading the dissertation which consists of an introduction motivating the dissertation and giving a structure to it and five academic papers.

My more detailed remarks on the papers which are rather well-done in technical terms and are in the fields of meta-analysis in macroeconomics and real-time properties and forecasting are the following.

The problems are clearly set out and the papers meet their own aims formulated in the introductions - in terms of rigor of the analysis. The empirical models the author employs have the advantage of being quite standard, simple to understand (which of course does not imply "easy to handle") and well-structured. Moreover, the papers are written in a logically consistent and quite fluent style. Finally, the author

could in some cases be more explicit in his conclusions about what is left for future research.

The papers deserve their main merits for their empirical efforts. By definition, there is no new theory developed in the selected sample of papers. The way in which the author implements his estimation strategies appears to be carefully prepared and well executed. The relevant literature has been identified to large parts as well. There are only few typos and misspellings left, see, for instance, "Fourth paper" instead of "The fourth paper" on the first page of the abstract.

But the author also succeeds to present a sound policy-oriented analysis as far as this issue is concerned. Since the author applies different state-of-the-art views and methods in a careful and convincing way, I would rate the inherent academic merits of the paper as sufficiently high. Hence, seen on the whole, the collected papers represent an innovation.

Seen on the whole thus, I can recognize an original contribution of the author. And I consider the thesis defendable at my home institution without hesitations. What is more, it is immediately clear that the results of the thesis allow their publication in a respected economic journal. Hence, I was not at all surprised to see the papers already published or in a revise and resubmit mode in good (Economic Modelling, International Journal of Central Banking and Czech Journal of Economics and Finance) and very good (European Economic Review and Journal of Money, Credit and Banking) journals. I assume that the PhD committee has clarified the significance of the candidate's contribution relative to his co-authors to the respective papers. While the author cannot put the exact number of his contribution he credibly states that for all the chapters in his dissertation, the collection of data, computations and first drafts of all co-authored papers were done by him.

The topics dealt with in the dissertation are still under-investigated in the relevant literature and appear to be applicable to several other policy fields. What is more, the author comes up with interesting, highly differentiated and widely usable results.

Yes, I recommend the thesis for defense *without substantial changes*. And I do not propose to change or enhance anything for the thesis to be defensible at the regular defense. Instead I have been proposing some *questions to be dealt with before the*

final defense to clarify some issues. The candidate has in the meantime replied to them in a satisfying manner.

Ad section 2: In the context of publication selection issue, wouldn't it make sense to differentiate between empirical results published in academic journals and those published in the grey literature (where the significance of the results probably does not play the same important role as in case of publications in academic journals? Here, the candidate agrees that this is a very good point. Nonetheless, he is right in arguing that including working papers and mimeographs in meta-analysis might not help alleviate publication bias: if journals systematically prefer certain results, rational authors will already adopt the same preference in the earlier stages of research as they prepare for journal submission. He correctly alludes to the fact that limited research on this matter exists. A meta-analysis of 87 analyses by Doucouliagos and Stanley (2012) finds no difference in the publication bias between published and unpublished studies. Furthermore, his own robustness check with gathering data from working papers on monetary transmission mechanism confirms this result. On the other hand, he is correct in arguing in the cases where the sample of published studies is too small to conduct MRA, the inclusion of working papers might be the only way to proceed.

Ad section 4: Under habit formation an agent's consumption exhibits a form of hysteresis, in that his current consumption depends on his past consumption. This has been taken into account in the literature by quite a lot of authors, among them Maurice Obstfeld. In addition, the academic literature addresses, among others, habits and hysteresis in labour supply and habit formation and labor migration. The candidate, however, convincingly argues why he is not dealing with it explicitly in this section. In this context, he points to the fact that primary studies estimating habit formation in consumption do not generally cite the hysteresis literature. But he admits that one could indeed look at the habit formation through the lens of hysteresis theory. As a result, in the chapter on habit formation reference is now made to the relevant literature related to hysteresis.

Ad section 5: Election cycles in GDP data revisions are possible and probable, for instance with an eye on the excessive deficit procedure in Europe. What may be the evidence for the Czech Republic? See http://onlinelibrary.wiley.com/doi/10.1111/jmcb.12049/full. Them Cndidate argues

that otivation for governments to influence the figures might indeed be present. This notwithstanding, he argues that one would expect that greater pressure would be on the fiscal variables than on the GDP data. And he correctly adds that evidence for the Czech republic suggests that revisions to GDP are well behaved. Unfortunately, his short sample covering only nine years does not lend itself to study election cycles, but is indeed good avenue for the future research.

As expressed above, the five papers which constitute Marek Rusnák's thesis represent excellent up-do-date and innovative economic research. Hence, I give Marek Rusnák full permission for the final public defense.

Date:	April 25, 2018
Opponent's Signature:	
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