

# Opponent's Report on Dissertation Thesis

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Title of the Thesis:	Three Essays on Financial Development
Type of Defense:	<b>DEFENSE</b>
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Opponent:	Martin Čihák Ph.D.

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 defensible 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-defensible in this form.

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

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**Original contribution.** As indicated in my earlier report, I find that the essays compiled in this thesis offer useful and original contributions to the considerable and expanding empirical literature on the intersection of finance, growth, and inequality. Given that chapters 2 and 3 are both joint with two coauthors, I had initially some questions about the extent of Mr. Mares's contribution, but I was pleased to read in the introductory chapter that according to Mr. Mares, his contribution to these papers was "substantial in all research stages, from drafting the ideas, data collection, analysis, drafting the paper, and responding to referees during the publication process." Moreover, I was heartened to read in the Advisor's Report from Prof. Roman Horváth Ph.D. that Mr. Mares "has served in [chapter 2] as a very valuable co-author rather than a research assistant" and "has been instrumental in this research and has contributed more than fully to [chapter 3]". Considering the above—and given that chapter 4 is solo-authored—the thesis as a whole provides more than sufficient evidence of Mr. Mares's original contribution.

**References.** The thesis does include many relevant references. The papers provide due recognition to a good share of the large literature on finance, growth, and inequality. At the same time, the list of references is far from comprehensive. That is understandable given that the literature is so vast and rapidly growing, but—as I mentioned in my earlier report—it may be useful to flag this selectivity upfront.

As pointed out in my earlier report, the thesis is relatively parsimonious, perhaps to the point of being telegraphic, on the theoretical front. The author concentrates on taking the empirical technique—BMA—to the data on finance and inequality. I do have sympathy for this heavily empirical approach, although I would have appreciated a clearer theoretical/conceptual discussion on the finance-inequality nexus. At this point, it may be easier to just refer to existing conceptual discussions in the literature.

As I also mentioned in my earlier comments, the thesis is short on references to policy-oriented literature. It would still seem to benefit from adding at least a few of those. (I have provided several suggestions in my earlier report.)

**Defensible.** I have little doubt that this thesis is defensible at a respected institution, including those where I gave lectures.

**Publishable.** Overall, the results presented in this thesis are clearly publishable in a respected economic journal. Chapter 2 has already been published in the *World Bank Economic Review* and Chapter 3 has been accepted for publication in the *Journal of International Money and Finance*. Chapter 4 has not been published, but seems publishable in a specialized journal of a similar caliber. Chapter 1 does not appear publishable in its present form.

**Remaining/outstanding/additional comments.** The author has nicely incorporated several of my earlier points on chapters 2, 3, and 4. For example, I have appreciated the author's response to my caution that net interest margin is at best only a partial proxy for efficiency. I see that the author has included a useful general discussion trying to conceptualize what the analysis is trying to measure as efficiency of finance and stress the limitations presented by the use of proxies. This said, I want to point out below some remaining/outstanding/additional comments:

- **Context: fintech and the changing financial landscape.** As I pointed out in earlier comments, key issues in the financial sector of the last decade included a massive rise of shadow banking and more recently the ascent of “FinTech”. The thesis seems to overlook these potentially game-changing, fundamental changes in the financial services landscape. As mentioned in my earlier report, I worry the thesis could come across as out-of-touch with these ongoing rapid developments. That is why I suggested to at least briefly discuss the effect of digitization/fintech innovation on financial services and inequality.
- **Policy takeaways.** In earlier comments, I have encouraged the author to clarify the relevance of the findings for policy makers. The discussion on policy implications is still the aspect where the thesis—across all chapters—leaves some room to be desired. Here are a few specific reactions:
  - **Linking the policy advice to the analysis.** In response to comments from me and the other opponents, the author has inserted ideas such as “support better allocation of savings”, “careful support of microcredit institutions with lending aimed at new business opportunities” and “establishing the ground for new financial products expanding the real economic opportunities rather than leverage”. These ideas sound broadly sensible (who would argue against better allocation of savings?) but they come out of the blue, do not seem well articulated, and could be better linked to the underlying analysis. Similarly, it is unclear what to make of the advice that “those

managing the regulatory changes in the financial industry should not underestimate the importance of the efficiency of financial intermediation”.

- ***Mindful regulation.*** The author ends chapter 1 by a call for “mindful regulation”. That could be confusing to readers—what is ‘mindful’ in this context? I doubt there are other authors suggesting ‘mindless’ regulation. Mindful regulation is not a widely understood term, and it would be useful to either clarify or re-word.
- ***Sweeping statements.*** The author offers some broad-brush statements (e.g., “Regulatory reforms often overlook the impact of the policies”) that would benefit from some examples, references to literature, or re-wording.
- ***Description of regulatory developments.*** The author’s descriptions of regulatory developments after the Global Financial Crisis (GFC) would also benefit from nuancing. The author states that “New regulatory waves followed, focusing primarily on the stability of the individual financial intermediaries and the overall systemic risk,” which is confusing. The author could briefly discuss the post-GFC regulatory reforms, noting that global reforms to the banking system are largely agreed, but implementation is stretched out and delays and setbacks are surfacing at national levels. The reform agenda on nonbanks still needs to be completed, including by further strengthening central counterparties and cross-border resolution provisions. Macroprudential policy frameworks and instruments are still work in progress, while international standards have been evolving.
- ***Future research.*** In responses to the comments from me and the other referees, the author has acknowledged some of the gaps and limitations in the presented analysis. It would be great to complete this and add possible directions for future research that could help address those limitations (and lead to more concrete policy analysis).
- ***Chapter 1.*** Further related to the above points, the newly added Chapter 1 could be especially strengthened. It summarizes the other three chapters and clarifies the author’s contribution, which is useful, but the summary of the other three chapters seems somewhat repetitive, partly duplicating what is already in the abstracts and conclusions of the respective chapters. In line with my earlier comments, summarized above, I suggest that the introductory chapter provides (i) a clear stage-setting, explaining the context for the work (including the changing landscape of financial services), (ii) a clear distillation of possible preliminary policy takeaways from the analysis (i.e., why should a policymaker care about this work), and (iii) an open discussion of the gaps and limitations in the presented analysis and therefore the possible directions for future research that could lead to more specific policy analysis.
- ***Covid-19.*** The thesis devotes considerable attention to the 2008–9 GFC, but it would be odd for a thesis issued in late 2020 not to include at least a brief reference to the Covid-19 pandemic and the associated economic shock, which has already exceeded the GFC and will likely be the economic crisis of our lifetimes. It has been testing the resilience of economies, societies, and financial systems (as well as serving as a shock towards further digitization). It is of course too early to evaluate the full effects, but it would be useful to at least include a clear up-front disclaimer that the studies in the thesis unfortunately do not reflect information from the Covid-19 period.

**Overall assessment.** It has been a pleasure to re-read this impressive tome. In my view, the thesis can be defended after revision indicated in my above comments. I believe it should be possible to address those remaining comments without much additional work.

Date:	September 14, 2020
Opponent's Signature:	
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