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Evaluation of the Dissertation of
Ivo Bakota
CERGE, Charles University Prague

Dear Alena,

I am writing to report on my evaluation of the dissertation of Ivo Bakota at CERGE, Charles University Prague. To state my assessment from the very beginning, **in my judgment the thesis satisfies formal and content requirements for a PhD thesis in economics. I therefore recommend the dissertation for defense.** I will now justify this judgment.

Relationship to Ivo

I first met Ivo on a visit to CERGE and invited him to spend a semester at UPenn during his doctoral studies. During his stay I had the opportunity to discuss his dissertation with him, and I believe chapter 1 of his dissertation to a significant part was conceived during his stay. I also have had preliminary conversations with Ivo about chapter 2 and 3 of his thesis.

As for my qualifications to perform this evaluation, I have been a full professor of economics at the University of Pennsylvania since 2008 where I now hold the Walter and Leonore C. Annenberg Chair in the Social Sciences. I have been a Co-Editor of the *American Economic Review* from 2009 to 2011 and a Managing Editor of the *Review of Economic Studies* from 2018 to 2019, both top 5 general interest journal in economics. In addition, I edited the *Journal of the European Economic Association* from 2016-2017. Administratively, I have been a member of the University of Pennsylvania's School of Arts and Sciences Tenure and Promotion Committee from 2012-2014 and have served as the Economic Department's Chair from 2014-2016. I am currently also the lead editor of the *International Economic Review*, a leading general interest journal in economics published by the University of Pennsylvania in collaboration with Oxford University Press. Thus, I would conclude that I am capable of judging Ivo's dissertation, both from a scientific and from an administrative point of view. At the same time, I am not a close friend or collaborator of Ivo, and thus I cannot think of a conflict of interest that would prevent me from offering an objective opinion.

Evaluation of Ivo's Dissertation

Bakota's dissertation is located at the intersection of quantitative macroeconomics, financial economics and computational economics. It combines careful modeling, frontier computational methodology and informative quantitative analysis to answer substantive questions. I will now comment on each chapter, mainly focusing on the first one since I view it as most relevant for Ivo's future publication success, and also most aligned with my own expertise.

Firm Leverage and Wealth Inequality

In this chapter Ivo asks whether changes over time in firm leverage, assumed exogenous in the model, are important drivers of secular changes in wealth inequality in the U.S. My interpretation of the answer is that qualitatively, for the period of the 1980's and 1990's, the answer is yes, but that the quantitative magnitude of this channel is not overly large.

To arrive at these answers the author constructs a quantitative heterogeneous household models in which individuals face idiosyncratic unemployment and labor productivity risk, as well as aggregate wage and return risk, driven by aggregate shocks to total factor productivity and capital depreciation, as in the work by Kent Smetters, as well as Kubler and Krueger (2006). Firm leverage is exogenous and determined by the parameter lambda. Firms can deduct debt interest payments from taxes, which makes debt a preferred instrument of firm finance, but since leverage is not a choice, the firms cannot act on this advantage. Households can purchase both safe corporate debt as well as risky levered equity and are subject to exogenous shortsale constraints on both assets, as well as to a per-period participation cost in equity. Investors are compensated for the higher risk of equity with an equity premium; sufficiently large risk aversion as well as leverage generates a significant equity premium, which remains well below its empirical counterpart, though. The model is solved with a modified version of the Krusell and Smith (1997, 1998) algorithm, and the main though experiment is an exogenous change in firm leverage, and ensuing comparison of wealth inequality in a world with low and with high firm leverage.

I think this is overall a very nice paper that should find a home in a leading field journal in quantitative macroeconomics. Clearly, the main assumption that one needs to buy into is the exogenous change in firm leverage driving the quantitative results. I think it would benefit the chapter if the author would introduce a subsection somewhere in the paper discussing what could be the underlying truly exogenous changes in the economic environment driving the change in firm leverage (which of course in principle is an endogenous decision). In addition to listing the potential reasons (e.g. a change in corporate tax policy, changes in the composition of corporations by sectors), one argument that seems important to make is that these changes affect wealth inequality *only* through changes in firm leverage, so that observed leverage can be taken as unbiased proxy for the non-modeled underlying change in the economy. This will likely come up in any journal submission. I think it is also important for the welfare analysis; whether the change in leverage is welfare improving will very likely depend on the reason for the change in leverage in the first place.

I think the paper is overall well-written but could perhaps start with a stronger empirical motivation. Why not show a time series plot of firm leverage and wealth inequality, for your preferred measure of wealth inequality? That plot will show whether at least qualitatively the mechanism envisioned by the paper can work, and *for what period* it is likely to work. It will also establish from the very beginning what is the paper's preferred measure of wealth inequality. Once you have solved the model, you could import into this empirical figure the model-implied time series of wealth inequality. That would give a succinct summary of the entire paper in one plot.

The second part where the chapter could provide more details and/or more discussion of the findings is in the calibration section. That section should clearly state at what frequency the model is solved (is a model period a quarter, a year…). Given the frequency, then the chapter should put into context the calibrated parameters. For example, the time discount factor beta seems very low in table 1 for a quarterly model, and the variance of the depreciation shock in table 3 as well (but of course their interpretation depends on the time frequency at which the model is solved). I am fine with these values if that is what the model needs to match the data, but then it could be explained in more detail why. Furthermore, since the paper emphasizes that it solves for asset prices in general equilibrium, reporting the key asset pricing moments (e.g. the Sharpe ratio) and contrasting them with the data would be useful in section 1.3, especially since the paper wants to argue that the model is indeed successful along this dimension. These comments are easy to deal with, largely editorial and will go a long way towards anticipating concerns by potential referees at potential scientific outlets of the paper.

Capital Taxation with Portfolio Choice

This chapter uses a variant of the model employed in the previous chapter in order to investigate the positive and normative consequences of household capital income taxation when the government can tax different forms of capital income (returns on risk-free debt and income from risky capital) at differential rates. A benevolent social planner might want to do so for redistributive reasons since the share of wealth invested in risky assets increases with household net worth. That is, ceteris paribus, taxing safe asset returns at lower (possibly negative) rates provides income redistribution from wealth-rich to wealth-poor households. It does, however, distort the household portfolio decisions of households, and also impacts investment and thus capital accumulation in the economy, at least as long as the Modogliani-Miller theorem does not hold. One can then ask what is the preferred capital income tax policy for different groups of the population, and what would a benevolent government that values all members of society do? These are the questions the second chapter of this dissertation sets out to answer.

The chapter has essentially two parts. In the first part a simple portfolio choice problem is studied in which heterogeneous households invest in a risky and in a safe asset whose prices (and thus, effectively, its returns) are potentially taxed at different rates. I think this part of the chapter is useful for establishing useful theoretical benchmark results but could perhaps do so more concisely. First, it would have helped me to identify from the beginning along what dimensions households are heterogeneous with respect to (and whether there are 2 or many different types). I think propositions 1 and 2 are incarnations of a general result that says that as long as all households choose the same portfolio shares, and if so, it would be worth stating this. Of course, the key result is proposition 3 that gives one set of sufficient conditions under which the optimal ratio of taxes on the two assets is not equal to 1. As in the previous comment, I would conjecture that as long as there is household heterogeneity in equilibrium portfolio shares and there is some (explicit or implicit) value in redistribution in this model, optimal taxes on the two forms of investment should not be equalized. In fact, the intuition given for proposition 3 seems very general, as is the reason why the production efficiency result does not apply.

The second part of the chapter then conducts a quantitative investigation of this point, using the model developed in the first chapter. Since the model and calibration remains the same, my comments above about the calibration apply equally to this chapter. The quantitative results are overall plausible but could benefits from a couple of additional statistics. First, since utility is an ordinal concept, reporting welfare in terms of utils (see e.g. first row of table 9) is not very informative. Rather, it would be useful to calculate the welfare gains (in terms of consumption equivalent variation) of moving the tax wedge from 0 (first column) to positive values (the remaining columns of the table). Also, to document that Cf=60% is indeed optimal, it would be useful to add a column for a higher value of Cf (say 70%) and display that welfare is indeed declining for Cf values beyond 60%.

A final comment about both chapters 1 and 2. Although it is plausible to separate the two chapters for the purpose of the dissertation, for publication it could be better to combine the quantitative components of both chapters into a single paper and argue that the change in firm leverage could be an important driver of wealth inequality *and* optimal capital income tax reform (with the theoretical part of chapter 2 perhaps be separated into a stand-alone paper suitable for a field journal in public finance theory).

Avoiding Root-Finding in the Krusell-Smith Algorithm Simulation

This chapter contains a potentially very useful methodological contribution. Solving models with idiosyncratic and aggregate risk has become one of the standard challenges in quantitative macroeconomics, and recent large aggregate shocks will make sure this will continue in the future. At the same time, any interesting version of the model requires not only a meaningful consumption-savings choice, but also a nontrivial portfolio choice. This in turn requires to solve for market-clearing asset prices. Standard versions of the Krusell-Smith algorithm to solve these models consist of two steps. First, for a given law of motion for the aggregate state vector the household decision problem is solved and second, given household decision rules a cross-section of households is simulated over time, and based on these simulations the estimated aggregate law of motion is updated. Typically, along simulations for every period market-clearing prices in all asset markets are computed. The current chapter proposes to suspend this part of the second step, which is very costly, since it requires the solution of many nonlinear equations, as many as there are asset markets times time periods. It would reserve the requirement that all asset markets clear to a step in the algorithm when the aggregate law of motion has converged.

I think this is a very plausible idea, and the chapter shows that it results vastly improved running times of the algorithm. In my view it is important, though, to guarantee that at the converged aggregate law of motion along simulations asset markets indeed clear, and perhaps the chapter could add more documentation that it indeed does. In other words, although it is fine to relax the requirement to compute market clearing prices along the way, the final result should be a household decision rule and an aggregate law of motion, together with an equilibrium pricing function such that asset markets approximately clear. Otherwise we substitute an algorithm that takes long to compute a desired object with one that takes shorter to converge but delivers an object we might not be so interested in. I have no reason to believe this is a problem here, but the chapter could add error statistics that convince the reader this is indeed no issue. Since the obvious target for this chapter is a journal specializing in computational methods (e.g. the *Journal of Economic Dynamics and Control*), I am fairly certain such a journal would require the documentation of such error statistics.

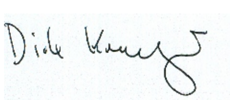
Absent this comment, this chapter could well become one of those papers that make a simple, yet incredibly useful computational point, and therefore be applied and cited very highly (as, for example, are Reiter's histogram method or Carroll's endogenous grid method, both papers published in specialized outlets, but with high impact and large citation counts).

Conclusion

Based on my overall evaluation of the chapters I am confident in reiterating that **the dissertation of Ivo Bakota satisfies the formal and content requirements for a PhD thesis in economics. I therefore recommend the dissertation for defense and** congratulate Ivo as well as his advisers for a job very well done.

I thank you for asking about my opinion in this important matter. Please feel free to contact me should you desire any additional information.

Sincerely yours,



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