Expect the Best and Run into Debt: The Effect of Biased Expectations on (Over)-Indebtedness*

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Abstract

Household over-indebtedness is increasing worldwide. This study investigates one possible reason for this increase: biased income expectations. Thereby, we refer to the "permanent income hypothesis," which predicts that individuals borrow more today if they expect a higher income in the future. We collect data from an emerging country where over-indebtedness can be devastating both on the micro and the macro level. Furthermore, our sample of poor, rural households in Thailand is exposed to a high degree of uncertainty, which makes expectation formation prone to behavioral biases. Controlling for various household characteristics, while also employing several distinct measures for biased expectations and over-indebtedness, we find a strong and robust relationship between the two. In an additional lab-in-the-field experiment, we explicitly find that overconfidence is related to overborrowing.

Keywords: Household over-indebtedness; Lab-in-the-field experiment; Emerging markets

JEL: D14; D84; D91

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1 Introduction

For households, taking out debt is a valuable tool to smooth consumption and often a necessary precursor of private investments. However, as consumer indebtedness is significantly increasing worldwide, there is widespread concern about when it turns detrimental. Specifically, when households become over-indebted, household well-being and consumption are threatened. Furthermore, household over-indebtedness poses a serious threat to the stability of the financial system as a whole; for example, as experienced during the U.S. financial crisis in 2007-08.

Emerging market economies are especially at risk of low growth and even financial crises when the level of household debt is high, as their institutions and financial regulations are weaker and income inequality is higher (IMF, 2017). Therefore, understanding the factors and reacting to the consequences of over-indebtedness are crucial for improving living conditions while also ensuring a stable development of emerging economies. The determinants of over-indebtedness are, however, not well understood. Building on the classical "permanent income hypothesis," this paper studies one potential driver of over-indebtedness: upward biased income expectations.

Thailand is, on the one hand, exemplary for an emerging market but, on the other, outstanding when it comes to household finances: Financial inclusion is comparatively high, with four out of five persons participating in the formal financial system. However, simultaneously, outstanding household debt has increased to over 78.03% of the country's GDP. This constitutes an increase of almost 37 percentage points since the beginning of the 2000s (Mbaye et al., 2018) and makes it the emerging market with the highest household debt to GDP ratio in the world (see Figure 1). Given these numbers, it is hardly surprising that both local policy makers and international institutions agree that (over-)indebtedness is a growing problem in Thailand (Tambunlertchai, 2015).

We investigate the potential effect of biased expectations on over-indebtedness using extensive survey data on the financial situation and financial behavior of one of the most vulnerable populations in Thailand: rural households in the North-East. In our regression analysis, we control for various household characteristics and shocks that households faced, which reduces reverse causality concerns. A crucial part of our survey was to collect objective and subjective data on potential symptoms of over-indebtedness. This allows us to construct different debt indicators.[1] A major contribution to the literature is that we relate these different debt indicators to sophisticated measures of subjective income expectation biases. We employ two alternatives to capture upward biased expectations, one quantitative and one qualitative measure. Further, we carried out a lab-in-the-field

[1] It is still a highly debated topic how to measure over-indebtedness and there is no clear-cut answer on the right method of elicitation.

experiment to explore the causal effect of biased expectations on overborrowing.

Our survey results show that there is a strong and robust relationship between upward biased expectations and indebtedness as well as over-indebtedness. Objective debt measures are relatively more affected by the quantitative expectation bias measure while subjective debt measures are rather affected by the qualitative bias measure. The results are robust to various specifications and become more precise if we exclude parts of the sample that may had difficulties understanding the questions on eliciting future income expectations. Another main outcome is that higher perceived certainty about the income expectation is positively related to over-indebtedness. Rural households are exposed to a highly uncertain environment, hence, being too certain actually harms them. In the supplemental experiment, we exogenously vary income expectations via two treatments that vary the level of self-confidence of the respondents. We find that overconfidence is related to more spending and overborrowing in our experimental setting. However, most probably due to "noise," our treatments themselves have no impact on overborrowing, which is why we cannot claim a causal relationship of biased expectations on overborrowing. These results are not driven by presumably confounding factors that the treatments could have affected and are relatively robust. Rather, we find evidence for "sticky" overconfident beliefs, which also points to a high level of perceived certainty in our sample.

Households' borrowing behavior around the world is still puzzling in various aspects and often hard to reconcile with standard neoclassical and behavioral models. Zinman (2015) argues that one main reason for many unresolved puzzles is the fact that household debt is vastly under-researched within the field of household finance (which itself is under-researched in financial economics). Admittedly, a vibrant literature on measuring over-indebtedness is emerging (for example D'Alessio and Iezzi, 2013; Keese, 2012; Schicks, 2013). In contrast, the determinants of over-indebtedness are still mostly unidentified. Our paper contributes to closing this gap by focusing on biased expectations as one likely cause of over-indebtedness.

Specifically, our study touches on three strands of literature: First, the literature on household over-indebtedness in emerging economies, second, research on the behavioral biases in financial decision-making and debt illiteracy, and, third, the literature on eliciting and using subjective expectations data. There are at least two reasons why the effect of biased expectations on over-indebtedness should be explicitly studied in an emerging market setting and why findings from "WEIRD"[2] populations might not translate to those rural populations. First, financial literacy is substantially lower, which implies lower debt literacy and, thus might hamper expectation formation on financial matters. For example, Lusardi and Tufano (2015) find that debt illiteracy is related to higher debt burdens and

[2] Western, educated, industrialized, rich and democratic

the inability to evaluate the own debt position. Burke and Manz (2014) experimentally show that economic illiteracy increases financial forecast errors. Second, our study sample faces higher uncertainty regarding their future incomes in two ways: through the generally high level of macroeconomic volatility in emerging markets and through individual shocks common for poor, small-scale agricultural households (see Loayza et al., 2007; Klasen and Waibel, 2015). A more volatile economic environment requires more individual belief formation, which makes biased expectation formation more likely (see for example Johnson and Fowler, 2011) and at the same time more dangerous.

Our work is most closely related to Hyytinen and Putkuri (2018) and Grohmann et al. (2019). The former establish a correlation between Finnish households' overborrowing and extreme positive forecast errors on the future financial situation. They show that households exhibiting high positive forecast errors are more likely to overborrow than households exhibiting smaller errors. They elicit households' forecast errors regarding their financial situation in general not regarding their future income, which gives rise to issues of reverse causality. Grohmann et al. (2019) conduct a very similar experiment to ours in Germany and underpin their results with data from the German Socio-Economic Panel (GSOEP). They find a causal link between overconfidence and overborrowing in the lab within a student sample and a relation between return expectations and household debt in the panel sample. In contrast to Hyytinen and Putkuri (2018), they explicitly ask about income expectations. As our study differs from these two, it contributes to the literature by (i) analyzing the research question in a setting where expectation formation is generally more difficult and over-indebtedness bears more severe consequences; and (ii) eliciting income expectations and over-indebtedness more precisely.

The paper proceeds as follows: Section 2 presents the survey data we use and explains how our variables of interest are constructed. In section 3, the estimation strategy is outlined and survey results are presented. Section 4 describes the experiment and its results, while section 5 concludes.

2 Data

This section introduces the data elicited during the survey and explains how the main variables of interest are derived, i.e. biased income expectations. We develop two alternative indicators each approximating possible biased perceptions about the future development of household income.

Then, we turn to explain the debt measures used in the analysis. As such, the concept and measurement of over-indebtedness is debated, with no consensus on a single indicator that measures it precisely. This would indeed be very hard to achieve given the multifaceted ways indebtedness can occur. Hence, we provide an overview on the distinct debt measures used as dependent variables and argue that they portray households' financial situations accurately in our sample.

2.1 The Thailand Vietnam Socio Economic Panel

The survey was conducted in Thailand in November 2017 and is an add-on project of the Thailand Vietnam Socio Economic Panel (TVSEP).[3] The TVSEP has been conducting yearly panel surveys in rural Thailand and Vietnam on a regular basis since 2007, with so far recurrent surveys in 2008, 2010, 2011, 2013, 2016, and 2017.

The TVSEP survey captures the living conditions of households in rural areas that are largely engaged in agricultural businesses. It focuses on factors affecting households' vulnerability to poverty. Among others, the survey includes socio-economic characteristics of every household member, sections on household consumption and savings, crop farming, livestock rearing, and, in particular, questions on exposure to shocks and anticipated risks. Furthermore, each wave captures additional topics of current research interest. About 4000 rural households in 440 villages across six provinces in Thailand and Vietnam are interviewed for the survey. The sample is set to represent the rural population in these two countries while households living in urban areas are deliberately excluded. To obtain a representative sample, a three-stage cluster sampling is used. The procedure is described in Hardeweg et al. (2013).

Our study is conducted in only one of the TVSEP provinces in Thailand, Ubon Ratchathani, which borders Cambodia and Laos (see Figures 2 and 3). Our sample consists of about 750 households in 97 villages. For the majority of our analysis, we concentrate on our own survey, adding data from the 2016 and 2017 general TVSEP survey.

[Figures 2 and 3 about here]

With our study, we want to gain new insights into over-indebtedness within a vulnerable population. Therefore, our survey includes extensive question batteries on over-indebtedness (see Sub-Section 2.3), savings, financial literacy, borrowing behavior in general, optimism, and income expectations (see Sub-Section 2.2). In addition, we collect data on health, subjective well-being, personality traits, and risk preferences. We use established items to assess these data. For example, personality traits are measured using the short version of the Big Five Inventory "BFI-S" (John and Srivastava, 1999; Gerlitz and Schupp, 2005). In order to test how financial knowledge affects households' debt situation, we develop a broad financial literacy score, which not only encompasses numeracy

[3] See https://www.tvsep.de/overview-tvsep.html

but also questions on financial behavior and attitude. The score is similar in style to that developed by the OECD (OECD, 2018). Furthermore, we construct a score for risk preference out of two questions: The first one asks whether the person is in general fully prepared to take risks and the second question specifically asks for risk-taking behavior in financial decision-making (i.e. investing and borrowing). Self-control is assessed using the well-established scale by Tangney et al. (2004).[4] Adjusted to the low numeracy within the sample, we add a phrase to each numerical value on questions involving scales.

2.2 Income Expectation Biases

In order to obtain an income expectation bias measure, we must elicit income expectations in the first place. Expectations play a central role in the economic theory of household decision-making, for example, determining saving, borrowing, consumption (Friedman, 1957), and occupation choices (Becker, 1964). Manifold research has tried to predict this choice behavior based on expectations, yet these are challenging to empirically elicit correctly.

2.2.1 Eliciting Income Expectations

Expectations from Former Income Realizations The traditional way of elicitation - referred to as revealed preference analysis - assumes that individuals have *rational expectations* (Dominitz and Manski, 1997; Manski, 2004). Furthermore, both the researcher and the respondent would have to have the same information set (Guiso et al., 2002). Given these strong assumptions, we decide for two alternative elicitation methods, which are explained in what follows.

Qualitative Expectations Questions The first way is to elicit expectations via qualitative questions, e.g. using Likert scales for questions on future expected events. We use such a measure in our analysis to confirm the results of Hyytinen and Putkuri (2018), who use Likert scales to construct their *forecast error* in predicting future income. Again, this approach suffers from two main drawbacks: First, answers might not be comparable across respondents and, second, response options are too coarse and leave room for responses different from what is proposed.

Subjective Income Expectations The second way suggested by Dominitz and Manski (1997) is to elicit *probabilistic expectations*. This approach is particularly useful for calcu-

[4] As more than 80% of our respondents are partly or fully responsible for household finances, we assume their individual characteristics to possibly affect the household's debt situation more than those of any other household member.

lating individual cumulative distribution functions and moments of the relevant variable (Attanasio, 2009). As we elicit expectations within a rural sample in an emerging economy, we re-phrase our percent change questions in a way similar to "how sure are you" and use visual aids to make the concept of probability more comprehensible.[5] Thereby, we address the concerns of Attanasio (2009) and Delavande et al. (2011), who state that the concept of probability might be hard to convey in contexts where people have low levels of education.[6]

To check whether respondents adhere to the basic laws of probability, we first ask them how sure they are that it will rain tomorrow and how sure they are that it will rain within the next two weeks. They can indicate their answer by putting between zero and ten of the marbles that we gave them beforehand into a cup, with zero marbles meaning they are absolutely sure it will not rain and ten marbles meaning they are absolutely sure it will rain. There are 182 out of 748 respondents (24.33 %) who do not answer based on what the laws of probability would tell us. This is a substantial share of respondents, most likely caused by the low educational level in our sample. In the subsequent analysis, we run our regression both with and without these individuals.

After this "warm-up" exercise, we ask respondents how certain they are that their monthly household income in the next twelve months will be in a pre-defined range. We use income quartiles from the 2013 TVSEP wave to pre-determine the four bins to which respondents allocate their ten marbles. The four bins range between 0 - 3,300 Thai Baht (THB), 3,300 - 8,100 THB, 8,100 - 16,590 THB, and 16,590 - 921,000 THB.[7] Respondents distribute their ten marbles based on how certain they are that their future monthly income will lie in each specific bin.[8] We assume that respondents do not give random answers just for the sake of finishing the interview, but provide reasonable estimates for their expected future monthly income. Hence, with this information, we are able to calculate the individual cumulative distribution function (CDF) for the expected monthly income as we interpret the number of marbles distributed between the cups as points on their individual CDFs.

We then fit a subjective income distribution following Attanasio and Augsburg (2016)

[5] Studies dealing with these kind of expectation elicitations include, among others, Attanasio and Augsburg (2016), which studies income processes in India, McKenzie et al. (2013), which investigates income expectations of Tongans if they were to migrate to New Zealand, and Attanasio and Kaufmann (2014), which elicit income expectations among high school students in Mexico.

[6] The average respondent in our sample only attended school for six years.

[7] The range of the last bin is very broad. Compared to the maximum monthly income respondents state, we find that only two respondents expect an income as high as 921,000 THB. All other maximum income guesses range between 0 - 300,000 THB. In order to avoid artificially high expected median incomes, we restrict the range of the last bin in our calculation of expected median income to a maximum of 300,000 THB.

[8] The enumerator places four cups in front of them, each labelled with a different income range and makes sure that all marbles are allocated at the end of the exercise.

and assume a piecewise (i.e. per cup) uniform probability distribution. This enables us to calculate a specific expected mean and median income, as well as the standard deviation, for each household.

[Table 1 about here]

Respondents allocate the number of marbles to the cups as a function of their underlying subjective probability to earn income in the specific income range. The average distribution of marbles per cup, i.e. the average implied probabilities to earn income in the respective income quartile is shown in Table 1. Additionally, Figure 4 presents the probability density function of expected income in our sample. The average respondent's expected income distribution is skewed to the right; that is, on average, respondents believe it is more probable that their average monthly future income is in the lower cups.

[Figure 4 about here]

We must ensure that the elicited expected income is not at odds with actual realized income. As measure for income, we use the actual realized income in 2016 and an income measure averaging the perceived income in a very bad and a very good month. Correlations between these measures are always statistically significant and range between 0.27 and 0.33, which is encouragingly high given that the correlation between actual income in 2016 and 2017 is only 0.48. As Attanasio (2009) proposes, we check how the subjective expected median income covaries with respondents' observed characteristics in our sample, particularly with the household composition, educational achievement, and realized income. Beyond the already stated influence of income, household total education affects the elicited median income significantly and positively. A little ambiguous, however, is the effect of the household composition on elicited income: While a higher number of elders in the household is associated with a decrease in income (albeit not significant), more workers in the household also seem to decrease elicited household income (results available upon request).[9]

2.2.2 Defining the Bias

We develop two kinds of expectation biases, one based on the subjectively elicited expected income and the other using qualitative income expectation measures as Souleles (2004) and Hyytinen and Putkuri (2018) apply them.

[9] Reflecting on this last result, we assume that households with more working members are, in general, poorer and have less stable incomes. There is a tendency in Thailand to abolish multi-generational households in favor of small family homes, which is however only possible if income is high enough and stable.

We define respondents whose expected median income ($Inc_{i,t+1}$) is larger than their actual income ($Inc_{i,t}$) to be upward biased:

$$Biased\ (=1)\ if\ Inc_{i,t+1} - Inc_{i,t} > 0 \tag{1}$$

While we cannot formally test rationality of expectations with our subjective expected income data,[10] we assume that the difference between expected income in 2018 and realized income in 2017 is partly due to respondents being overconfident of what they will earn in the future. This assumption is based on studies finding that expectations about various future outcomes may tend toward being positively biased (see for example Zinman, 2015).

The second expectation bias is derived following Souleles (2004) and Hyytinen and Putkuri (2018). We make use of the available panel data and combine categorical answers to the question on "How do you think your average monthly income will develop in the next twelve months?" ($E_{i,t-1}$) asked in 2016 (one year prior to our survey) with responses to the statement "the household is better off than last year" asked in 2017 ($A_{i,t}$).[11] As in Hyytinen and Putkuri (2018), the difference between these two questions is coined financial forecast error:

$$Financial\ Forecast\ Error = E_{i,t-1} - A_{i,t} \tag{2}$$

A positive forecast error occurs if the expected household situation is better than the realized one and a negative if the opposite is true. The forecast error we use in the main analysis is derived at the household level, meaning that the respondent may not be the same for all three data points. Therefore, we re-run the analysis for a sub-sample with only identical respondents, which does not change the results. We assume that the household's qualitative assessment regarding its own development stays similar for a time period of two years and, thus, is able to explain indebtedness in 2017. There are two reasons encouraging this view: We are able to control for a rich set of socio-economic variables that capture household formation and, as incomes are rather stationary, expectations may change slowly, too. Our two bias measures differ in nature. While the forecast error is based on a qualitative assessment about the household's financial situation, the expected income bias is derived from respondents' income elicitation exercise and the

[10] For example, because we lack data about realized income in 2018, the year after we asked for expected income, and we do not know (yet) about shocks households endured during that time.

[11] Answer options range on a scale from 1-5. For the question asked in 2016, one means "decrease a lot" and five "increase a lot." The question asked in 2017 ranges from one being "much worse off" to five "much better off." A valid criticism regarding the measure asked in 2017 is that it does not explicitly refer to the financial situation of the household. However, we informally ask how respondents understand the question and the majority of them think about household development in economic terms.

actual household income. Last, we also account for perceived income uncertainty in our analysis. In addition to asking respondents how they think that their income will develop over the next 12 months, we ask how certain they are that this income development will truly become reality. Being too certain about expectations can be a form of biased expectations called "over-precision" (Moore and Healy, 2008). In addition, we calculate the inter-quartile range of elicited subjective future income to account for uncertainty.

2.3 (Over-)Indebtedness Indicators

We distinguish between households that are indebted and those that are over-indebted. These measures mainly differ in that the former contain continuous variables and the latter comprise dummy variables, turning one if the specific debt measure passes a certain pre-defined threshold. As already indicated, there is not a consensus regarding a single set of indicators measuring (over-)indebtedness precisely.[12] In general, all measures share economic, social, temporal and psychological dimensions (D'Alessio and Iezzi, 2013): The amount of debt exceeds income over a medium- to long-term time horizon and the household is not able to fulfill its debt commitments without increasing its income or lowering its standard of living, which might lead to stress and worry. So-called objective debt measures relate to the household's debt service capacity, subjective measures rather emphasize the psychological consequences of being indebted.

Objective Debt Measures The main indicator we use for this part of the analysis is an aggregated and standardized index measuring objective debt. It consists of the following components: The debt service to income ratio (DSR), the remaining debt to income ratio, and whether the household defaulted or paid late on a loan in the last twelve months. Each component is well established in the literature (see, for example D'Alessio and Iezzi, 2013). Among them, the DSR is especially widely recognized as standard measure to capture indebtedness. We explain how the index and its components are derived in Appendix B.

Subjective Debt Measures While objective debt indicators may provide numerically accurate debt measures, these are criticized for various reasons, such as failing to account either for the reasons why households borrow or for the household's undisclosed ability to pay back debt. Therefore, we also include subjective "respondent driven" debt measures in our analysis. As before, we derive a standardized index aggregating different components of subjective debt. The components include an assessment of whether the

[12] Among others, D'Alessio and Iezzi (2013) provide a summary on different indebtedness indicators, their usage, and possible drawbacks.

household feels it has too much debt, whether it has difficulties paying them off, and the so-called "sacrifice index."[13] The index and its components are explained in detail in Appendix B. Schicks (2013) prefers to use subjective debt measures over objective ones in her work analyzing over-indebtedness from a customer-protection point of view in microfinance. D'Alessio and Iezzi (2013) also rely heavily on a subjective debt measure to study over-indebtedness in Italy. However, in line with Keese (2012) and Lusardi and Tufano (2015), we argue that these measures describe a situation of financial distress rather than over-indebtedness such that these measures should not be used without considering objective debt indicators as well.

Over-Indebtedness Measures Again, we construct an overall standardized index that aggregates various measures of over-indebtedness. We include the following components in the index: a debt service to income ratio greater than 0.4 and households with more than four loans. The threshold we set for the DSR is based on considerations from the literature where a range between 0.3 and 0.5 is used to indicate over-indebtedness (Chichaibelu and Waibel, 2017; D'Alessio and Iezzi, 2013).The detailed construction of the index is explained in Appendix B. All indices we derive point to accumulating more debt the higher the household scores.

2.4 Descriptive Statistics

The following subsection provides descriptive statistics about the financial situation in Thailand and in our sample population. Since we use a restricted sample for the analysis in Section 3, the descriptives are provided for the same group. In the analysis, we exclude outliers by the following means: First, we trim the 1 percent highest and lowest monthly household incomes in 2016 and 2017. Second, we exclude households whose income is negative and who have a debt service to income ratio either smaller than zero or greater than four. These restrictions all downward bias our results because we cut extremely high debt service ratios as well as those households who have negative debt service ratios and whose incomes are already negative. For the latter case, we trim them as we do not know whether a negative income itself means that these households are in financial distress.

Our average respondent is 57 years old, female, the spouse of the household head, and has 5.7 years of education. While 57.27% of our respondents are the sole financial decision makers in their households, 28.05% share this task with someone else. Hence, while capturing some respondent specific characteristics, we are still confident that these individual traits determine the household's state of indebtedness because the majority of respondents is in charge of making financial decisions. However, as a robustness check, we

[13] We closely follow Schicks (2013) in constructing the sacrifice index.

re-run the analysis without respondents who are not at all in charge of financial decision-making within the household.

In Thailand, over 80% of the population has a bank account and over 60% use them for digital payments. The gaps in financial inclusion between women and men as well as between the rural and urban population have declined and are relatively small (Demirguc-Kunt et al., 2018). Financial inclusion in our sample is similar: 78.34% of our sample households have an account with a formal banking institution.

Simultaneously, the rural credit market in Thailand has evolved extensively , providing manifold loan options for consumers. This is mainly due to heavily subsidized government programs. The Thai credit market is dominated by government-financed institutions (Chichaibelu and Waibel, 2017). The most important ones are the Bank for Agriculture and Agricultural Cooperatives (BAAC) and the Village and Urban Community Fund (VF) program,[14] with the former reaching approximately 95% of all farm households (Terada and Vandenberg, 2014). In our sample, the majority (73.4%) of households have a loan that is either still owed or has been paid back within the last 12 months. Those households have on average 2.4 loans. Figure 5 exhibits a graphic overview of the loan situation. Households borrow from formal and informal sources alike. In fact, loan sources are diverse, with the two most important credit sources being the BAAC and the VF. Nevertheless, households also borrow from agricultural cooperatives, business partners, relatives, and friends. Households take out loans for various reasons. Most loans are primarily used for buying agricultural related goods like fertilizer or pesticides (23.96%), for buying consumption goods (22.39%), and for agricultural investments e.g. farm land or agricultural machines (16.58%). Loans are also used for paying back another loan (9.87%), buying durable household goods (6.72%), and education (3.15%).

[Figure 5 about here]

A descriptive overview of our main variables of interest is provided in Table 2. The first part represents the two bias measures explained in Sub-Section 2.2. The expected income bias indicates that, on average, respondents are rather underconfident with regard to their future income. A total of 75% of the respondents expect their future income to be lower than what they earned in the year of the survey. The financial forecast error suggest that no respondent is extremely biased in any of the two directions, since it ranges between minus three and three. Generally, expectations between future household well-being and *ex post* reflection on past household development match well in our sample: The median respondent does not make any forecast error (i.e. the difference is zero).

[14] The aim of the VF is to improve financial access in rural areas in Thailand. It is one of the largest microfinance programs in the world (Kislat and Menkhoff, 2013)

The second part of Table 2 depicts our previously derived objective and subjective debt measures (see Sub-Section 2.3). The average DSR lies at 0.23. Hence, on average households are in debt, but below a critical threshold, i.e. not over-indebted. About 18% of the households have a DSR which is higher than 0.4 and are therefore considered as over-indebted, while 14% of our sample households have more than four loans.

[Table 2 about here]

Furthermore, Table 3 presents correlations between all our debt indicators. Naturally, the objective and subjective indices are significantly correlated with their respective sub-indicators. However, our objective and subjective measures also correlate significantly with each other. This is encouraging, since it rebuts criticism with respect to objective debt measures neglecting important dimensions of indebtedness.

[Table 3 about here]

Another important variable for our study is financial literacy. Our financial literacy index (described in Sub-Section 2.1) indicates a relatively low level of financial literacy. On average, respondents answered four out of seven knowledge questions correctly, reached five out of nine possible points concerning financial behavior, and three out of seven possible points with regard to financial attitude. This is in line with findings from the OECD/INFE study for Thailand from 2016 (OECD, 2016).

Figure 6 provides a graphic overview of the results on our measure for perceived income certainty: 55.56% of respondents are at least somewhat certain about their income development and 28.44% are very certain. The survey took place during the harvest season, so that respondents might have an idea about the harvest outcome and therefore perceive their expected future income as rather certain.

[Figure 6 about here]

3 Survey Results

This research examines the link between upward biased income expectations and (over-)indebtedness. In the following, we relate the derived bias measures to the debt indicators. We run simple OLS regressions estimating correlations between the variables in question. In addition, we present experimental results in Section 4.

3.1 Estimation Strategy

The regressions we run take the following form (standard errors are clustered at the village level):

$$DebtMeasure_i = \beta_0 + \beta_1 Bias_i + X_i^{'}\beta_2 + \epsilon_i \tag{3}$$

The dependent variable $Debt\ Measure_i$ represents the debt measures we apply to mirror the financial situation of the household as clearly as possible. It contains: the objective debt index,[15] the subjective debt index,[16] the debt service to income ratio, the sacrifice index, and an over-indebtedness index.[17]

The main variable of interest is $Bias_i$. It represents the bias measures we derived: First, it is a dummy turning one, if the subjective expected median income in the next twelve months is greater than the realized income in the survey period and, second, the forecast error focusing on the household's financial situation.

The vector X_i controls for household and respondent specific characteristics that are likely to determine indebtedness of the household. Precisely, these are the number of shocks the household had to cope with in the year prior to the general TVSEP survey in 2017 (time period 5/16-4/17), occupation dummies for farming, self-employment, and wage employment, monthly household income in 2016 and 2017, the number of children between 0-6 years, 7-10 years, and 11-16 years old, the number of elders and of working members in the household, total household education (sum of all educational levels of its members), age and age squared of the respondent, and respondent's financial literacy score. In alternative specifications, we add as control variables (where possible) the lagged value of the dependent variable to control for the existing stock of debt (similar to Hyytinen and Putkuri, 2018).

3.2 Main Results

To begin, we simply relate the respective bias measures to each debt indicator. In a second step, we add the aforementioned control variables to our regression as the debt indicators depend on other respondent and household specific characteristics as well. We are interested in comparing our two main debt biased expectation indicators with each other, namely the bias derived from the expected median income and the financial forecast

[15] Standardized average of debt service to income ratio, remaining debt to income ratio, a dummy regarding whether the household paid late or defaulted on a loan

[16] Standardized average of the sacrifice index, answers to questions on debt position and whether the household has difficulties paying off debt

[17] Standardized average of a dummy turning one if the debt service to income ratio is greater than 0.4 and a dummy turning one if the household has more than four loans.

error. Tables 4, 5, and 6 provide results on the expected income bias measure and Tables 7, 8, and 9 show regression outputs for the financial forecast error. The first column in each table represents the standardized and averaged index whereas the subsequent columns depict results for the single non-standardized components of the indices.

[Tables 4 - 9 about here]

We find a strong statistically significant relation between both bias indicators and the objective debt measure. The objective debt index[18] increases by 0.35 - 0.41 standard deviation units if respondents exhibit very high positive income expectations based on their expected future median income (columns (1) and (2), Table 4). The debt service to income ratio and the remaining debt ratio mainly drive this effect. The DSR increases by 14.9 - 20.5 percentage points (columns (3) and (4)) and the remaining debt ratio by 16.2 - 19.7 percentage points (columns (5) and (6)) for households with biased income expectations. These are substantial increases given that the mean DSR is 0.23 and the fact that we already exclude households with a DSR greater than four. Furthermore, the probability that a household paid late or defaulted on a loan increases by 5.7% - 7% if a household's expected future median income is greater than the current income.

The direction of the relationship between objective debt and biased expectations remains similar with respect to the financial forecast error. Point estimates, however, tend to be lower compared to the bias dummy coefficients. If the financial forecast error increases by one unit,[19] the objective debt index increases by 0.11 - 0.14 standard deviation units (columns (1) and (2), Table 7). This effect is mainly related to the influence of the remaining debt to income ratio, which increases by 9.8 - 11 percentage points for households with a higher financial forecast error (columns (3) and (4)). The other two index components are not influenced by the forecast error if other important debt determinants are controlled for.

Concerning the control variables, income, and the type of occupation significantly affect a household's debt situation for both biased expectations specifications. Furthermore, age and age squared are both highly significant determinants of (over-)indebtedness; thus suggesting a hump-shaped pattern in line with life-cycle-income-smoothing. Objective debt, however, remains largely unaffected by the household composition and education.

We find interesting results for subjective indebtedness. While there are no significant relations between biased median income expectations and subjectively perceived debt, the financial forecast error strongly and significantly affects the subjective debt index. If

[18] This is the standardized average of the debt service to income ratio, the remaining debt to income ratio, and whether the household defaulted or paid late on a loan.

[19] This means households are more optimistic regarding their future income development than what was actually realized and recalled later on

the financial forecast error increases by one unit, 0.10 - 0.16 standard deviation units are added to this score (columns (1) and (2), Table 8). Mainly, this is due to the positive effect the financial forecast error has on the "debt position" component of the index. Households with a higher error tend to state more frequently that they "have too much debt right now" (columns (3) and (4)). We conclude that the nature of the financial forecast error being more "subjectively" elicited than the calculated biased expectations dummy *per se*, might be reflected in more pronounced results regarding subjectively "felt" debt. Subjective debt may, thus, be actually rather a concept of perceived financial distress affected by not only the household's true debt situation but also by respondent characteristics.

This becomes clearer when analyzing the control variables. Unlike the regressions on objective debt, personality characteristics such as risk aversion and self-control significantly affect subjective debt measures: More risk loving respondents and those with lower self-control are more likely to subjectively be indebted. Delving deeper into the relationship between respondent characteristics, we run further regressions on subjective debt and include the Big Five measures[20] as additional control variables (results are available on request). They almost exclusively determine subjective debt measures and less over-indebtedness or objective debt. If a respondent scores high on openness and neuroticism, her subjective debt index and the underlying components are affected positively, i.e. debt rises.[21] Furthermore, scoring higher on financial literacy and acquiring more education is related to less subjective debt. Income sources do not play a role in determining this kind of debt, but the number of shocks experienced by the household in the last year affects subjective debt positively. This may suggest that experiencing a shock may have psychological consequences on household members exceeding those on income.

Lastly, greater financial forecast errors are strongly related to all over-indebtedness measures (see Table 9). The over-indebtedness index increases by 0.10 - 0.13 standard deviation units, when the financial forecast error increases by one unit. Both index components are similarly responsible for this estimate: Households that make more optimistic income forecasts are, by 3.2% - 3.7%, more likely to have a DSR greater than 0.4 and are, by 3.5% - 4.5%, more likely to hold more than four loans (columns (3) - (6), Table 9). Results for the bias dummy measure are not as consistent: We fail to see a relation between the bias and the over-indebtedness index. The expected median income bias solely and positively affects the probability to have a DSR greater than 40% (columns (3) and (4), Table 6).

In an additional exercise, we add an income certainty measure as a control variable

[20] The Big Five comprise the following personality traits: openness, conscientiousness, extraversion, agreeableness, and neuroticism. More details on their construction are found in Appendix B.

[21] Openness is the only trait of the Big Five that determines debt in almost all specifications. It is possible that individuals with a high level of openness are also over-confident persons.

to our main specifications in order to investigate whether the certainty about future household income development affects (over-)indebtedness status.[22] Tables 10, 11, and 12 present results.

[Tables 10 - 12 about here]

There is no clear effect of certainty about future income on objective or subjective indebtedness measures, except for that being more certain about the income development is weakly related to an increased debt service to income ratio (columns (3) and (4), Table 10) and a higher "debt position" (columns (3) and (4), Table 11).

Yet, we find strong effects of certainty for all over-indebtedness measures across both bias specifications: If a respondent is very certain about the development of future household income, this is linked to an augmented over-indebtedness index, a higher probability to have a DSR greater than 0.4 and an increased probability of holding more than four loans (Table 12). Moreover, the effect of certainty increases while the effect of the specified bias variables decreases to a point where the expected median income bias no longer affects significantly the over-indebtedness index. Thus, certainty - representing a form of overconfidence, namely over-precision - is likely to constitute a part of the expectation biases we derived.[23]

Hence, we conclude, (i) that there is a significant and robust relationship between biased income expectations and (over-)indebtedness; (ii) We are also reassured that subjective and objective debt indicators measure different dimensions of indebtedness. While the "hard" objective debt measures are affected by both expectation biases, the more subjective measures are affected relatively more by the financial forecast error. This indicates that these debt measures rather show respondent's perceived financial distress no matter the actual numerical debt level, and (iii) Certainty about the household's future income development is a likely driver of biased expectations and it primarily affects over-indebtedness.

3.3 Robustness

Excluding Possibly Confounding Observations. Before eliciting the subjective expected income of respondents, we ask two questions testing their understanding of probability.

[22] Details on how the certainty measure is constructed are found in Appendix B.

[23] As another variable controlling for certainty, we add the interquartile range of the elicited income distribution to the regressions using the bias dummy as main variable of interest. While this certainty measure does not affect over-indebtedness, it affects the subjective debt measures in a similar way as over-precision: Higher uncertainty expressed through a higher interquartile range affects the subjective debt and the sacrifice index significantly and negatively. Hence, uncertainty is related to lower debt and less financial distress. The coefficient itself remains close to zero, however. Results are available upon request.

We here examine whether our main results hold and re-run the analysis for only those respondents that correctly answer the probability probing questions. Results are presented in Tables A.1, A.2, and A.3 in the Appendix. The effects for this sub-sample stay highly significant and almost all coefficients increase in size emphasizing the link between biased expectations and (over-)indebtedness.

In order to verify that respondents have an actual understanding of their household's finances, we only include those individuals who are in charge of making household financial decisions either by themselves or together with someone else (results are available upon request). Overall, the results stay virtually unchanged with regards to the significance of our coefficients of interest. Point estimates are slightly higher for the expected median income bias.

Different Bounds for Biased Expectations. We are aware of the fact that for some households a non-zero difference between expected and actual income is rationally justified. Thus, we calculate both wider and narrower measures for the expected median income bias to make sure we actually capture biased expectations. We define the threshold from which a household is said to exhibit biased income expectations narrower by including only the upper 20% of households that have a large positive difference between expected and actually realized income and we define the threshold wider by including the upper 30% of households from this "bias" distribution. The results are similar in size and significance to the expected median income bias we use in the main part of the analysis (see Tables A.4, A.5, and A.6 in the Appendix). Thus, we are reassured that our effects are not due to arbitrarily setting the threshold of having biased expectations at zero.

Adding the Lag of the Dependent Variable. In line with Hyytinen and Putkuri (2018), we control for the stock of already accumulated debt by including the lagged dependent variable in the regression with the debt service to income ratio as endogenous variable. This way, we can detect how debt evolves holding the accumulated level of debt constant (see Table A.7 in the Appendix). As expected, the past level of the DSR has a large impact on the present level. Nevertheless, the bias dummy remains significant. The financial forecast error, however, still does not relate to the DSR, much like our main results above.

Interacting the Bias with Personality Traits. We do not claim to show a causal effect because we acknowledge that the relation between (over-)indebtedness and biased income expectations may also work in the reverse. For example, if people are indebted, they might have a great bias regarding future expected income as they plan to work harder in the future to pay down their debt. We expect such people to exhibit a high level of conscientiousness, the personality marker describing achievement oriented (McClelland et al., 1953), hard-working, effective, and dutiful characters (Barrick and Mount, 1991). Hence, we interact our bias variables with this character trait, expecting to find significant effects for conscientious people. Results for the aggregated indices as dependent variables are shown in Table A.8. The interaction is not significant for any debt measure, no matter which bias we interact conscientiousness with. This counteracts the assumption that the achieving respondents with biased expectations drive the relationship between biased expectations and debt status. Hence, personality traits do not seem to support the claim that more indebted people have a higher income bias, because they strive to work more in the future.

4 The Experiment

The preceding section shows that biased expectations and (over-)indebtedness are strongly related to each other, even when controlling for important socio-economic characteristics and shocks. However, methodologically, the implemented regression analysis only represents correlations. In what follows, we try to prove that biased expectations are one potential *cause* why persons in our sample spend more than they can actually pay for.

4.1 Experimental Design

As final part of the survey, we play a market game in which respondents can buy different kinds of goods for a discounted price with money they earn in the experiment. They can buy packs of coffee, chips, dried mango, or detergent for 10 THB (ca. 25 euro cents) each instead of the 20 THB list price.[24] Each participant receives an endowment of 40 THB. Additional money can be earned by answering questions in a trivia game. The amount earned depends on how many questions the participant answers correctly in comparison to the other participants. We rank them from 1-10, where rank ten corresponds to answering the most questions correctly and rank one to answering the least number of questions correctly.[25] Participants ranked 1-4 do not earn anything additionally to their endowment,

[24] At least for the bag of chips, it is common knowledge that they usually cost 20 THB as, for a long time, they had the price printed on their front. To further convince participants that the products are truly discounted, we attached "20 THB" price tags to each product.

[25] In the field, participants from the first villages were ranked against participants from our pilot villages and our interviewers who also took the quizzes. For later villages, we replaced our interviewer data with data from the previous villages and told participants that they are ranked against ten persons who live in a village similar to theirs. For the final analysis, we use all the observations to create a ranking. In each treatment, we have two accumulation points in the number of correctly answered questions that are next to each other and around the mean. We set these two points as rank five and

those ranked 5-6 earn 10 THB, those ranked 7-8 20 THB and those ranked 9-10 earn 40 THB additionally. Thus, participants can earn up to 80 THB and can buy at most eight goods.

We make expectations a crucial factor in the game by requiring participants to decide how much and what to buy before they take the pay-off relevant quiz, i.e. before they know their final payoff. We divide participants in two treatment groups; one group faces a "hard" quiz and the other one an "easy" trivia quiz. To convey the difficulty of each quiz and to exogenously vary expectations about relative performance, participants do a test quiz with seven questions upfront where difficulty again depends on treatment. Based on the test quiz participants infer how good they will be in the pay-off relevant main quiz and form expectations about the performance of the others and, thereby, their relative rank. They are ranked within each treatment group and they are told that everybody they are ranked against took the exact the same quiz. With this design, we can exploit the so-called hard-easy gap analogous to Dargnies et al. (2016) and very similar to Grohmann et al. (2019). Much research finds that people tend to overplace themselves in easy tasks and to underplace themselves in hard tasks (for example Merkle and Weber, 2011; Hartwig and Dunlosky, 2014; Benoit et al., 2015). Over-(under-)placing is a form of over-(under-)confidence in which individuals over-(under-)estimate their relative performance in comparison to others. Thus, by assigning participants to two different treatments, we exogenously vary their expectations through varying self-confidence (see Figure 7).[26] We subsequently measure confidence as difference between expected rank and actual rank:

$$confidence = rank_{exp} - rank_{act} \tag{4}$$

Theoretically, upward biased expectations can arise for two reasons; either an individual is overly optimistic or overly confident. We follow Heger and Papageorge (2018) in defining overoptimism as the tendency to overestimate the probability of preferred outcomes and overconfidence as the tendency to overestimate one's own performance. For our experiment, we decide to concentrate on overconfidence because numerous studies show that overconfidence is related to important life and financial decisions.[27]

six. Each one point deviation in correctly answered question then constitutes a one point deviation in rank (e.g. if rank five means nine questions answered correctly, rank four means eight questions answered correctly). Since there are more questions than possible ranks, we have some bunching of correctly answered questions around rank one and rank ten, the boundaries of the ranking.

[26] The exogenous variation is one of the reasons why we do not include this measure for self-confidence in our survey regressions as an alternative measure for expectation bias. Another reason is that self-confidence is domain dependent, which can also later be seen comparing the on average observed under-confidence in financial literacy and the overconfidence we find here.

[27] For example, Camerer and Lovallo (1999), who experimentally test the effect of overconfidence on entrepreneurial decision-making (this relationship is a well-researched field of study), conclude that excess entry in a market game is strongly related to overconfidence and not to overoptimism.

[Figure 7 about here]

Except for the difference in difficulty, the procedure is the same for every participant: If participants agree to play the game, the interviewer prepares the set-up and starts reading out the instructions. The instructions include comprehension questions to test whether participants understand how their rank is determined and how much they can earn. If participants do not answer these questions correctly, the interviewer does not continue with the instructions.[28] After they have finished the instructions, the participants start to answer the test quiz, which has seven trivia questions. They have five minutes to answer all the questions. For each question, four possible answers are given. When the time is up or participants have finished answering, they receive a decision sheet. On the decision sheet, they first have to write down the rank and the earnings they expect to reach in the following main quiz. Then, they have to indicate their buying decision based on their expected earnings. Afterwards, participants continue with the main quiz where they have to answer 15 questions in ten minutes. Following the quiz, there are three debriefing questions including a question on the expected rank after the second quiz has actually been taken (such that we can check for belief updating). Finally, the interviewer calculates the rank and earnings, then hands over the products and money, if applicable.

In most cases, participants could read, write, and answer the quizzes on their own. Sometimes, especially older people needed assistance in reading and writing, which was provided by the interviewer. The supplemental material for the experiment are in the Appendix in English (for the experiment everything was translated to Thai).

Rational Decisions

If participants want to buy more than they can afford, including their endowment, their consumption has to be restricted. They receive at most as many goods as they can buy with their earnings and nothing beyond that amount. Participants are aware of that fact.

We implicitly assume that expectations influence buying decisions. If this does not hold, the aforementioned design feature seriously distorts our results as follows. If it was the case that "rational" participants strictly prefer goods over money because, for example, they are cheaper than list price and can be stockpiled, expectations would become meaningless for the consumption decision. Indicating to buy eight goods is weakly dominating any other number of goods for this kind of participants, since they clearly prefer goods over money independent of the budget.[29]

[28] Still, there are participants who had serious difficulties in understanding the game such that we exclude them from the main analysis

[29] If the participant expects less than 80 THB, there is a potential loss in indicating to buy less than eight goods because the prediction might be under-confident. However, given our setting, there is no loss if she indicates to buy eight goods but actual earnings are lower than 80 THB.

Eventually about 4% of our participants decided to buy eight goods even though they expect to earn less. An additional 3% wanted to buy more than they expected to earn but less than eight goods. In our main analysis, these observations are excluded because i) we already know that expectations do not impact consumption in this setting for them and ii) they could artificially inflate our results. We present additional analyses on this sub-sample in the Appendix Section "The Rationals" (A.12) and discuss whether they truly acted in a rational way or rather had difficulties understanding the game.

For the other 93%, we still assume that in general respondents prefer a bundle out of products and cash. The exact composition depends on individual preferences but also expected earnings. Thus, being overconfident (or underconfident) creates a distortion in utility. Following these reflections, we derive the following hypotheses:

Hypothesis 1: *On average, individuals in the easy treatment will buy more than individuals in the hard treatment.*
Hypothesis 2: *A great level of overconfidence will lead to excessive spending.*

Hypothesis 1 is implied by the finding on the hard-easy gap. Hypothesis 2 follows from the fact that we define respondents to be overconfident if their expected rank is higher than their actual rank, which implies that they earn less than expected. Since we cannot allow respondents to pay from personal money if experimental money is insufficient, restricting consumption in some cases is necessary. Therefore, they cannot accumulate debt. Nevertheless, this is what would actually happen in real life and, therefore, we opted for this experimental design to estimate the effect of overconfidence on (over-)indebtedness.

4.2 Experimental Results

Overall, 604 respondents participated in the game. Since participation is self-selected, participants and non-participants are compared in Table A.9 in the Appendix. As can be seen, participants and non-participants significantly differ in some variables.[30] In all these variables, the difference is in the expected direction: female, older, less occupied, less educated, financial illiterate and less numerate and more financial risk averse respondents are less likely to participate in the game. Several of these variables are significantly correlated with each other. Running a simple regression on the likelihood to participate, we find that some of these variables are insignificant and that the time of day is one of the strongest predictors of game participation (see A.10). Since the time of day at which we

[30] A complete list of all variables and their explanation is provided in the Appendix.

visited households for the interviews is mostly exogenous,[31] self-selection into the game is less pronounced than initially expected.

Out of the 604, seven observations are excluded because either treatments for them are mixed up, personal information is missing, or a third person helped them answer the questions. We exclude 44 observations that are also excluded from the survey regression analysis because they are outliers in income or the debt service to income ratio (see Section 2.4).[32] Additionally, 84 observations are excluded because it can be inferred from the data that comprehension was insufficient[33] or because they want to buy more than they expect to earn in total (see previous Sub-Section on these special cases). Those 84 cases differ only in their number of children between 7-10 years.

In Table 13 characteristics of the remaining 471 participants are compared across treatments. The significantly unequal number of participants per treatment is due to fact that we slightly over-sampled the easy treatment. Results from previous studies suggest that the effect of easy tasks on self-confidence is generally stronger than the effect of hard tasks (see for example Dargnies et al., 2016). The characteristics depicted here might be important for the general level of self-confidence and the willingness to buy products. Given the sample size and the number of variables analyzed, randomizing participants into the treatments worked well; the two groups only significantly differ with regard to their health status, their monthly household income, and their (objective) over-indebtedness index. Controlling for these variables leaves our results virtually unchanged.

[Table 13 about here]

Shift in Beliefs

On average, participants answered 9.07 out of 15 trivia questions correctly in the easy treatment and 5.09 out of 15 in the hard treatment. Thus, it can be assumed that for our sample the easy treatment is truly "easier" than the hard treatment. The average expected rank in the hard treatment is 6.89 whereas the average expected rank in the easy treatment is 7.22. In Figure 8 the cumulative distribution functions of the expected ranks for both treatments are plotted. It seems that there is only a small shift in beliefs, since the distributions are still almost overlapping.[34] Indeed, if we compare the distributions

[31] We interviewed households according to a schedule we designed together with our interview team manager, which tried to minimize travel distances for each interview team. Hence, this schedule was exogenous to individual household characteristics, except for the village that the household resides in. However, a few houses were empty the first time we visited them and we had to reschedule another date with the household itself.

[32] The results are robust to this exclusion.

[33] For example, one participant writes that he expects to earn 30 Baht from the game, which is, however, not an possible option. Another one wants to buy 35 products although the maximum affordable number is eight.

[34] We focus on the expected rank in our analysis but everything holds analogously for expected earnings.

of the “second” expectations that are elicited after respondents actually took the main quiz, we find a much larger shift (see Appendix Figure A.1). Thus, either our test quizzes are not as hard or easy as the main quizzes and, therefore, the shift in first beliefs is smaller or participants have such strong beliefs that they only gradually update their beliefs. Still, the distributions of first beliefs are significantly different from each other (Kolmogorov-Smirnov one-sided p=0.056; Wilcoxon rank-sum two-sided p=0.041). The t-test for mean expectations is significant at the 5% level (one-sided) as well (see Figure 11).

[Figure 8 about here]

The difference in self-confidence is larger than the difference in expected rank (see Figure 9). This might be driven by our ranking procedure or by the fact that the easy quiz is not a perfect shift of the hard quiz with respect to the number of questions answered correctly. In any case, this suggests that our manipulation via the treatments to shift the level of beliefs and thereby self-confidence worked.

[Figures 9 and 10 about here]

As seen in Figure 10, across both treatments the mean and median respondents are slightly overconfident (even in the hard treatment). The whole distribution is a little bit skewed to the left but still resembles a normal distribution. Over 14% of the sample have perfectly accurate beliefs and have a self-confidence of “0.” Small deviations from 0 could be considered accurate as well because they could present a form of Bayesian updating.[35] Still, a substantial fraction of participants seems to be tremendously overconfident.

Buying Decision

We find a significant positive correlation between expected rank (earnings) and the amount of goods participants want to buy. However, there is no significant relation between the treatment itself and mean desired consumption as presented in Figure 12.

[Figures 11 and 12 about here]

If we run regressions where we can control for the variables that are unbalanced across treatments, the picture stays the same: the treatment is positively related to the expected rank, the expected rank is positively related to the desired amount of goods, but the treatment is not related to the amount of goods (see Table 14).

[Table 14 about here]

[35] On this discussion see Merkle and Weber (2011).

A similar pattern emerges if we look explicitly at spending behavior (see Table 15). We distinguish *overborrowing*, meaning buying more than actual earnings including endowment can pay for, from *overspending*, meaning buying more than actual game earnings can pay for, but the spending can still be paid with the endowment. The expected rank as well as confidence have a significant effect on both variables, but treatment does not.[36]

[Table 15 about here]

A supplementary result we find worth mentioning is that having higher objective and subjective burdens as well as being over-indebted in "real life" is actually related to spending behavior in our experiment (see Table 16). Likewise, our regressions results from Section 3.2 on over-indebtedness become more precise if we only look at the persons who overspend in the game. Thus, those respondents who have problems controlling their spending in real life are also those who spend less carefully in the game. We see this as evidence that our experiment, although highly artificial, still captures aspects of real life behavior.

[Table 16 about here]

Summarized, our treatments shifted expectations in hypothesized directions; expectations are positively related to spending behavior, but the treatment has no impact on the latter. Therefore, we cannot claim that there is a causal link between expectations and overborrowing with our experiment.

4.3 Confounding Factors

The previous findings are exceptionally robust to various restrictions. For example, they are not driven by participants who are very old or have mild comprehension difficulties (we already excluded those with large difficulties in the main analysis). It is also not the case that the treatments only affect expected ranks but not expected earnings.[37] This suggest that there are confounding factors or "noise" interfering with our treatments. We run further analyses to rule out that the treatments affected factors other than expectations:

Frustration and Gratification. One of the most likely confounds could be that participants in the hard treatment feel frustrated because of the difficult questions and want to

[36] The level of significance is higher not lower when we exclude possibly "rational" participants who want to buy more than they expect to earn in total.

[37] This could happen if there is a piecewise treatment effect (shifting expectations only within the same earnings category) because earnings are only piecewise increasing in ranks and not equidistant.

treat themselves with "shopping." In contrast, some others might be proud of mastering such a hard quiz and also want to reward themselves. Both motives should lead to the result that especially participants with extreme expectations behave differently across treatments. Participants who are frustrated should rank themselves rather low whereas participants that are proud should rank themselves rather high. Subsequently, the buying behavior of participants with the same expected rank across treatments should be significantly different for the lowest and highest ranks. However, the only (marginally) significant difference we can detect is for the five participants who expected to reach rank two: here, participants in the hard treatment want to buy more than participants in the easy treatment. Excluding these observations does not change our results. For all other ranks participants in both treatments exhibit the same spending pattern. This finding is not in favor of frustration and gratification being possible confounding factors.

Temptation. Another possibility is that participants in the hard treatment are more susceptible to temptation goods. They have to exercise more cognitive effort, which decreases their self-control, so-called "ego depletion" (see, for example, Hagger et al., 2010). Running separate regressions on each product, we find a significantly different treatment effect only for dried mango. Still, self-control (measured with the scale from Tangney et al., 2004) and BMI do not have significant effects on buying mango, which opposes the ego depletion interpretation. We also do not find evidence that frustrated (more depleted) participants are more likely to buy mango. Furthermore, detergent is the most popular product and the share of detergent in all goods desired is not different across treatments, whereas mango is the least popular. Detergent is the one product we would expect to be least related to self-control issues. Summarized, we do not find convincing evidence that persons in the hard treatment are more likely to give in to temptation.

Based on the tests above, we argue that we can rule out the most probable confounding factors interfering with the treatments. We believe that the reason we do not find a treatment effect on spending and borrowing is that the induced shift in beliefs was not strong enough to eventually be reflected in spending. We can only speculate why the well-established hard-easy gap is so small in our setting. Consulting our interviewers and the data, we have no reason to believe that participants did not perceive the test quizzes as either hard or easy when they should. Several other studies find larger shifts in beliefs although participants had less exposure to manipulation.[38] The rural Thai population may have more persistent beliefs than Western populations, which makes

[38] For example, Grohmann et al. (2019) only use four questions they frame as "example questions" and find larger treatment effects on expectations.

changing these beliefs more difficult. Given the tremendous level of overconfidence we find, this circumstance might not be beneficial for our participants. It relates to our regression result that being too certain about the future income is related to over-indebtedness. "Sticky," biased expectations bear implications for policy making. They must be taken into account when measures to reduce household over-indebtedness are designed.

5 Conclusion

Over-indebtedness can pose a serious threat to households' welfare and the financial stability of a country, especially in emerging markets. However, the determinants of the worldwide high level of over-indebtedness are, so far, not well understood. Theoretically, as modelled in the permanent income hypothesis, higher income expectations should lead to a higher level of borrowing.

In this study, we analyze the effect of biased income expectations on over-indebtedness by using data from an extensive household survey and a lab-in-the-field experiment. Little financial knowledge and high income uncertainty demand for explicit research in emerging countries and not to rely on results for Western populations. Our sample belongs to a panel survey of relatively poor and rural households in Thailand. Indeed, we can confirm a low level of financial literacy in several dimensions and find substantial uncertainty in income expectations for our sample. While over-indebtedness is increasingly recognized as a growing problem in Thailand, our study sheds light on its potential drivers.

In our regression analysis, we find a strong and robust positive relationship between biased expectations and (over-)indebtedness controlling for various household characteristics and shocks. This finding holds for two alternative measures for biased income expectations and various measures for objective and subjective debt measures. Subjective debt measures are, however, much more related to the qualitative bias measure. This measure is likely to be influenced more heavily by judgments on the household's financial situation and by the respondent's personality traits. Last, certainty about the future household income development positively affects household over-indebtedness and is likely to be a driver of biased expectations themselves. The results are robust to various specifications.

We attempt to establish a causal relationship between biased expectations and overborrowing in our experiment by exogenously varying self-confidence via the so called hard-easy gap. Thereby, we change expectations about the future payout in the game. Our results show that also in the experiment, overconfidence is related to more spending and overborrowing but we cannot claim causality. The most probable reason why our treatments do not affect spending behavior are too "sticky" beliefs. This also suggests that rural households are too certain about their income expectations.

Two caveats of our study warrant mentioning: First, all our results are correlations and do not show causality. Still, by accounting for shocks households experienced, we can reduce the concern that over-indebtedness drives biased expectations or that both are spuriously correlated to each other. Second, because we will never know the true income generating process, we cannot know with certainty whether the expectations of our respondents are truly biased.

Nevertheless, we find reassuring evidence that too high expectations can lead to household over-indebtedness, thus pushing households into severe poverty. One of the potential channels why overconfident expectations affect over-indebtedness is being too certain about own expectations in the highly uncertain environment that rural households in emerging markets are living in. Given the supplemental evidence for sticky beliefs from our experiment, changing beliefs or their certainty seems to be challenging. More appropriate policy measures would reduce vulnerability and uncertainty with the expansion of assistance and insurance schemes, especially for households engaged in agriculture, but also by training to improve information processing in general.

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Tables and Figures

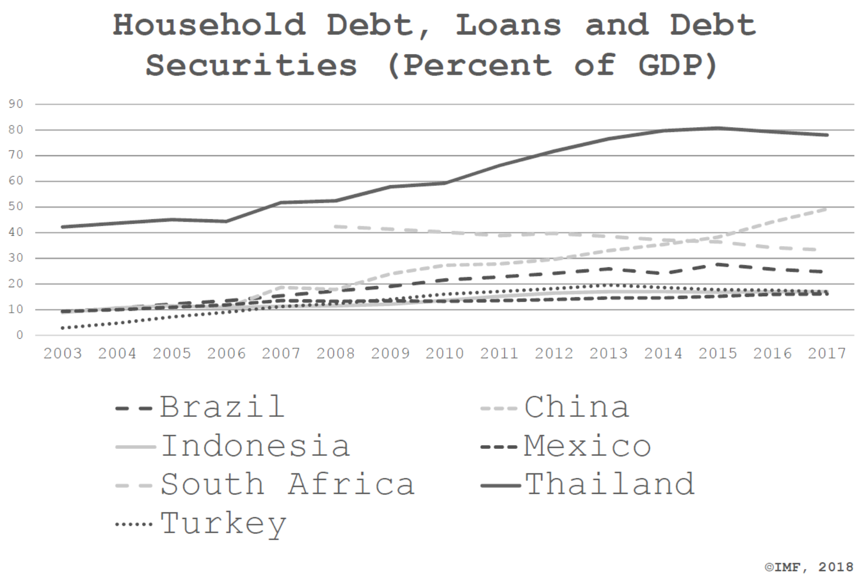

Figure 1: Household Debt to GDP Ratio, Selected Emerging Markets

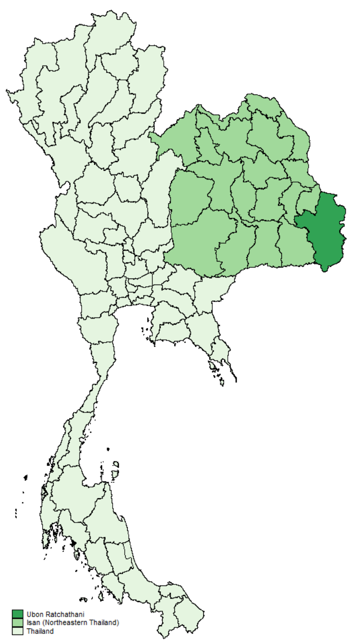

Figure 2: Study Site, Ubon Ratchathani Thailand

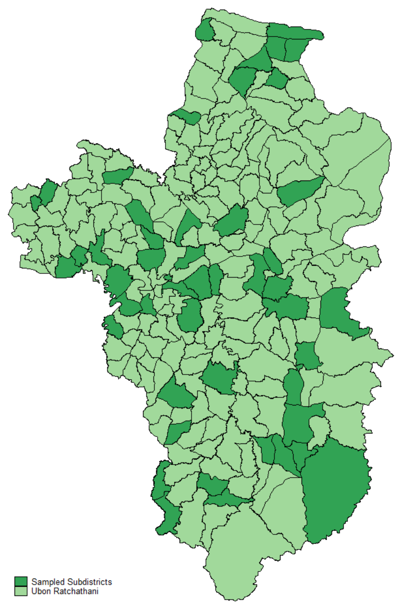

Figure 3: Sampled Subdistricts

Table 1: Probabilities Assigned to Sections of the Income Distribution

	Observations	Minimum	Maximum	Median	Mean	S.D.
0-3300 THB	737	0	100	20	32.18	35.1
3301-8100 THB	737	0	100	30	30.71	29.27
8101-16590	737	0	100	20	24.03	28.38
16591-300000	737	0	100	0	13.08	24.08

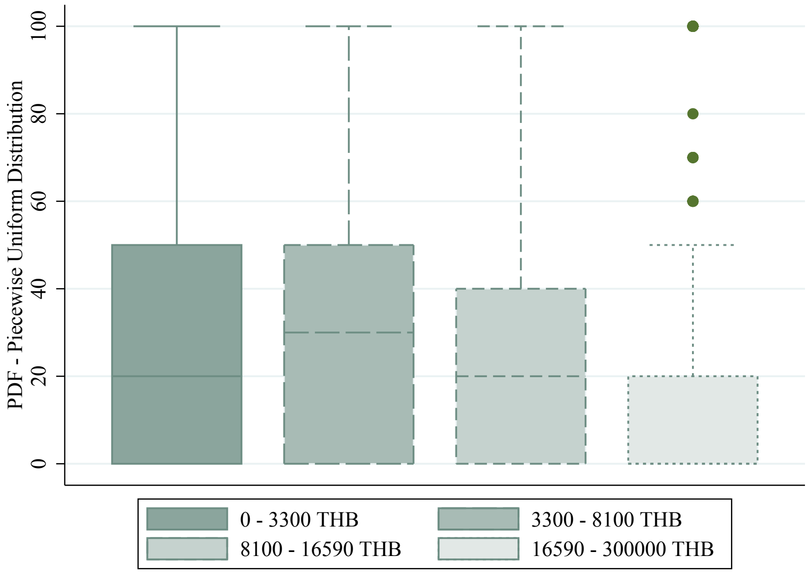

Figure 4: Probability Density Function of Expected Income

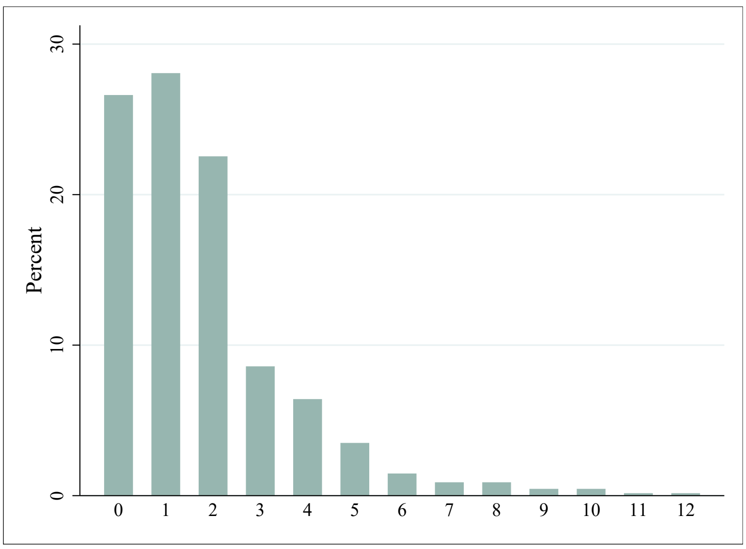

Figure 5: Number of Loans

Table 2: Summary Statistics - Main Variables

	Mean	SD	Minimum	Maximum	Observations
Expectation Bias Indices					
Expected Income Bias (=1)	0.24	0.43	0	1	686
Financial Forecast Error	0.17	0.95	–3	3	674
Debt Variables					
Objective Debt Index	0.00	1.00	–1	5	688
Debt Service Ratio 2017	0.23	0.48	0	4	688
Remain. Debt/Income Ratio	0.34	0.70	–1	10	665
Paid Late/Defaulted on Loan	0.15	0.36	0	1	685
Over-Indebtness Index	–0.00	1.00	–1	3	688
DSR > 0.4 (=1)	0.18	0.39	0	1	688
Holds > 4 Loans (=1)	0.14	0.35	0	1	688
Subjective Debt Index	–0.00	1.00	–2	3	688
Sacrifice Index	–0.08	1.19	–2	4	688
Debt Position	–0.02	0.87	–2	1	688
Diff. Paying Debt	1.37	0.60	1	3	686

Note: The debt index variables are standardized. The components of the indices are given in non-standardized real terms.

Table 3: Correlation Matrix - Debt Variables

	Obj. Debt	DSR 2017	RD to Inc.	Paid Late/ Default	Over- indebted	=1 if DSR >40%	=1 if > 4 Loans	Subj. Debt	Sacrifice Index	Debt Position	Diff. Pay. Debt
Obj. Debt Index	1										
DSR 2017	0.694***	1									
Remain. Debt/Inc.	0.551***	0.370***	1								
Paid Late/Default	0.750***	0.107***	0.146***	1							
Overindebt. Index	0.531***	0.672***	0.368***	0.126***	1						
DSR > 0.4 (=1)	0.556***	0.759***	0.320***	0.111***	0.845***	1					
Holds > 4 Loans (=1)	0.345***	0.381***	0.303***	0.102***	0.849***	0.434***	1				
Subj.Debt Index	0.485***	0.253***	0.292***	0.426***	0.296***	0.209***	0.292***	1			
Sacrifice Index	0.252***	0.135***	0.106***	0.233***	0.141***	0.0881**	0.150***	0.738***	1		
Debt Position	0.427***	0.290***	0.322***	0.300***	0.349***	0.273***	0.319***	0.797***	0.333***	1	
Diff. Paying Debt	0.466***	0.171***	0.261***	0.474***	0.207***	0.130***	0.220***	0.832***	0.423***	0.544***	1

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The objective debt index, the subjective debt index, and the over-indebtedness index are standardized with mean zero and standard deviation of one.

Correlations are based on the trimmed sample.

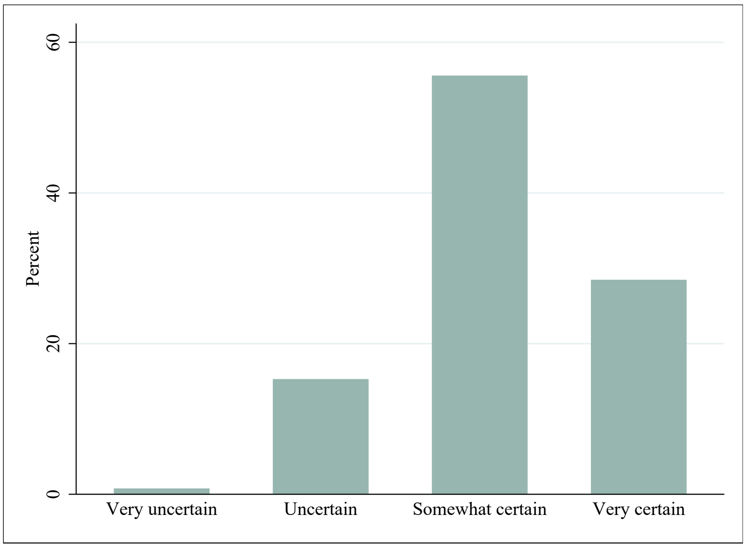

Figure 6: Income Certainty

Table 4: Income Expectation Bias Dummy - Objective Debt Indicators

	Obj. Debt Index		DSR 2017		Rem. Debt/Income		Paid Late/Default	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bias Dummy	0.410***	0.357***	0.205***	0.149**	0.197***	0.162**	0.058*	0.070**
	(0.107)	(0.110)	(0.055)	(0.057)	(0.072)	(0.077)	(0.032)	(0.032)
Monthly Inc. 2017		–0.000**		–0.000***		–0.000***		–0.000
		(0.000)		(0.000)		(0.000)		(0.000)
Age		0.051***		0.015*		0.028***		0.013*
		(0.018)		(0.009)		(0.010)		(0.007)
Age Squared		–0.001***		–0.000**		–0.000***		–0.000**
		(0.000)		(0.000)		(0.000)		(0.000)
No. of Shocks		0.072*		0.013		0.040**		0.024
		(0.040)		(0.022)		(0.020)		(0.019)
FL-Score		–0.001		0.012*		0.009		–0.009*
		(0.015)		(0.006)		(0.012)		(0.005)
Risk Aversion		0.030		0.012		0.005		0.009
		(0.020)		(0.009)		(0.017)		(0.008)
Self-Control		0.006		0.001		–0.001		0.003*
		(0.005)		(0.002)		(0.004)		(0.002)
Main Inc. Farming		–0.327*		–0.142		0.116		–0.106*
		(0.168)		(0.091)		(0.093)		(0.057)
Main Inc. Employed		–0.378**		–0.228***		–0.015		–0.057
		(0.169)		(0.082)		(0.088)		(0.062)
Main Inc. Self-Emp.		–0.242		–0.217**		0.191		–0.029
		(0.208)		(0.090)		(0.179)		(0.077)
Main Inc. Remitt.		–0.395**		–0.195**		–0.003		–0.092
		(0.162)		(0.083)		(0.092)		(0.060)
Children (0-6 yrs)		–0.044		–0.021		–0.102***		0.005
		(0.051)		(0.028)		(0.026)		(0.022)
Children (7-10 yrs)		0.038		0.014		0.033		0.003
		(0.077)		(0.034)		(0.044)		(0.031)
Children (11-16 yrs)		0.070		0.002		0.023		0.028
		(0.066)		(0.030)		(0.034)		(0.027)
No. of Elders		0.062		0.008		0.019		0.027
		(0.053)		(0.030)		(0.040)		(0.020)
No. of Working Mem.		0.012		0.010		0.002		–0.001
		(0.043)		(0.021)		(0.024)		(0.015)
Total HH Education		0.002		0.002		–0.001		0.001
		(0.005)		(0.002)		(0.002)		(0.002)
Constant	–0.098**	–1.011*	0.185***	–0.114	0.294***	–0.304	0.141***	–0.066
	(0.047)	(0.584)	(0.021)	(0.273)	(0.035)	(0.385)	(0.017)	(0.227)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	686	678	686	678	663	655	683	675
Adj. R-squared	0.030	0.082	0.032	0.079	0.013	0.051	0.003	0.025

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table 5: Income Expectation Bias Dummy - Subjective Debt Indicators

	Subj. Debt Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bias Dummy	0.061	0.100	0.072	0.119	0.013	0.023	0.054	0.078
	(0.093)	(0.093)	(0.083)	(0.085)	(0.053)	(0.054)	(0.104)	(0.104)
Monthly Inc. 2017		–0.000		–0.000		–0.000		–0.000
		(0.000)		(0.000)		(0.000)		(0.000)
Age		0.062***		0.062***		0.022**		0.045**
		(0.018)		(0.016)		(0.011)		(0.020)
Age Squared		–0.001***		–0.001***		–0.000***		–0.000**
		(0.000)		(0.000)		(0.000)		(0.000)
No. of Shocks		0.102**		0.083**		0.020		0.136**
		(0.042)		(0.033)		(0.024)		(0.054)
FL-Score		–0.034**		0.006		–0.020**		–0.066***
		(0.015)		(0.013)		(0.009)		(0.016)
Risk Aversion		0.050**		0.055***		0.027**		0.013
		(0.021)		(0.018)		(0.013)		(0.024)
Self-Control		0.010**		0.004		0.005*		0.015***
		(0.004)		(0.003)		(0.003)		(0.005)
Main Inc. Farming		–0.187		–0.096		0.009		–0.397**
		(0.147)		(0.127)		(0.087)		(0.191)
Main Inc. Employed		–0.033		–0.029		0.054		–0.158
		(0.160)		(0.138)		(0.094)		(0.203)
Main Inc. Self-Emp.		–0.016		–0.000		0.053		–0.149
		(0.168)		(0.136)		(0.103)		(0.219)
Main Inc. Remitt.		–0.209		–0.157		–0.066		–0.241
		(0.151)		(0.127)		(0.090)		(0.188)
Children (0-6 yrs)		–0.045		–0.084		0.013		–0.033
		(0.064)		(0.053)		(0.039)		(0.077)
Children (7-10 yrs)		–0.067		0.071		–0.064		–0.166*
		(0.075)		(0.068)		(0.044)		(0.094)
Children (11-16 yrs)		0.070		0.045		–0.005		0.141
		(0.071)		(0.054)		(0.045)		(0.092)
No. of Elders		0.003		0.027		0.014		–0.052
		(0.050)		(0.046)		(0.032)		(0.061)
No. of Working Mem.		0.122***		0.114***		0.014		0.159***
		(0.042)		(0.039)		(0.028)		(0.056)
Total HH Education		–0.010**		–0.007		–0.002		–0.016***
		(0.005)		(0.004)		(0.003)		(0.006)
Constant	–0.016	–1.349**	–0.043	–1.993***	1.367***	1.041***	–0.100*	–0.537
	(0.050)	(0.563)	(0.043)	(0.468)	(0.031)	(0.350)	(0.060)	(0.669)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	686	678	686	678	684	676	686	678
Adj. R-squared	-0.001	0.086	-0.000	0.089	-0.001	0.040	-0.001	0.084

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table 6: Income Expectation Bias Dummy - Over-Indebtedness Indicators

	Over-indebtness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Bias Dummy	0.188**	0.133	0.131***	0.093**	–0.007	–0.005
	(0.090)	(0.094)	(0.037)	(0.037)	(0.028)	(0.030)
Monthly Inc. 2017		–0.000		–0.000***		0.000
		(0.000)		(0.000)		(0.000)
Age		0.033**		0.006		0.014***
		(0.015)		(0.008)		(0.004)
Age Squared		–0.000***		–0.000		–0.000***
		(0.000)		(0.000)		(0.000)
No. of Shocks		0.038		0.001		0.022
		(0.045)		(0.018)		(0.014)
FL-Score		0.020		0.008		0.005
		(0.015)		(0.006)		(0.005)
Risk Aversion		0.047***		0.013*		0.016**
		(0.018)		(0.007)		(0.006)
Self-Control		–0.003		–0.002		–0.000
		(0.004)		(0.002)		(0.001)
Main Inc. Farming		–0.090		–0.066		0.006
		(0.142)		(0.062)		(0.047)
Main Inc. Employed		–0.266*		–0.114*		–0.055
		(0.147)		(0.060)		(0.050)
Main Inc. Self-Emp.		–0.261		–0.100		–0.064
		(0.173)		(0.076)		(0.059)
Main Inc. Remitt.		–0.236		–0.073		–0.074
		(0.165)		(0.067)		(0.053)
Children (0-6 yrs)		–0.056		–0.014		–0.021
		(0.063)		(0.025)		(0.022)
Children (7-10 yrs)		0.059		0.013		0.023
		(0.085)		(0.034)		(0.029)
Children (11-16 yrs)		–0.033		–0.018		–0.003
		(0.061)		(0.028)		(0.022)
No. of Elders		–0.056		0.002		–0.035*
		(0.062)		(0.023)		(0.020)
No. of Working Mem.		0.052		0.019		0.014
		(0.038)		(0.016)		(0.014)
Total HH Education		–0.000		–0.000		0.000
		(0.005)		(0.002)		(0.002)
Constant	–0.044	–0.901*	0.150***	0.088	0.145***	–0.304*
	(0.057)	(0.542)	(0.019)	(0.250)	(0.020)	(0.175)
Controls	No	Yes	No	Yes	No	Yes
Observations	686	678	686	678	686	678
Adj. R-squared	0.005	0.046	0.020	0.053	-0.001	0.040

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table 7: Fin. Forecast Error - Objective Debt Indicators

	Obj. Debt Index		DSR 2017		Rem. Debt/Income		Paid Late/Default	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fin. Forecast Error	0.143***	0.114**	0.040**	0.031	0.110***	0.098**	0.031*	0.023
	(0.045)	(0.045)	(0.019)	(0.019)	(0.038)	(0.039)	(0.018)	(0.018)
Monthly Inc. 2017		–0.000***		–0.000***		–0.000***		–0.000
		(0.000)		(0.000)		(0.000)		(0.000)
Age		0.051***		0.016*		0.029***		0.012*
		(0.019)		(0.008)		(0.010)		(0.007)
Age Squared		–0.001***		–0.000**		–0.000***		–0.000**
		(0.000)		(0.000)		(0.000)		(0.000)
No. of Shocks		0.056		0.008		0.026		0.021
		(0.045)		(0.023)		(0.019)		(0.020)
FL-Score		0.003		0.015**		0.013		–0.009
		(0.015)		(0.006)		(0.012)		(0.006)
Risk Aversion		0.028		0.012		0.004		0.007
		(0.020)		(0.010)		(0.017)		(0.008)
Self-Control		0.007		0.001		–0.001		0.003*
		(0.004)		(0.002)		(0.004)		(0.002)
Main Inc. Farming		–0.380**		–0.166*		0.094		–0.115*
		(0.172)		(0.091)		(0.093)		(0.058)
Main Inc. Employed		–0.475***		–0.264***		–0.050		–0.081
		(0.170)		(0.080)		(0.085)		(0.064)
Main Inc. Self-Emp.		–0.312		–0.239***		0.165		–0.049
		(0.207)		(0.088)		(0.181)		(0.078)
Main Inc. Remitt.		–0.442***		–0.219***		–0.030		–0.098
		(0.162)		(0.083)		(0.095)		(0.061)
Children (0-6 yrs)		–0.038		–0.017		–0.100***		0.005
		(0.051)		(0.028)		(0.025)		(0.023)
Children (7-10 yrs)		0.026		0.012		0.038		–0.004
		(0.081)		(0.034)		(0.047)		(0.032)
Children (11-16 yrs)		0.051		0.000		0.017		0.019
		(0.066)		(0.031)		(0.035)		(0.027)
No. of Elders		0.047		–0.001		0.009		0.027
		(0.052)		(0.029)		(0.039)		(0.019)
No. of Working Mem.		–0.022		–0.002		–0.016		–0.008
		(0.046)		(0.022)		(0.027)		(0.016)
Total HH Education		0.005		0.003		0.000		0.001
		(0.005)		(0.003)		(0.002)		(0.002)
Constant	–0.023	–0.852	0.230***	–0.071	0.326***	–0.298	0.148***	–0.008
	(0.040)	(0.609)	(0.020)	(0.260)	(0.029)	(0.392)	(0.015)	(0.242)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	674	667	674	667	652	645	671	664
Adj. R-squared	0.017	0.073	0.005	0.071	0.020	0.061	0.005	0.021

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table 8: Fin. Forecast Error - Subjective Debt Indicators

	Subj. Debt Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fin. Forecast Error	0.158***	0.109**	0.140***	0.097**	0.057**	0.042	0.139***	0.091*
	(0.045)	(0.045)	(0.037)	(0.038)	(0.028)	(0.028)	(0.048)	(0.046)
Monthly Inc. 2017		–0.000		–0.000		–0.000		–0.000*
		(0.000)		(0.000)		(0.000)		(0.000)
Age		0.057***		0.057***		0.020		0.043**
		(0.020)		(0.017)		(0.012)		(0.022)
Age Squared		–0.001***		–0.001***		–0.000**		–0.000**
		(0.000)		(0.000)		(0.000)		(0.000)
No. of Shocks		0.084*		0.065*		0.014		0.122**
		(0.043)		(0.034)		(0.024)		(0.055)
FL-Score		–0.033**		0.008		–0.022**		–0.061***
		(0.015)		(0.013)		(0.009)		(0.017)
Risk Aversion		0.047**		0.051***		0.026**		0.010
		(0.021)		(0.018)		(0.013)		(0.024)
Self-Control		0.011***		0.004		0.004*		0.017***
		(0.004)		(0.003)		(0.003)		(0.005)
Main Inc. Farming		–0.183		–0.104		0.015		–0.387**
		(0.141)		(0.122)		(0.085)		(0.185)
Main Inc. Employed		–0.036		–0.036		0.054		–0.156
		(0.157)		(0.133)		(0.094)		(0.199)
Main Inc. Self-Emp.		–0.021		–0.017		0.046		–0.127
		(0.162)		(0.130)		(0.100)		(0.216)
Main Inc. Remitt.		–0.199		–0.168		–0.057		–0.218
		(0.143)		(0.119)		(0.087)		(0.183)
Children (0-6 yrs)		–0.041		–0.082		0.015		–0.028
		(0.067)		(0.055)		(0.042)		(0.078)
Children (7-10 yrs)		–0.074		0.070		–0.072		–0.169*
		(0.077)		(0.071)		(0.045)		(0.095)
Children (11-16 yrs)		0.064		0.045		–0.007		0.129
		(0.070)		(0.054)		(0.043)		(0.091)
No. of Elders		–0.003		0.016		0.014		–0.055
		(0.050)		(0.046)		(0.031)		(0.061)
No. of Working Mem.		0.098**		0.091**		0.006		0.139**
		(0.041)		(0.038)		(0.028)		(0.055)
Total HH Education		–0.008*		–0.004		–0.001		–0.015***
		(0.005)		(0.004)		(0.003)		(0.006)
Constant	–0.019	–1.170*	–0.041	–1.784***	1.364***	1.145***	–0.102*	–0.520
	(0.043)	(0.630)	(0.037)	(0.516)	(0.027)	(0.389)	(0.053)	(0.720)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	674	667	674	667	672	665	674	667
Adj. R-squared	0.021	0.093	0.022	0.094	0.006	0.043	0.011	0.090

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table 9: Fin. Forecast Error - Over-Indebtedness Indicators

	Over-indebtness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Fin. Forecast Error	0.133***	0.108***	0.037***	0.032**	0.045***	0.035**
	(0.035)	(0.039)	(0.014)	(0.015)	(0.014)	(0.014)
Monthly Inc. 2017		–0.000*		–0.000***		0.000
		(0.000)		(0.000)		(0.000)
Age		0.033**		0.008		0.012***
		(0.015)		(0.007)		(0.004)
Age Squared		–0.000***		–0.000*		–0.000***
		(0.000)		(0.000)		(0.000)
No. of Shocks		0.020		–0.004		0.016
		(0.050)		(0.020)		(0.015)
FL-Score		0.023		0.010		0.005
		(0.015)		(0.006)		(0.005)
Risk Aversion		0.046**		0.013*		0.016**
		(0.018)		(0.007)		(0.006)
Self-Control		–0.003		–0.002		–0.000
		(0.004)		(0.002)		(0.001)
Main Inc. Farming		–0.103		–0.077		0.009
		(0.141)		(0.063)		(0.045)
Main Inc. Employed		–0.288**		–0.133**		–0.051
		(0.140)		(0.059)		(0.050)
Main Inc. Self-Emp.		–0.271		–0.112		–0.060
		(0.171)		(0.075)		(0.060)
Main Inc. Remitt.		–0.250		–0.085		–0.071
		(0.163)		(0.067)		(0.052)
Children (0-6 yrs)		–0.048		–0.009		–0.020
		(0.064)		(0.025)		(0.022)
Children (7-10 yrs)		0.058		0.015		0.021
		(0.086)		(0.034)		(0.029)
Children (11-16 yrs)		–0.031		–0.019		–0.001
		(0.062)		(0.029)		(0.022)
No. of Elders		–0.063		–0.005		–0.033
		(0.062)		(0.023)		(0.021)
No. of Working Mem.		0.031		0.010		0.009
		(0.040)		(0.016)		(0.014)
Total HH Education		0.002		0.000		0.001
		(0.005)		(0.002)		(0.002)
Constant	–0.021	–0.858	0.176***	0.064	0.135***	–0.257
	(0.049)	(0.540)	(0.017)	(0.236)	(0.017)	(0.175)
Controls	No	Yes	No	Yes	No	Yes
Observations	674	667	674	667	674	667
Adj. R-squared	0.014	0.053	0.007	0.053	0.013	0.046

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table 10: Certainty Measure - Objective Debt Indicators

	Obj. Debt Index		DSR 2017		Rem. Debt/Income		Paid Late/Default	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bias Dummy	0.352***		0.147**		0.151*		0.070**	
	(0.113)		(0.059)		(0.080)		(0.032)	
Fin. Forecast Error		0.118**		0.033*		0.098**		0.025
		(0.045)		(0.019)		(0.039)		(0.018)
Overprecision	0.034	0.030	0.057*	0.057*	–0.007	–0.011	–0.014	–0.015
	(0.065)	(0.066)	(0.032)	(0.033)	(0.047)	(0.046)	(0.023)	(0.023)
Constant	–0.891	–0.761	–0.216	–0.199	–0.226	–0.266	0.049	0.115
	(0.669)	(0.695)	(0.332)	(0.314)	(0.506)	(0.504)	(0.244)	(0.257)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	665	655	665	655	642	633	662	652
Adj. R-squared	0.079	0.071	0.084	0.076	0.049	0.060	0.023	0.021

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table 11: Certainty Measure - Subjective Debt Indicators

	Subj. Debt Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bias Dummy	0.077		0.114		0.004		0.057	
	(0.094)		(0.086)		(0.053)		(0.106)	
Fin. Forecast Error		0.109**		0.098**		0.042		0.091*
		(0.045)		(0.039)		(0.028)		(0.047)
Overprecision	–0.004	–0.006	0.092*	0.092*	–0.054	–0.055	–0.031	–0.036
	(0.068)	(0.069)	(0.054)	(0.053)	(0.041)	(0.042)	(0.084)	(0.085)
Constant	–1.330**	–1.229*	–2.357***	–2.204***	1.242***	1.311***	–0.367	–0.429
	(0.611)	(0.673)	(0.534)	(0.576)	(0.378)	(0.417)	(0.751)	(0.794)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	665	655	665	655	663	653	665	655
Adj. R-squared	0.084	0.093	0.092	0.099	0.042	0.046	0.080	0.087

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table 12: Certainty Measure - Over-Indebtedness Indicators

	Over-indebtness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Bias Dummy	0.128		0.092**		–0.007	
	(0.096)		(0.038)		(0.030)	
Fin. Forecast Error		0.113***		0.034**		0.036**
		(0.038)		(0.015)		(0.014)
Overprecision	0.178***	0.177***	0.049**	0.049**	0.061***	0.060***
	(0.053)	(0.053)	(0.020)	(0.020)	(0.019)	(0.019)
Constant	–1.284**	–1.303**	0.026	–0.021	–0.474**	–0.443**
	(0.573)	(0.571)	(0.266)	(0.248)	(0.188)	(0.188)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	665	655	665	655	665	655
Adj. R-squared	0.060	0.068	0.060	0.060	0.054	0.059

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

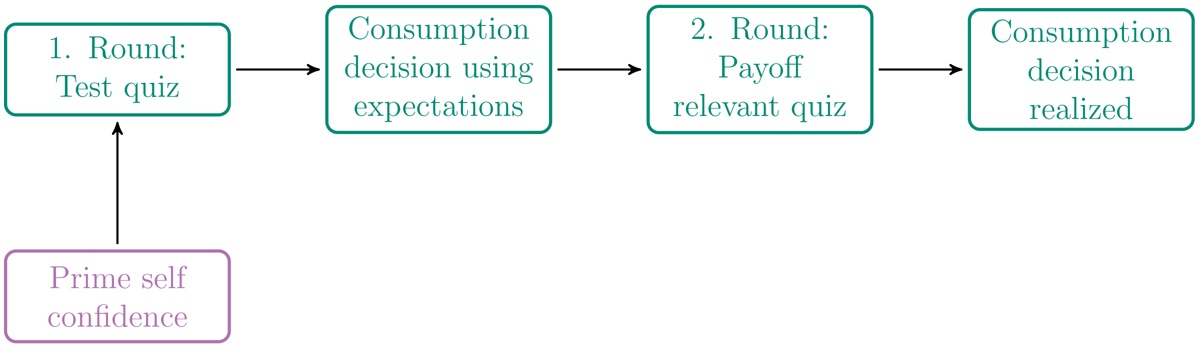

Figure 7: Experimental Flow

Table 13: Descriptive Statistics across Treatments

	(1) Full Sample	(2) Hard Treatment	(3) Easy Treatment	(4) Difference
Sex	1.64	1.60	1.67	−0.07
Age	56.16	55.23	56.93	−1.70
Relation to HH Head	1.70	1.69	1.71	−0.02
Marital Status	2.13	2.09	2.16	−0.07
Main Occupation	4.79	4.29	5.20	−0.90
Years of Schooling	5.92	6.08	5.79	0.28
Children (0-6 years)	0.33	0.37	0.29	0.08
Children (7-10 years)	0.26	0.26	0.26	0.01
Numeracy	2.14	2.09	2.19	−0.10
Health Status	1.38	1.32	1.43	−0.11**
BMI	23.58	23.25	23.86	−0.61
Fin. Decision Maker	1.57	1.55	1.59	−0.03
Self Control	20.94	21.19	20.75	0.44
Risk Taking	4.02	3.96	4.07	−0.12
Fin. Risk Taking	4.06	3.99	4.12	−0.13
FL-Score	5.66	5.55	5.75	−0.20
Monthly Inc. 2017	18653.06	20802.79	16893.44	3909.35**
Obj. Debt Index	−0.01	−0.07	0.05	−0.12
Subj. Debt Index	−0.02	−0.00	−0.03	0.03
Over-Indebtedness Index	0.03	−0.06	0.11	−0.17*
Morning	0.53	0.51	0.54	−0.03
Midday	0.27	0.26	0.28	−0.02
Read Alone	1.44	1.44	1.44	−0.00
Difficulties in Game	1.14	1.15	1.13	0.01
Observations	471	212	259	471

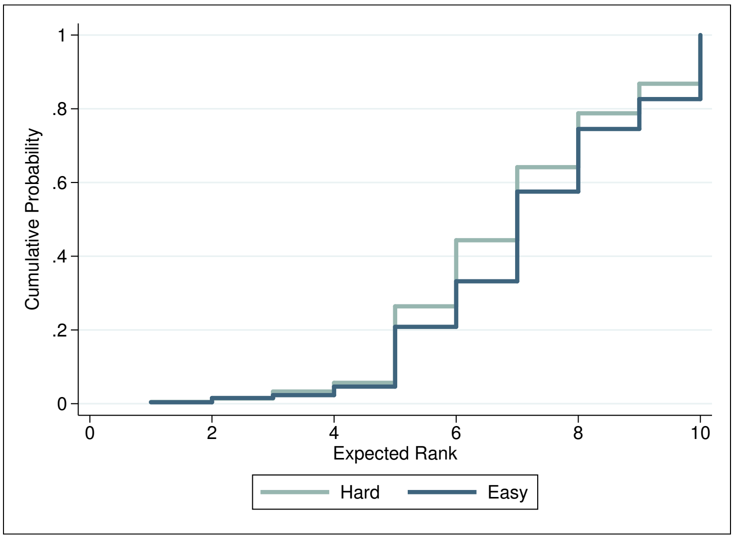

Figure 8: Cumulative Density Distribution of Expected Rank by Treatment

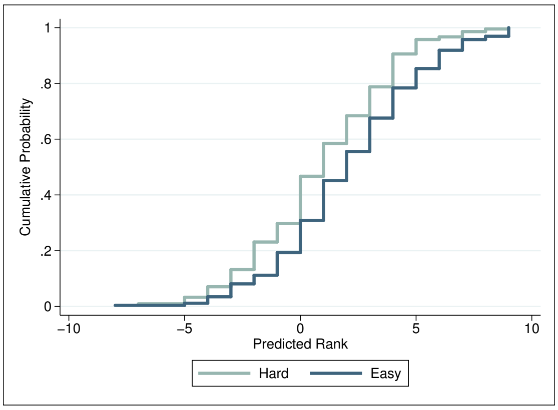

Figure 9: CDFs of Self-Confidence

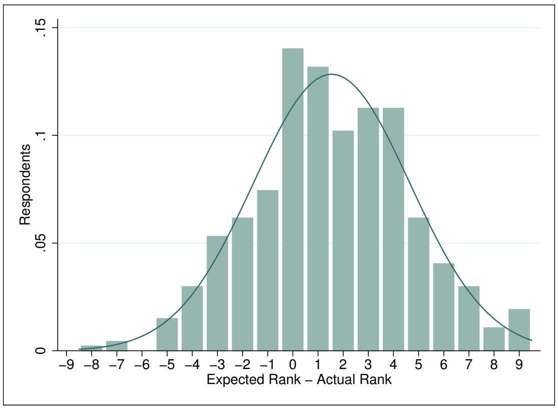

Figure 10: Histogram for Self-Confidence

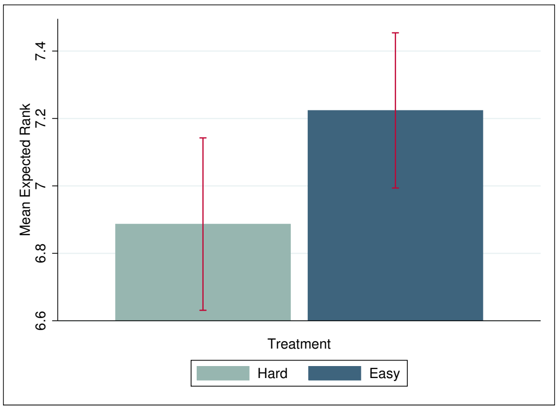

Figure 11: Mean Expected Rank by Treatment

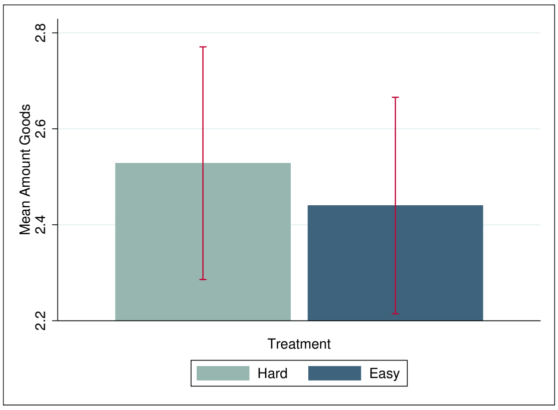

Figure 12: Mean Consumption by Treatment

Table 14: Consumption Decision

	Exp. Rank	No. Goods		
	(1)	(2)	(3)	(4)
Treatment	0.371**	–0.143		–0.200
	(0.175)	(0.173)		(0.171)
Exp. Rank			0.147***	0.152***
			(0.046)	(0.046)
Controls	Yes	Yes	Yes	Yes
Observations	470	470	470	470

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Standard errors in parentheses. Treatment: 0=Hard Quiz, 1=Easy Quiz; A higher expected rank corresponds to a higher expected performance. Controls: Health Status, Monthly HH income and Over-Indebtedness Index.

Table 15: Overborrowing and Overspending

	Overconfidence	Overborrowing		Overspending
	(1)	(2)	(3)	(4)
Treatment	1.216***	0.009	–0.008	–0.035
	(0.282)	(0.019)	(0.019)	(0.045)
Overconfidence			0.014***	0.044***
			(0.004)	(0.007)
Controls	Yes	Yes	Yes	Yes
Observations	470	470	470	470

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Standard errors in parentheses. Treatment: 0=Hard Quiz, 1=Easy Quiz; Controls: Health Status, Monthly HH income and Over-Indebtedness Index.

Table 16: Overborrowing in the Game and in Real Life

	No. Goods			Overborrowing			Overspending		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Obj. Debt Index	0.055			0.001			0.039*		
	(0.076)			(0.009)			(0.022)		
Subj. Debt Index		0.137*			–0.010			0.054**	
		(0.080)			(0.008)			(0.022)	
Over-Indebtedness Index			0.081			0.008			0.046**
			(0.079)			(0.010)			(0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	465	465	465	465	465	465	465	465	465

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Standard errors in parentheses. Controls: Treatment and all variables listed in Table 13.

A Appendix

Survey Appendix

Table A.1: Subsample: Income Expectation Bias Dummy - Objective Debt Indicators

	Obj. Debt Index		DSR 2017		Rem. Debt/Income		Paid Late/Default	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bias Dummy	0.435***	0.395***	0.232***	0.172**	0.204***	0.182**	0.053	0.072*
	(0.120)	(0.122)	(0.066)	(0.068)	(0.071)	(0.074)	(0.037)	(0.037)
Constant	–0.122**	–1.218*	0.179***	–0.269	0.278***	–0.836**	0.135***	0.028
	(0.048)	(0.706)	(0.022)	(0.321)	(0.029)	(0.357)	(0.019)	(0.272)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	532	525	532	525	513	506	529	522
Adj. R-squared	0.034	0.096	0.038	0.084	0.020	0.084	0.002	0.031

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table A.2: Subsample: Income Expectation Bias Dummy - Subjective Debt Indicators

	Subj. Debt Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bias Dummy	0.118	0.173	0.134	0.181*	0.025	0.049	0.108	0.151
	(0.104)	(0.113)	(0.091)	(0.102)	(0.060)	(0.063)	(0.119)	(0.121)
Constant	–0.012	–1.168*	–0.034	–1.942***	1.366***	1.147***	–0.097	–0.293
	(0.055)	(0.701)	(0.048)	(0.562)	(0.035)	(0.436)	(0.070)	(0.795)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	532	525	532	525	530	523	532	525
Adj. R-squared	0.001	0.077	0.002	0.074	-0.002	0.034	-0.000	0.090

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table A.3: Subsample: Income Expectation Bias Dummy - Over-Indebtedness Indicators

	Over-indebtness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Bias Dummy	0.241**	0.187*	0.161***	0.123***	–0.002	0.000
	(0.101)	(0.109)	(0.042)	(0.042)	(0.032)	(0.036)
Constant	–0.074	–1.155*	0.134***	–0.100	0.141***	–0.286
	(0.055)	(0.673)	(0.019)	(0.266)	(0.020)	(0.231)
Controls	No	Yes	No	Yes	No	Yes
Observations	532	525	532	525	532	525
Adj. R-squared	0.009	0.053	0.031	0.062	-0.002	0.037

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table A.4: Wider and Narrower Bias Measures - Objective Debt Indicators

	Obj. Debt Index		DSR 2017		Rem. Debt/Income		Paid Late/Default	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Narrower Bias (20%)	0.433***		0.186***		0.180**		0.083**	
	(0.123)		(0.064)		(0.083)		(0.035)	
Wider Bias (30%)		0.357***		0.136***		0.214***		0.070**
		(0.094)		(0.050)		(0.078)		(0.028)
Constant	–1.018*	–1.101*	–0.120	–0.137	–0.296	–0.397	–0.067	–0.085
	(0.570)	(0.593)	(0.266)	(0.279)	(0.382)	(0.353)	(0.225)	(0.229)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	678	678	678	678	655	655	675	675
Adj. R-squared	0.089	0.084	0.086	0.077	0.052	0.059	0.026	0.025

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table A.5: Wider and Narrower Bias Measures - Subjective Debt Indicators

	Subj. Debt Index		Debt Position		Diff. Pay off Debt		Sacrifice Index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Narrower Bias (20%)	0.138		0.176*		0.055		0.029	
	(0.103)		(0.092)		(0.061)		(0.111)	
Wider Bias (30%)		0.054		0.095		0.022		–0.020
		(0.082)		(0.074)		(0.047)		(0.100)
Constant	–1.363**	–1.326**	–2.018***	–1.998***	1.023***	1.037***	–0.494	–0.453
	(0.562)	(0.564)	(0.475)	(0.472)	(0.349)	(0.350)	(0.668)	(0.673)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	678	678	678	678	676	676	678	678
Adj. R-squared	0.087	0.085	0.092	0.088	0.041	0.040	0.083	0.083

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table A.6: Wider and Narrower Bias Measures - Over-Indebtedness Indicators

	Over-indebtness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Narrower Bias (20%)	0.218**		0.136***		0.006	
	(0.101)		(0.041)		(0.032)	
Wider Bias (30%)		0.158**		0.108***		–0.004
		(0.080)		(0.033)		(0.027)
Constant	–0.943*	–0.961*	0.070	0.048	–0.312*	–0.304*
	(0.537)	(0.549)	(0.245)	(0.253)	(0.176)	(0.177)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	678	678	678	678	678	678
Adj. R-squared	0.051	0.048	0.062	0.058	0.040	0.040

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table A.7: All Biases - Incl. Lagged Dependent Variable

	DSR 2017	DSR 2017
	(1)	(2)
Bias Dummy	0.141**	
	(0.059)	
Debt Service Ratio 2016	0.401***	0.401***
	(0.147)	(0.147)
Fin. Forecast Error		0.027
		(0.020)
Constant	–0.037	0.070
	(0.241)	(0.228)
Controls	Yes	Yes
Observations	663	665
Adj. R-squared	0.159	0.148

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Table A.8: All Biases - Interaction with Conscientiousness

	Obj. Debt Index		Subj. Debt Index		Over-Indebtedness Index	
	(1)	(2)	(3)	(4)	(5)	(6)
Conscientiousness	0.009	0.007	0.033	0.003	–0.003	–0.001
	(0.040)	(0.037)	(0.055)	(0.053)	(0.036)	(0.034)
Bias Dummy	0.194		0.628		–0.120	
	(0.502)		(0.445)		(0.547)	
Bias Dummy × Conscient.	0.028		–0.092		0.044	
	(0.087)		(0.077)		(0.093)	
Fin. Forecast Error		–0.308		–0.163		–0.078
		(0.229)		(0.334)		(0.229)
Fin. FE × Conscient.		0.073*		0.047		0.032
		(0.040)		(0.056)		(0.039)
Constant	–1.055*	–0.865	–1.551**	–1.167*	–0.875	–0.840
	(0.596)	(0.608)	(0.617)	(0.656)	(0.541)	(0.559)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	678	667	678	667	678	667
Adj. R-squared	0.080	0.075	0.085	0.092	0.044	0.051

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Clustered standard errors in parentheses.

Experiment Appendix

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Table A.9: Descriptive Statistics by Participation in Game

	(1) Full Sample	(2) Participating	(3) Non-Participating	(4) Difference
Sex	1.66	1.63	1.76	0.12***
Age	57.01	56.35	59.78	3.43***
Relation to HH Head	1.67	1.66	1.71	0.05
Marital Status	2.15	2.14	2.22	0.09
Main Occupation	4.97	4.66	6.29	1.64*
Years of Schooling	5.74	5.83	5.33	−0.51*
Children (0-6 years)	0.32	0.32	0.33	0.01
Children (7-10 years)	0.24	0.23	0.25	0.02
Numeracy	2.05	2.13	1.69	−0.45***
Health Status	1.40	1.38	1.46	0.08
BMI	23.64	23.70	23.41	−0.28
Fin. Decision Maker	1.57	1.56	1.60	0.03
Self Control	21.26	21.02	22.26	1.24
Risk Taking	3.95	3.99	3.78	−0.21
Fin. Risk Taking	3.94	4.04	3.57	−0.47**
FL-Score	5.50	5.63	4.95	−0.68***
Monthly Inc. 2017	19197.02	19313.71	18704.57	−609.14
Obj. Debt Index	0.00	−0.01	0.02	0.03
Subj. Debt Index	−0.00	−0.01	0.03	0.04
Over-Indebtedness Index	−0.00	0.01	−0.04	−0.05
Morning	0.53	0.53	0.53	0.00
Midday	0.24	0.26	0.17	−0.09***
Observations	748	604	144	748

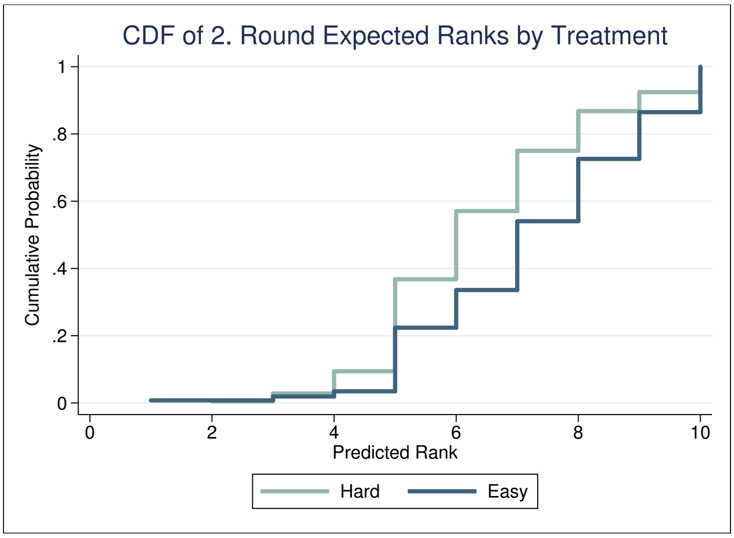

Figure A.1: CDF for the Expected Rank by Treatment, After the Main Quiz

Table A.10: Linear Probability Model Participation in Game

	Participation
Sex	−0.070*
	(0.036)
Age	−0.003**
	(0.002)
Fin. Risk Taking	0.018*
	(0.010)
FL-Score	0.020**
	(0.010)
Morning	0.089**
	(0.041)
Midday	0.134***
	(0.044)
Observations	679

Only significant variables reported, remaining variables are the same as in Table A.9.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: Descriptive Statistics for Excluded Sample

	(1) Full Sample	(2) In	(3) Out	(4) Difference
Sex	1.65	1.64	1.67	−0.03
Age	56.40	56.16	57.75	−1.59
Relation to HH Head	1.68	1.70	1.56	0.14
Marital Status	2.14	2.13	2.24	−0.11
Main Occupation	4.68	4.79	4.08	0.71
Years of Schooling	5.87	5.92	5.60	0.32
Children (0-6 years)	0.31	0.33	0.25	0.08
Children (7-10 years)	0.24	0.26	0.13	0.13***
Numeracy	2.13	2.14	2.04	0.11
Health Status	1.38	1.38	1.38	0.00
BMI	23.69	23.58	24.27	−0.68
Fin. Decision Maker	1.56	1.57	1.52	0.05
Self Control	21.05	20.94	21.62	−0.67
Risk Taking	3.98	4.02	3.74	0.28
Fin. Risk Taking	4.03	4.06	3.90	0.15
FL-Score	5.62	5.66	5.40	0.26
Monthly Inc. 2017	18523.65	18653.06	17798.04	855.02
Obj. Debt Index	−0.01	−0.01	−0.01	0.00
Subj. Debt Index	−0.01	−0.02	0.05	−0.07
Over-Indebtedness Index	0.01	0.03	−0.10	0.13
Read Alone	1.45	1.44	1.49	−0.04
Difficulties	1.15	1.14	1.21	−0.08
Observations	555	471	84	555

The Rationals

As mentioned above, so far we have excluded experiment participants who want to buy more than they expect to earn. We refer to these persons as "rationals." In this section, we discuss whether these participants are actually rational or had difficulties in understanding the experiment and how including these observations change our results. Comparing our main sample against all rationals does not yield results that differ substantially from those presented in Table A.11. However, if we divide the rationals into those participants who want to buy more than expected earnings could pay for but less than eight goods and those who want to buy exactly eight goods (which would be the "truely" rational decision), we find interesting differences. The former group has significantly lower education, numeracy, and financial literacy than the main sample. We see this as evidence that they may have had difficulties understanding the game (we will refer to them as non-rationals from here on). It does not seem to be the case, however, that these are persons who generally have problems controlling their own spending behavior (also outside the lab) because their debt to service ratio is significantly smaller compared to the main sample (see Table A.12).

Table A.12: Descriptive Statistics for Non-Rationals (only significant effects reported)

	(1) Full Sample	(2) Others	(3) Non-Rationals	(4) Difference
Years of Schooling	5.84	5.91	5.00	0.91***
Children (7-10 years)	0.24	0.26	0.12	0.14**
Numeracy	2.10	2.13	1.76	0.36*
FL-Score	5.60	5.64	5.10	0.54*
Debt Service Ratio 2017	0.23	0.24	0.14	0.09**
Observations	532	490	42	532

The remaining rationals, however, not only have significantly higher numeracy and financial literacy, as perceived by the interviewers, but also thought to have a better understanding of the game (for non-rationals the difference is in the opposite direction, but not significant). Thus, these participants might have took advantage of the set-up and reasoned that it is optimal for them to buy as many goods as possible because of the large discount.

Table A.13: Descriptive Statistics for Rationals (only significant effects reported)

	(1) Full Sample	(2) Others	(3) Rationals	(4) Difference
Main Occupation	4.70	4.76	3.48	1.28*
Numeracy	2.16	2.13	2.78	−0.66*
FL-Score	5.66	5.64	6.22	−0.58*
Difficulties in Game	1.15	1.16	1.00	0.16***
Observations	513	490	23	513

Including these two groups into the analysis, the results change as anticipated: the effect of expected rank on goods turns insignificant and negligible (see Table A.14). All other effects are almost unchanged.

Table A.14: Consumption Decision including Rationals

	Exp. Rank	No. Goods		
	(1)	(2)	(3)	(4)
Treatment	0.373**	–0.234		–0.254
	(0.168)	(0.199)		(0.199)
Exp. Rank			0.048	0.054
			(0.052)	(0.052)
Controls	Yes	Yes	Yes	Yes
Observations	511	511	511	511

Note: *, **, and *** denote significance at the 10, 5, and 1 percent levels. Standard errors in parentheses. Treatment: 0=Hard Quiz, 1=Easy Quiz; A higher expected rank corresponds to a higher expected performance.Controls: Health Status, Monthly HH income and Over-Indebtedness Index.

B Description of Variables

Debt Indices	
Objective Debt Index	It contains the equally weighted average of z-scores of three debt indicators. The procedure of aggregating these specific outcomes is adapted from Kling et al. (2007). It "improves statistical power" and helps "to detect effects that go in the same direction" among indicators (Kling et al., 2007, p.89). The debt index captures the debt service to income ratio, the remaining debt to income ratio, and a dummy variable turning one if the household paid late or defaulted on a loan during the last twelve months.
Over-Indebtedness Index	The index contains two measures of over-indebtedness: Households with a debt service to income ratio greater than 40% and households with more than four loans. The literature has defined (kind of arbitrary) thresholds for the DSR indicator beyond which a household is over-indebted. A household is deemed over-indebted, for example, if its DSR exceeds - depending on the study - 0.3 to 0.5 (Chichaibelu and Waibel, 2017). Hence, we set the over-indebtedness threshold at a DSR of 0.4 following what we deem is best practice among researchers (Georgarakos et al., 2010).
Sacrifice Index	This index is adapted by Schicks (2013), which asks for several sacrifices households may make because they lack money. Like them, we combine these indicators into one "sacrifice index" applying polichoric principal component analysis such that a continuous index is created giving more weight to more serious sacrifices people have to make and transforming the categorical responses into a continuous measure (Kolenikov and Angeles, 2009; Smits and Günther, 2017). In total, we ask respondents about ten possible sacrifices both for a shorter term (i.e. twelve months) and for a longer term (five years). Unlike Schicks (2013), we do not pose questions about the acceptability of sacrifices made but ask only for the frequency of distress events that occurred in the household. We added two questions introduced by Smits and Günther (2017) and two new questions that are more context-specific to the rural setting in North-East Thailand. Depending on the question asked, respondents could answer on a scale from 1-3 (e.g. had to work much more, more, not more) or from 1-5 (e.g. had to buy less food: never, sometimes, regularly, often, almost always, always).
Subjective Debt Index	It equally weights and averages the standardized sacrifice index and two assessments on whether the household has too much debt and whether it has difficulties paying them off.

Debt Measures	
Debt Service to Income Ratio	It is the ratio of all annual interest and principal payments on loans divided by all annual income generating activities of the household.
Debt Position	The question on whether the household has too much debt right now is asked twice in almost identical fashion to check for response consistency. For this reason, we combine both questions and calculate their mean. The exact formulation of both questions is the following: "I have too much debt right now" (Disagree fully, disagree strongly, disagree a little, neither agree nor disagree, agree a little, agree strongly, agree fully) and "Which of the following best describes your current debt position?" (I have too little debt; I have about the right amount of debt; I have too much debt right now.).
Difficulties to Pay Off Debt	Categorical question with answer options 1-"I have no difficulties paying off my debt", 2-"I have some difficulties [...]", and 3-"I have a lot of difficulties [...]".
Remaining Debt to Income Ratio	The ratio relates a household's actual, yearly debt burden to the average income of 2016 and 2017.

Expectation Biases	
Bias Dummy	Dummy taking the value 1 if expected median income from the probabilistic expectations elicitation is larger than actual income.
Financial Forecast Error	Difference between expected income in 2016 and actual welfare of the household as evaluated in 2017.

Expectation Measures	
Actual welfare of the household	Answer to "Do you think your household is better off than last year?", from 1-"much worse off" to 5-"much better off".
Certainty	Answer to "How certain are you that this income development will truly become reality?". The scale ranges from 1-"Very uncertain" to 4 "Very certain".
Expected income	Answer to "How do you think your average monthly income will develop in the next twelve months?", from 1-"Decrease a lot" to 5-"Increase a lot".
Probabilistic expectations	Probabilities assessing how individuals assess future outcomes.

Experiment Measures	
Treatment	1=Hard Quiz, 2=Easy Quiz.
Expected Rank	Rank that participant expects to reach after taking the test quiz from 1-“Least questions answered correctly” to 10-“Most questions answered correctly”.
Number of Goods	Amount of goods participant wants to buy.
Overconfidence	Difference between expected and actual rank of participant.
Overborrowing	Dummy variable, that takes the value 1 if participant wants to buy more than earnings including endowment can pay for.
Overspending	Dummy variable, that takes the value 1 if participant wants to buy more than earnings excluding endowment can pay for.

Controls	
Age	Age of respondent in years.
Age Squared	Squared term of age.
BMI	Respondents Body Mass Index as of 2017.
Financial Decision Maker	Answer to question “Who is responsible for making day-to-day decisions about money in your household?” where means 1-“Myself”, 2-“Myself and someone else” and 3-“Someone else”.
Financial Literacy Score	Our index is based on seven questions eliciting financial knowledge, on nine assessments concerning financial behavior, and on three questions regarding financial attitude. The overall index is composed of the sum of the sub indices and ranges between 0 and 22 with higher numbers indicating a higher level of financial literacy.
Financial Risk Taking	Answer to “Attitudes towards risk change in different situations. When thinking about investing and borrowing are you a person who is fully prepared to take risk or do you try and avoid taking risk?”, from 1-“Fully unwilling to take risks” to 7-“Fully willing to take risks”.

Health Status	Health status of the respondent in 2017: 1-“Good”, 2-“Can manage”, 3-“Sick”
Main Income Dummies	We include four income dummies that tell us whether the main income comes from farming, off-farm employment, self employment or remittances.
Marital Status	Respondents marital status: 1-“Unmarried”, 2-“Married”, 3-“Widow”, 4-“Divorced/separated”.
Monthly Inc. 2017	Monthly household income in 2017
Number of children	This variable is split in three age categories for the analysis. Number of children aged 0-6 years; Number of children aged 7-10 years; Number of children aged 11-16 years.
Number of Elders	Number of elder household members, defined as people older than 60 years.
Number of Shocks	Number of experienced shocks in 2017.
Number of Working Members	Number of working household members.
Numeracy	The numeracy index is based on six questions about simple arithmetic problems. It ranges between zero and six. Zero, if the respondent does not give any correct answer and six if the respondent gives only correct answers.
Optimism	We use the“Reevaluated Life Orientation Test” (LOT-R) of Scheier et al. (1994) and add up the Likert-Scale answers to one score. The scale ranges from 1-“Disagree fully” to 7-“Agree fully”. The final score ranges from 1 to 23 where higher numbers indicate a higher level of optimism.
Relation to HH Head	Respondents relation to the household head: 1-“Head”, 2-“Wife/Husband”, 3-“Son/Daughter”, 4-“Son/Daughter in law”, 5-“Father/Mother”, 8-“Grandchild”, 9-“Nephew/Niece”, 11-‘Other relatives”.
Risk Aversion	Equally weighted average of risk taking and financial risk taking.
Risk Taking	Answer to “Are you generally a person who is fully prepared to take risks or do you try to avoid taking risk?”, from 1-“Fully unwilling to take risks” to 7-“Fully willing to take risks”.

Self-Control	We use the questions introduced by Tangney et al. (2004) and add up the Likert-Scale answers to one score. The scale ranges from 1-"Disagree fully" to 7-"Agree fully". The final score ranges from 0 to 49 where lower numbers indicate a higher level of self-control.
Sex	Sex of respondent: 1-"Male", 2-"Female".
Total HH Education	Sum of years all working household members went to school.
Years of Schooling	Years respondent went to school.
Big Five - Personality Traits	
Agreeableness	A person, who scores high on Agreeableness (Item scale ranges from 1 to 7 for all items) has a forgiving nature, is considerate and kind and not rude to others.
Conscientiousness	A person, who scores high on Conscientiousnes does a thorough job, works efficiently and is not lazy.
Extraversion	A person, who scores high on Extraversion is communicative, talkative, outgoing and not reserved.
Neuroticism	A person, who scores high on Neuroticism worries a lot, gets nervous easily and is not relaxed.
Openness	A person, who scores high on Openness values artistic experiences, is original and has an active imagination.
Additional Controls Experiment	
Difficulties in Game	Answer to "Did the respondent have difficulties answering questions?" with 1-"Not at all", 2-"Yes, a little bit", 3-"Yes, very much". Filled in by the enumerator.
Morning	Dummy variable that takes the value 1 if the interview took place in the morning, i.e. before 11am.
Midday	Dummy variable that takes the value 1 if the interview took place around noon, i.e. between 12am and 2pm.
Read Alone	Dummy variable that takes the value 1 if the participant could read the experimental instructions without help. Filled in by the enumerator.