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Re: PhD dissertation report for Ante Šterc

Dear Professor Anatolyev,

It was my pleasure to read the PhD thesis by Ante Šterc. Here, I provide a short summary of the thesis as a whole and my evaluation. Further below I also include more detailed summary of individual chapters and my comments that are intended to help to with improving the individual papers before submitting them to academic journals.

The thesis consists of three papers with second and third co-authored. In his thesis, Ante demonstrates a broad range of interests and skills. The first and second papers challenge the standard paradigm of full rationality and show that households do fall short of this high bar and costly consequences thereof. The third chapter examines the size fiscal multipliers under different taxation regimes. Whereas the second chapter is purely empirical, the first chapter combines empirics and quantitative modelling, the third chapter develops a fully fledged HANK using a state of the art solution method.

In my opinion, the second chapter is clearly the strongest. It represents a very strong contribution and requires no additional work before it can be submitted. I find the first chapter quite intriguing and original, however I expect that it will need substantial work before being submittable to an academic journal. Compared to the first chapter, the third chapter is much more polished, but I find the exercise somewhat less original.

Overall, I believe the thesis satisfies the general standards for PhD thesis in economics and I recommend it for defense.

Yours sincerely,

Filip Rozsypal

The views in this report are mine only and do not represent any position of Danmarks Nationalbank.

Chapter 1

Summary

The first chapter examines households' investment (or lack thereof) in risky investment funds. First, Ante documents investment fund holding patterns in Survey of Consumer Finances micro data. Then he develops a limited consideration model for household fund allocation problem. Finally, he shows that the welfare costs of sub-optimal assets allocation are potentially significant. This is an important question because personal savings play an important role in consumption smoothing over personal life-cycle and not participating in higher yield assets negatively affects consumption in retirement.

Comments

1. I would prefer if the theoretical exercise started from a clearly defined problem. Suppose that households maximize their expected next period utility and the choice is where and how much to invest subject to the initial wealth and the menu of expected returns on different forms of investment. This is a standard problem studied in asset pricing and there are many standard results that follow. For example,

   - as standard asset pricing teaches us, investment share will be a function of risk aversion and hence an endogenous variable. In CARA + normally distributed returns, it is the case that the optimal investment is independent on wealth, but this is a results, and thus it should be at least stated as such. It seems that W, the size size of the investment, is treated as exogenous. Is this an assumption or a result?

   - the standard portfolio allocation problem would dictate that households should consider the covariance of the assets when constructing their portfolio.

2. I am not sure about the way expense fees enter (unnumbered equation at the bottom of page 15). Doesn't it imply that for negative realization of fund return (negative r_j), households actually prefer to have high expense ratio because it lowers the loss? If so, I would prefer a specification where the fees are paid upfront, so $W_i(1 + r_j - \xi_j)$ instead of $W_i r_j(1 - \xi_j)$.

Minor comments

- When evaluating the model, Ante uses *realized* returns as a proxy for *expected* returns (see the description of Table 2, page 11), seemingly calculated on one year of returns. Also, the returns seem to be computed for different years? It is not clear whether past returns are ever a good proxy for expected future returns, but at least the time period over which the return is approximated should be consistent.

- It would be worth to explain how identification work in the estimation procedure.

- In Figure 1, do the numbers represent simple counts of people who have a specific type of investment? What if people have more than 1 type of investment? What about some weighting by invested assets?

- There are seven mutual fund types in Figures 1-5, but many more in Table 2.

- In the empirical exercise, is the information about W used to somehow identify risk aversion?

Chapter 2

Summary

The second chapter continues to explore the implications of imperfect consumer choice. The authors find that less sophisticated borrowers pay higher interest rates on their mortgages. This effect is substantial; it amounts to over $9000 on $100000 loan.
Specifically, the authors impute a measure of financial literacy from Survey of Consumer Finances to a mortgage dataset NSMO. In this combined dataset, the authors show that heterogeneity in financial literacy together with heterogeneous search effort lead to significantly difference in mortgage rates (after controlling for other household characteristics).

Comments

1. I would prefer if the main regression included fixed effects controlling for differences across finely defined geographical units. Such a regression would make more intuitive sense to me as it would be comparing borrowers with different financial literacy and search effort *in the same market*.

   Why is this important? For example, in rural areas there might be less competition among bank increasing the interest rates. If there is only one bank in town, both interest rates will be higher and it is not possible to do lots of shopping around for lower rate. If this is the case, increasing financial literacy or decreasing costs would not lead to lower rates.

2. If financially illiterate households are more likely to default, isn't it rational that they pay higher rates to compensate the bank for the higher risk? If so, the causality would not run the way the authors suggest and increasing financial literacy and search, as long as it does not lower default risk, would not lead to lower rates.

Minor comments

- What is up with the stars on estimated standard errors in Table 8?

Chapter 3

Summary

Following work of Kaplan et al., the paper starts with documenting large cross country differences in illiquid and liquid wealth to income share and resulting share of wealthy and poor hand-to-mouth (w/p-HtM) households. The authors then build a two assets HANK model to make three observations. First, show the differences between HANK, RANK and TANK models for consumption dynamics. Second, they examine the differences in consumption response conditional on wealth and HtM status. Third, they compare fiscal multipliers under alternative budget balancing rules.

Specifically, the flexibility of the model allows the authors to study the differences between the effects of changes in transfers (T) and taxes to consumption (τ^c), dividends (τ^k) and income (τ). They find that the effect of government spending is that largest when financed via debt with negative transfers. Finally, they argue that countries with less debt face lower potency of fiscal policy, which can be mitigated by decreasing income tax progressivity.

In my opinion, the strongest contribution of the paper is in extension of the paper to allow for various taxes and showing these differ in their aggregate impact. I find this a well executive exercise but I believe that publication prospects could be significantly improved, if the authors either provided broader discussion of the results or/and leveraged the model to draw some the cross-country heterogeneity from the motivation to uncover

Comments

1. The main finding of the paper is that government should use lump sum transfers/taxes to maximize the size of fiscal multiplier. Wouldn't some representative model also find that lump sum transfers/taxes lead to better allocations compared to taxes that distort agent's choices? The same for the finding about less progressive taxation?

   It seems to me that after going through all the trouble to developing and solving complicated heterogeneous agents model, the authors could with very small additional effort ask welfare questions that would make the paper much richer. Indeed it very well might be the case there there is a trade-off between the size of the fiscal multiplier and welfare. The model with imperfect insurance like the one the authors built is the right tool to explore it.

2. There is a disconnect between the motivation in section 3.3 and the model. The authors document the heterogeneity in household wealth/income measures across several European countries, whereas the model is calibrated on the US data and the exercise focuses at tax progressivity and level of debt, neither of the two were discussed in the introduction.

   My suggestion would be either focus on documenting the debt differences across countries (potentially beyond just level differences) in section 3.3 and drop the HtM, or (in the case the authors

were more ambitions) add another exercise where one could see the implications of different tax schemes *interacted* with additional dimension of heterogeneity that would be delivering differences between HtM levels. With such framework, one could for example predict cross-country differences in impact of different European-wide tax policies.

Minor comments

- The write-up of the model (section 3.4) could be improved:

    - All variables need to be defined explicitly first time they appear even if they are standard in the literature. For example, workers' productivity e (which seems to also appear in C.2 on page 109 as z) is never mentioned in the text.

    - From the definition of the value function, I can only assume that workers' productivity e and discount factor β_{it} follow some stochastic processes, but neither is defined. From seeing ρ_e in Table 11, I can only assume that e follows AR1. The fact that there are two values for β in Table 12 suggests that there is some Markov chain with untold degree of persistence.

- Authors claim that "we show RANK and TANK models cannot reproduce aggregate response *as observed in the data.*" (in conclusion, page 71, second paragraph, emphasis added). The authors use this as a motivation to build a HANK model. As far as I am aware, there is substantial heterogeneity in the empirical evidence about fiscal multipliers and the point estimates are surrounded by nontrivial confidence bands. As far as I can see from the Figure 20, apart from consumption response, all lines are qualitatively similar. So the authors should clarify how exactly they reach their conclusion.

    Furthermore, in 3.5.1 they write "Consumption response in the TANK model is *positive* and stronger and stronger than in the RANK model, which results in in impact fiscal multiplier larger than 1.", whereas the first sentence of 3.5.2 couple of lines below, they write "... HANK is the only one able to produce fiscal multipliers larger than 1 in addition to positive consumption multipliers. Are these two sentences consistent with each other?

    Finally, given that this result is quantitative and model-based, the calibration of RANK and HANK model should be at least briefly discussed.