Opponent's Report on Dissertation Thesis

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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 consists of an introductory chapter and four additional chapters corresponding to four papers, out of which the first two were published in impacted journals (Prague Economic Papers – JCR Q4, International Journal of Finance and Economics – JCR Q4, SJR – Q2), one as an IES Working Papers, and the last one in a reviewed journal (Procedia Economics and Finance).

The first paper compares profitability and stability of Czech credit unions with European cooperative banks based on a dataset from the period 2006-2013. The conclusions are not favorable for the Czech credit unions and the authors discuss possible reasons, in particular an unsuitable business model, or regulations not allowing credit unions to offer mortgages and pushing them to more risky loan products such as consumer loans or real estate investment loans. I have the following comments to the empirical study:

1) The only variable used in the study to measure stability is the time varying z-score defined as

$$ZSCORE_{i,t} = \frac{ROAA_{i,t} + CAR_{i,t}}{\sigma(ROAA)_i}$$

where ROAA and CAR are time-varying, but the standard deviation is calculated over the whole sample period. In my opinion, this definition is problematic since it does not capture the dynamics of volatility which is a key factor of stability. In addition, the logic of the z-score is that it should be predictive in terms of default probability, i.e. not using forward looking information in the definition. It is true that this approach is mentioned in Lepetit and Strobel (2013) as one of five possibilities (the standard approach having time dependent standard deviation of the asset returns), but they point out that this approach is very similar to the approach when ROAA is also taken as an average across the whole sample, i.e. when the dynamics of the z-score is almost fully determined only by the development of CAR. In my opinion, by using the definition with fixed $\sigma(ROAA)$ the study omits changing volatility of returns as an important factor of the credit union stability dynamics. Hence, the question is why the author did not use a more dynamic z-score definition or an alternative stability measure such as NPL ratio etc.?
The author has provided a detailed response to this question in the appendix of the final version of the thesis discussing five alternative z-score definitions from Lepetit and Strobel (2013) and implementing the regression analysis with two selected alternative z-score definitions using dynamical volatility of ROAA definitions. The empirical results more-or-less confirm the results with the fixed ROAA volatility z-score definition, and so from this perspective the raised question can be considered as sufficiently answered.

2) Table 2.1 suggests CAR as a possible independent variable, but it should not be definitely used due to the definition of z-score and the discussion above. Indeed, it is not reported in the final model, but the logic of its elimination is not explained.
A brief explanation (that the CAR variable has been omitted as a variable used in the z-score definition) was added to the text.
3) The dataset consists solely of the credit unions that were active during the whole sample period 2006-2013. Does not this restriction cause a survivorship bias in the dataset? In particular, in case of Czech credit unions, those that have got into financial difficulties and defaulted in the period are omitted in the dataset. Are not those cases more important in terms of the stability analysis?
The author agrees that it would be better to have financial information on the unions that failed during the period and discusses the issue of unavailable data specifically in the case of three particular Czech credit unions that are known to have failed. However, I still miss some discussion of this data deficiency and of its possible impact on the estimations in the main text.

The second paper compares profitability of European cooperative banks and commercial banks in the low interest rate period 2009-2015. The main conclusions are that commercial banks are more profitable than the cooperative banks in the period, and, on the other hand, that the stability of the cooperative banks measured by the z-score (defined as in the first paper) is better than the stability of the commercial banks. The higher profitability of commercial banks appears related to the level of provisioning that is lower than in case of cooperative banks. The abstract and the conclusion emphasize that “commercial banks unjustifiably decreased their loan loss provisioning in a low interest rate environment in order to maintain their profitability” as if this was one of the main empirical findings of the paper. However, I do not find this conclusion empirically supported, it seems to be just a subjective judgement or a hypothesis worth of further research. One can formulate alternate hypotheses such as that the low provisioning of commercial bank compared to cooperative banks is

related to different regulatory or accounting rules, better asset quality, etc. Hence, the question is whether the paper does contain any empirical evidence supporting the finding on "**unjustifiably**" decreased provisioning of the commercial banks?
The author agrees that the claim was made based on the assumption of the same portfolio quality of both cooperative and commercial banks which is too strong without empirical justification. The formulations in the text were changed accordingly.

The third paper analyzes the efficiency of European cooperative banks and finds that smaller cooperative banks are significantly more efficient than the bigger ones, which is an interesting and important finding. The empirical study uses the method of Stochastic Frontier Analysis. The model is briefly described in equation (4.1), but some details seem to be omitted. In particular, the inefficiency term probability distribution needs to be clear specified in order to estimate the model. The formulation "inefficiency is positive and it is independent and identically distributed with normal error" seems to be contradictory – a variable with normal error is not always positive, and the following statement "The inefficiency term of the composite error can be estimated using the formula by Jondrow et al. (1980)" appears to be rather an escape formulation when the estimation is done by a software package (not specified in the paper) and the author does not exactly know what is inside. The set-up of the SFA model is also puzzling, normally an output variable is on the left-hand side (explained) of the equation while input variables are on the left-hand side. Nevertheless, in the paper set up the right-side involves both input (funds, labor) and output (loans) prices and volumes. Could the author explain better the logic of the SFA equation and compare it with the more standard DEA approach used for banks?
The outlined questions are discussed in great detail in the response of the author in the appendix and some additional explanations/corrections are also added to the text.

The last paper is a rather historical and analytical overview and analysis of the Czech credit unions development and their current situation. Although it is not a more involved empirical study, the paper contains just a few figure and statistical tables, I like it a lot and consider it as an important contribution to the discussion on possible regulatory reforms of the domestic credit unions. I have only one technical remark or question: Table 5.3 shows the development deposits and assets of the credit union sector in 1996-2001. If assets are interpreted as total assets being equal to total liabilities, and if deposits are just part of liabilities, then assets should be larger than reported deposits. However, this is not the case in 1997 and significantly in 1999. Could the author comment the numbers?
The author explained this surprising imbalance by the fact that the Czech credit union sector underwent a major crisis at that time and the equity of the sector as a whole was negative.

To summarize, the answers to the "mandatory" questions are as follows:

g) *Can you recognize an original contribution of the author?* **Yes, there are original and valuable contributions of the author.**
h) *Is the thesis based on relevant references?* **Yes, it is based on relevant references.**
i) *Is the thesis defendable at your home institution or another respected institution where you gave lectures?* **Yes, the thesis would be defendable at my home institution.**
j) *Do the results of the thesis allow their publication in a respected economic journal?* **Three of the four papers have been published in respected economic journals and the fourth one, after possible improvements, can be also published in a good journal.**
k) *Are there any additional major comments on what should be improved?* **Most comments and questions from the small defense were sufficiently answered or resolved in the final version.**

l) *What is your overall assessment of the thesis?* **The thesis can be recommended for defense.**

Date:	19.4.2023
Opponent’s Signature:	
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