Opponent’s Report on Dissertation Thesis

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Title of the Thesis:	Banks' performance in low and negative interest rate environment
Type of Defense:	**DEFENSE**
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Opponent**:**	Prof. Jiří Witzany (VŠE)

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.)

The dissertation looks at performance of banks in the low or even negative interest rate environment characteristic for the decade after the global financial crisis. It consists of four papers, where the first two were published in impacted (Q4) journals, the second in conference proceedings, and the third is planned to be published.

The first paper applies various regression techniques on a panel of 629 banks from EU countries for the 2011- 2016 period investigating the relationship between the net interest margins (NIM) and market interest rates while controlling for a number of bank specific, country specific and macroeconomic variables. The results show a positive, and even concave, dependence of NIM on the short interest rate, and some other interesting dependencies, in particular a positive dependence on the concentration measured by the Herfindahl index. In my opinion, the empirical analysis has been done thoroughly and the results are interesting, however my main concern is the length of the time period spanned by the dataset. Since the basic time period is one year, there are only 5 annual observations for each bank. Even though the panel is large (629 bank) it is questionable whether it can capture the sensitivity of banks on the short-term interest rate which is for most banks the EUR interest rate observed only over 5 years. Interestingly, the short interest rate and even its squared value coefficients are significant, but there is still a concern of some kind of spurious regression result. The argument of the author responding to pre-defence reports that the same relationship between the short rate interest rate and NIM was found in other studies is not in my opinion sufficient. The other studies might use different datasets spanning longer periods and their result do not exclude spurious regression on the dataset with the limited timespan. Unfortunately, there is no detailed description or univariate analysis of the key explanatory variables, i.e. the short-term interest rate, the spread, and their squared values, that would give a better insight into the causal relationship with respect to the NIM target variable. It would be also useful to estimate the model with dummy variables for years, but without the short rate and spread variables (given by EUR rates for most countries in the dataset).

The second paper extends the second by enlarging the panel dataset with (526) US banks. The empirical analysis and the results are very similar to the previous paper with an additional observation on significantly larger average NIM of banks in capital-based markets (US+UK) than in the bank-based market (EU-UK). The time span is still 2011-2016 with annual observations and so there are the same questions as for the first paper. It is surprising why the author does not compare sensitivities of NIM on the interest rates and possibly other factors between the two markets. There is only the additional dummy variable for capital-based markets yielding a significant positive regression coefficient which is clear already from the simple descriptive statistic. The additional results provided by the author after the pre-defense confirm the higher sensitivity to the short-term rate in bank-based markets. Although the Hansen test rejects the null hypothesis, it would have been appropriate to report the result, or rather look at some alternative specifications, for example eliminating non-significant variables, or simply estimating the model separately for the two markets.

The third paper deals with a different interesting topic, it looks on the prepayment risk and provides an empirical evidence from the Czech banking sector in the low interest rate environment. This is a very importing issue that has recently become a challenge for banks providing retail consumer and mortgage loans. However, the paper is just a case study calculating possible negative effects of prepayments over different time periods and under different assumptions. The paper unfortunately does not propose or discuss possible general approaches how banks should deal with the prepayment risk.

The fourth paper investigates the impact of the introduction of the liquidity coverage ratio (LCR) on two liquidity indicators, namely liquid-assets-to-deposits and net-loans-to-deposits ratios. The dataset covers 707 EU banks and the period 2012-2018. The LCR limit became effective in 2016 starting at 60% gradually going up to 100% in 2018. Therefore, it is set to 0% in 2012-2015. The panel regression implemented in the empirical part confirms a positive impact of LCR on the liquid-assets-to-deposits ratio and a negative effect on the net-loans-to-deposits ratio as expected. Again, the relatively short time span of five years and low variability of the key explanatory variable (LCR) is a concern that could be discussed by the author in mode depth. However, it can be hardly fixed due to the given short period over which LCR was introduced.

To summarize, let me answer to the given key questions:

a) *Can you recognize an original contribution of the author?*
   Yes, all the presented papers apparently provide some original and interesting contribution of the author to the topic of banks' performance in low and negative interest rate environment.
b) *Is the thesis based on relevant references?*

Yes, the papers start with thorough reviews of literature and contain many relevant references.

c) *Is the thesis defendable at your home institution or another respected institution where you gave lectures?*
Yes, I believe that the thesis would be defendable at the Faculty of Finance and Accounting (VŠE).

d) *Do the results of the thesis allow their publication in a respected economic journal?*
Yes, the results of the first two and the last paper have been published or can be published in respected economic journal. The third paper (that has been published in a conference proceedings) has a form of a case study and should not be considered as a pure research paper.

e) *Are there any additional major comments on what should be improved?*
The research focuses banks' performance in low and negative interest rate environment, but there is almost no empirical comparison between the performance in the normal interest rate environment and in the low interest rate environment. It would be interesting if the author could some evidence what is special in the low interest rate environment.

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.*

Yes, I recommend the thesis for defense

	9. 2022
Opponent’s Signature:	
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