Opponent's Report on Dissertation Thesis

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Title of the Thesis:	Corruption, Tax Abuse, and Financial Secrecy
Type of Defense:	**DEFENSE**
Date of Pre-Defense	April 22, 2020
Opponent**:**	Prof. Niels Johannesen M.A., M.Sc., 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 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.

I am very happy with the changes to the dissertation and with the detailed responses given by Mr Palansky in his letter.

I therefore endorse that he be awarded the PhD degree.

Date:	4 December 2020
Opponent's Signature:	
Opponent's Affiliation:	Prof. Niels Johannesen M.A., M.Sc., Ph.D. University of Copenhagen

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Chapter 2: Value of Political Connections.

The chapter aims to identify empirically firms' financial return to their investment in political connections. The analysis uses a manually collected dataset on the political contributions of Czech firms as well as their financial performance.

The empirial strategy is to compare the return to equity of «connected firms», i.e. firms that have made contributions to political parties in recent years, to matched firms that have made no such contributions but are similar in dimensions such as size, location, industry and, importantly, pre-contribution profitability.

The main result indicates that there is a sizable return to party contributions: connected firms have returns that are significantly larger, in an economical and statistical sense, than matched firms making no contributions. The estimated return to political connections in the Czech Republic is higher than estimates from low-corruption economies suggesting that the strength of political institutions is an important determinant of the scope for rent seeking through political connections.

My main suggestion for improvements is the following: an alternative and possibly more transparent method for estimating the return to political contributions would be the event study method where the event is the contribution and a vector of event time dummies captures dynamics in profitability before and after the contribution relative to other firms (either firms that make contributions in other years or firms that never make contributions).

The chapter is very well executed. It has a clear research question and a well-explained methodology. It is generally well written and it is easy to follow the exposition. The relevant literature is cited appropriately and it is clear how the chapter relates to other work in the field.

I find that the chapter makes a clear contribution to the literature on political connections by:
- creating a high–quality dataset on political contributions for the Czech Republic
- using solid methodology to provide credible estimates of financial returns to policial contributions in a post-transition economy
- proposing refinements of this methodoly

The chapter is certainly publishable in a respectable academic journal.

Chapter 3: International Corporate Profit Shifting.

The chapter sets out to measure the global scale of corporate profit shifting, regional differences in exposure to profit shifting and associated revenue losses. The main data source is publicly available on foreign direct investment (FDI) at the country-level.

Following the methodology developed by UNCTAD (2015), the chapter estimates the empirical relation between the share of a country's FDI coming from tax havens and the average return on the FDI. It is well-known that many corporate tax avoidance techniques shift profit from high-tax economies to tax havens. Theoretically, this trafic will raise the share of FDI coming from tax havens and lower the recorded return to FDI. Under strict assumptions, the relation between the two variables thus yields a baseline return on FDI in countries with no exposure to tax havens, which can be used to infer how much exposure to tax havens erodes FDI returns and thus corporate tax bases.

The results suggest that gradients vary considerably across countries in different geographical regions and a different income levels. Transforming the estimated gradients into revenue losses, the chapter finds global losses in excess of $100 billion, which is consistent with other recent studies using different methodologies.

The main limitation of the empirical exercise is that it relies on macro-data and therefore cannot control for possibly confounding factors. For instance, if the share of tax havens in FDI correlates with the risk-profit of the underlying investments, it may create a correlation between with returns on FDI even in the absence of profit shifing.

I think the empirical identification could be enhanced by controlling for country fixed factors. Currently, the estimates are identified by both cross-sectional variation and time variation while it is possible – at least in principle, to include country fixed effects and identify from time variation only. In practice, it may be that the year-to-year variation is too limited for a standard panel model. In that case, it may be possible to estimate a model in longer differences, e.g. 2010-2018, correlating the <u>change</u> in the share of haven FDI and the <u>change</u> in FDI returns within countries.

Ultimately, the uncertainties are forgivable because the question of revenue losses from profit shifting is challenging and yet of first-order importance in international taxation.

This is another good chapter. It is well written and very thorough in explaining the analytical steps and interpreting the results. The comparison to the related literature is very useful. However, it would be useful if the authors could discuss whether the methodology is robust to the general critique delivered in Blouin and Robertson (2019, «Double Counting Accounting: How Much Profit of Multinational Enterprises is Really in Tax Havens»)

I find that the chapter makes a clear contribution to the literature on profit shifting by:
- providing estimates of profit shifting and revenue losses, globally and by country groups, applying a clear and transparent methdology
- comparing to other estimates using different methodologies and thus shedding light on where the remaining uncertainties are

The chapter is definitely publishable in a respectable academic journal.

Chapter 4: Secrecy Jurisdictions and the Countries They Harm

The goal of the chapter is to develop a bilateral measure of financial secrecy: an index capturing the importance of the opportunities for secrecy provided by a specific haven to specific country. The exercise relies on macro-data on cross-border financial positions from the IMF as well as the Financial Secrecy Index (FSI) which expresses the de jure transparancy of a jurisdiction.

The Bilateral Financial Secrecy Index (BFSI) thus combines the financial secrecy embedded in the laws of a counterpart jurisiction (FSI) and the relative importance of the counterpart jurisdiction in the external financial flows.

I have identified some problematic aspects about the procedure:

First, the two parts of the index are combined in a non-linear and highly opaque way. For instance, I suppose the ranking of the index across country pairs is quite sensitive to the methodological choice to raise FSI to the power of 3 while raising the bilateral component to the power of 1/3. It is unclear why this particular functional form is chosen.

Second, given the way the bilateral component is constructed it is unavoidable that major economies such as the United States and Japan will emerge as large «providers of secrecy». As a minimum, one would expect that the bilateral component would account for the «size» of the counterpart. Perhaps, one could estimate a gravity model and use the residuals, the part of bilateral positions that cannot be explained by country size and geography, instead of the bilateral positions themselves.

Third, the paper relies on assets observable in international portfolio statistics as an indicator of the «secrecy services» whereas the influential paper by Gabriel Zucman (2013, «The missing wealth of nations») shows that hidden wealth gives rise to positions that are observable from the liability side but not from the asset side. So, in a sense the secrecy index is based on the non-secret flows (e.g. to the U.S.) whereas it ignores the secret flows (e.g. to Bahamas).

Like the other chapters, this chapter is well motivated and well written. However, I find that important elements of the methodology are questionable.

In its current form, my guess is that the paper would be rejected at serious economics journals.

Conclusion

Overall, I find that the dissertation consists of two chapters (2,3) – both with good prospects for publication in international journals and clearly above the bar for a successful phd - and one chapter (4) that I do not think is publishable in a good economics journal in its current form and also, in my assessment, falls short of the usual standard in a phd dissertation.

I would recommend that chapter 4 is revised to somehow address the three comments made above before it is defended. As a minimum, I would like to see the points discussed verbally, but it would be great if some of them gave rise to improvements in the methodology or robustness tests.

Date:	16 April 2020
Opponent's Signature:	
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