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EVALUATION OF DISSERTATION

Dear Prof Anatolyev,

I hereby provide my evaluation of **Marta Cota's** dissertation "**Behavior and Complexity in Household Finance**". Below I provide brief summaries of the thesis as a whole and of each chapter, followed by suggestions to improve.

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Summary of Thesis and General Assessment

The thesis investigates two important financial decisions of households: saving for retirement and financing a home purchase through mortgages. The thesis highlights that patterns in micro data about those two household decisions can be explained by taking into account two non-standard features of households: extrapolative expectations and financial literacy (via its effect on search effort), respectively. For both financial decisions, the thesis further highlights that taking these household features into account is important when predicting the impact of policy measures. The thesis therefore impressively analyzes important questions in the field of household finance. In terms of methods, it spans a large set of techniques and analyses, ranging from detailed analyses of micro data using innovative approaches, to analytic theory, to computational simulation of dynamic models, and to policy analysis.

Chapter 1

Summary:

The paper shows that allowing for biases in income expectations can help explain why U.S. households do not contribute to defined-contribution pension plans even though standard theory predicts that it would be advantageous. The analysis proceeds in three steps. First, using data from the Michigan Survey of Consumers, the paper replicates

findings in Rozsypal and Schlafmann (2023) to show that people have biased income expectations. In particular, low-income households are too pessimistic about their future income while high-income households are too optimistic. Second, the paper proves analytically in a stylized model that such a bias leads pessimistic households to underuse illiquid savings. Third, the bias is incorporated into a realistic life-cycle model of liquid and defined-contribution savings, calibrated to the U.S. institutional environment. The results show that incorporating the expectations bias helps explain why young low-income households tend to postpone contributing into attractive retirement plans. Moreover, the paper argues that autoenrollment – a commonly proposed policy to overcome non-participation - will not have a strong effect.

Suggestions to improve:

1. Empirical Part:

a. Comparison to Rozsypal and Schlafmann (2023) (RS2023 from now on):

The paper essentially repeats the empirical analysis in RS2023. While it is clearly stated that these results are not new, the length of the discussion distracts from the actually new analyses. So, in my opinion this part of the paper could be substantially shortened and instead clearly state what the differences are between this paper and RS2023. What comes to mind is: The paper features a life-cycle setup while RS2023 is infinite horizon. The calibration of the bias parameters thus is different. Moreover, some additional analyses are conducted related to the probability of increasing income and losing a job. Are there more differences? Are those additional analyses used in the process for expectation formation?

Small comment: RS2023 is wrongly cited in the literature discussion: it finds overestimation of the persistence of income, not of persistence of income growth.

b. Overestimating Persistence vs Extrapolative Expectations vs Overestimation of Volatility:

The paper changes from calling the biased expectation extrapolative, to writing that volatility of income is overestimated. Moreover, when expectations are modelled the bias is implemented as overestimating persistence of income. This change of terms is a bit confusing and deserves clarification.

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2. Mechanism (Optimism for high income households):
The mechanism for why pessimistic low-income households do not contribute to illiquid accounts is very intuitive. Moreover, the stylized three period model nicely illustrates their behavior analytically. However, there is also the flip side for optimistic high-income households. What is the mechanism at play here? Why do they not contribute either? This is not completely clear yet in the current version. In the small model, wouldn't we expect too high of an illiquid share? I feel that it would strengthen the message of the paper if this was clarified.

3. Simulated behavior:
Figure 7 compares the average consumption levels of rational-expectation households and biased households over the life cycle. Why is consumption late in working life higher for biased households than under rational expectations? Don't these households have fewer retirement savings made early in their career and shouldn't they hence be forced to make up for that with high savings (and thus low consumption) later in life?

4. Policy Analysis:
a. Auto-enrollment:
The paper analyzes an auto-enrollment policy that forces households to contribute 3% of their income in the first period of working life. After this initial year, however, households are free to contribute (or not!) as they wish. There is no further friction, whether in the form of sign-up costs or switching cost etc. It is therefore not surprising that the analysis finds very small effects of this policy (it only constrains one period in life). Or are there some parts of the expectations mechanism that would indicate that having joined or not alters your expectations and hence behavior? I feel this deserves more discussion, otherwise the policy analysis feels a bit like a straw man.
b. Auto-escalation:
The introduction states "Consequently, my structural model offers insight regarding less distorting policies, such as auto-escalating contribution rates in retirement plans." (last sentence in section 1.1, p.9). However, there is no analysis about auto-escalating plans in the paper. Nor is it obvious to me why auto-escalating plans would be more beneficial for biased households. There is no

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friction in the model that would keep biased households to freely choose exactly such an auto-escalating structure themselves already. What is the setup that you have in mind that would make them better off? Is it mandatory auto-escalating contributions? But once it is mandatory, why not mandate (a version of) the optimal path? I feel that without detailed analysis / explanation this claim in the introduction should be removed.

Chapter 2

Summary:

This chapter empirically analyzes the relationship between financial skills, search behavior in the mortgage market, and the obtained interest rates. It employs the novel approach of stochastic data linkage to combine two data sets: (1) the Survey of Consumer Finances (SCF), which contains an objective score of financial literacy, and (2) the National Survey of Mortgage Originations (NSMO), which contains information on mortgage borrower characteristics, contract characteristics, and mortgage shopping behavior. The chapter finds simultaneous support for two channels through which financial literacy and search are related to mortgage rates: First, low-financial-skill households do not attain lower mortgage rates even if they shop more. This is consistent with a “fear of rejection channel” (Agarwal et al. 2020). High-financial-skill households, in contrast, obtain lower mortgage rates if they shop more. This is what the chapter coins “Effective Search”.

Suggestions to improve:

1. Stochastic Record Linkage:

The chapter uses a novel approach to link the two data sets. In particular, it stochastically imputes financial skill into the NSMO data set (so that it can be used as explanatory variable within that new data set). Since this imputation is at the core of the paper, and since this is a non-standard way of imputing information, I feel that the paper would benefit from more robustness analyses on this point. In particular:

- a. Could you provide more intuition behind which variables are driving the imputation? Could you show some evidence that the imputed financial skill is not just picking up a non-linear impact of the characteristics used in the imputation (e.g. by including higher order

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moments / interactions of the other characteristics alongside the imputed financial skill)?

- b. If you repeat the analysis from the SCF (the regression that links financial literacy score to observables (table 10) or shopping behavior to financial skill (table 12)) in the NSMO+ data set, do you get similar results as in the SCF?

2. Relationship between financial skill and late payments

The paper shows in section 2.7.5 that lower financial skills are correlated with higher likelihood of late payments. This raises two questions:

- a. How should we think about this result? The paper only considers the demand side of the mortgage market, arguing that the characteristics of the mortgage would hold constant the supply side. Implicitly, the discussion of the results seems to assume that all borrowers are observationally equivalent after controlling for mortgage characteristics and hence can obtain the same rates. But isn't this result in line with a scenario where the lenders correctly view borrowers with characteristics correlated with low financial literacy as riskier? They would then rationally offer higher rates to these borrowers to account for the higher risk.
- b. Why is there no interaction term between financial skill and search effort in the regression for table 17? Wouldn't it be useful to separate the "fear of rejection" households from the "effective searchers"?

3. Write-up

Some of the write-up of this chapter is slightly confusing, in particular:

- a. What is the question regarding search effort in the SCF (from the motivation for the imputation it sounds as if there was no shopping behavior question in the SCF, but then in section 2.4.2 / table 12 there is a shopping behavior analysis)?
- b. Section 2.5 sounds as if it would introduce the NSMO data set, but then shows descriptive statistics for groups of financial skills, even though financial skills are only part of the NSMO+ data set and their imputation is only described later in the paper.

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Chapter 3

Summary:

This chapter builds on chapter 2 and sets up a structural model to rationalize the observed patterns in the data and to conduct counterfactual policy experiments. The model is a model of mortgage search (similar to a Diamond- Mortensen-Pissarides search model, applied to the mortgage market) with endogenous investment into financial skills. The model formalizes the “effective search channel” proposed in chapter 2: Households endogenously invest in financial skills which leads to heterogeneity in search costs. More financially skilled households then optimally search more, which allows them to lock in at lower mortgage rates. The model predicts that easier access to mortgages through online lenders (modelled as lower search costs for all households) increases homeownership and reduces consumption inequality. However, it at the same time increases the delinquency rate through a decrease in financial skills in the pool of mortgage borrowers. Lower mortgage rates (modelled through an exogenous shift of the mortgage rate offer distribution), on the other hand, increase the search effort and hence increases consumption inequality in the population.

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Suggestions to improve:

1. Financial shocks vs risk-based mortgage pricing

The model features financial shocks that only homeowners face. Those are shocks that force homeowners to go back to renting. Importantly, the probability of such a shock occurring is modelled as a direct function of financial assets and financial skills. This is plausible, as both higher liquid wealth and higher financial skill in reality can be expected to make it less likely that a household comes under financial distress. However, if households differ in their likelihood of default, shouldn't lenders internalize these differences? In the model lenders do not take the probability of default into account when making a mortgage offer. Instead, there is no risk-based mortgage pricing and all households, independent of financial skill and wealth (and hence likelihood of default), face the identical, exogenous distribution of mortgage rates.

This poses a problem of reverse causality: In the model, being hit by a financial shock means going back to renting and hence losing the current mortgage. Note that the difference between renting and owning is modeled exclusively through lower costs as renters only become homeowners if they receive a mortgage

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offer that makes mortgage payments sufficiently low compared to rental costs. Thus, going back to renting is particularly costly for low mortgage rates. Households who have secured such rates therefore have large incentives to reduce the risk of financial shocks through building up wealth and financial skill. In reality, however, mortgage lenders likely engage in risk-based pricing such that low-risk households are offered lower rates. In other words, the model predicts that low rates trigger incentives to reduce risk, while in reality low risk should lead to lower mortgage rates.

While a full model of the supply side seems out of scope for the analysis in the current chapter, I feel that this point should be thoroughly discussed.

2. Policy analysis:

a. Effects on search intensity and financial skill accumulation:

The policy experiments are very interesting and highlight the complex interplay of search costs, costs of skill accumulation, the interest rate environment, and the endogenous behavior of the agents. Having said that, the discussions of the underlying mechanisms are very short in this part of the chapter. The tables (tables 21-25) provide many details on the effects of the policies on the equilibrium outcomes. The text, however, does not help much to understand the underlying mechanisms. Could you provide decompositions of these effects, e.g. though highlighting changes in the policy functions vs the effects of the endogenous evolution of state variables over time?

b. Composition effects

All policy experiments change the composition of the population in terms of homeowners vs renters. While the experiments regarding financial education and search costs show the composition effect in the tables, this is not the case for the alternative interest rate scenarios. However, my prior would be that the interest rate distribution has a first order effect on the share of homeowners (as moving from renting to owning is a pure cost minimization exercise: for constant rental costs (as modelled in the chapter) the lower the interest rate distribution, the more likely it is that a renter draws a mortgage rate that makes owning cheaper than renting). Since part of the results refer to the

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consumption inequality and skill gap between renters and owners, I would expect this composition effect to play a large role for these statistics. It would be great to clearly separate those composition effects from the effects of changed incentives within the two groups.

c. Figure 37

The Lorenz curve for baseline and low interest rate environment seem effectively identical. How does that square with the description in the text?

3. Role of income risk and mortgage size:

The model features income risk through two levels of productivity and a mortgage size that seems tied to the income level. What exactly is the role of those two components? It would be great to discuss this in more detail in the description of the model.

Recommendation

All of the above comments are suggestions to improve the three chapters of the dissertation. However, none of these suggestions are binding recommendations that would need to be addressed for the dissertation to satisfy the requirements of a PhD thesis in Economics. **In my assessment, the thesis satisfies the formal and content requirements for a PhD thesis in Economics. I therefore recommend the dissertation for a defense.**

Sincerely,



Kathrin Schlafmann