

Beyond Access to Finance: Essays on Financial Inclusion and Development

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Abstract

This dissertation contributes to enhancing the knowledge about financial inclusion by focusing on its consequences in emerging economies. Chapter 1 emphasizes the importance of financial inclusion, defined as access to and use of financial services, and relates the different chapters to the overall topic. Chapter 2 analyzes whether the mode of providing access to finance itself changes financial behavior and business outcomes. It analyzes a randomized controlled trial which provided a one-time cash transfer to micro and small entrepreneurs in Kampala, Uganda, in 2013. One half of the treatment group received the transfer in cash, the other half had the money transferred on their bank accounts, which we assume to work as a soft commitment device inducing entrepreneurs to rather use the money for business related expenses. We do not find any direct effect of the transfer on monthly profits and capital stock. However, we detect positive short-term treatment effects on the more “upstream” variables inventories and sales for entrepreneurs in the account treatment group.

Chapter 3 analyzes determinants of financial inclusion and focuses on financial literacy as a possible demand side driver of inclusion. It combines cross country data on financial literacy with information on financial inclusion, financial infrastructure, and other country characteristics. We establish a robust positive relation between financial literacy and four different dimensions of financial inclusion. Considering institutional variation across countries and regarding “access to finance”, financial literacy and financial infrastructure mainly substitute each other. With regards to the “use of finance”, higher financial literacy strengthens the effect of more financial depth. To respond to reverse causality concerns, we employ an instrumental variable strategy, which supports a causal interpretation of our results. Further robustness checks do not alter the findings either.

Last, Chapter 4 points out a possible drawback of using financial services. It asks whether too high expectations regarding future income may actually harm households and lead them to accumulate too much debt. We collect extensive data on debt and borrowing behavior of households in rural Thailand which enable us to calculate both expectations about future monthly income as well as various debt indicators. Controlling for specific household characteristics, we find a strong relationship between our two measures of biased expectations and (over-)indebtedness. The more quantitative expectation measure is stronger related to objective debt and the more subjective expectation measure is rather related to our subjective debt measure. An additional lab-in-the-field experiment shows that over-confidence is indeed related to over-borrowing.

Keywords: Cash transfer, financial literacy, financial inclusion, household over-indebtedness, lab-in-the-field experiment, emerging markets

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Chapter 1

Introduction

The life of money-making is one undertaken under compulsion, and wealth is evidently not the good we are seeking; for it is merely useful and for the sake of something else.

- Aristotle, *The Nichomachean Ethics*, I.5 -

Money is one of the key prerequisites for leading a good life. Individuals who face a lack of money do not just fall short of sufficient finances, but they are restricted in their freedom to attain the desired set of functionings that they deem important for their lives (Sen, 1999, 2003). Clearly, then, finances are ‘not the good we are seeking’ but they are ‘for the sake of something else’. In the same manner, the proficient management of finances is not an end in itself. Rather, the ability to save, invest, borrow, and to take out insurances is an important functioning that helps individuals increase their freedom and live the lives they envision.

Most individuals in industrialized countries are able to conveniently deal with financial matters using a diverse range of services offered by banks, insurances, and other financial service providers. However, access to such services is far from universal in developing and emerging economies. Households in these economies have developed sophisticated coping mechanisms in response to the lack of formal financial services (Collins et al., 2009; Banerjee and Duflo, 2012). Yet, these mechanisms cannot fully offset the effect of not having access to formal financial infrastructure. Therefore, individuals face greater risks with regards to unexpected shocks, they are constrained in taking out loans keeping them from investing in their education, health, or businesses, and their ability to save is limited due to insufficient safe storage places for money

(Collins et al., 2009; Cull et al., 2013; Dupas and Robinson, 2013a; Dupas et al., 2018). On a global scale, access to formal financial services is propagated as a key enabler to achieving various of the 17 Sustainable Development Goals. Eradicating poverty (SDG 1), ending hunger (SDG 2), achieving good health (SDG 3) and gender equality (SDG 5) might be the most prominent goals that are connected to having access to finance (Klapper et al., 2016).

Financial inclusion, defined as access to and use of formal financial services, has hence been pushed forward by both researchers and high level policy makers alike such as the G20 via the Global Partnership for Financial Inclusion, the United Nations, the World Bank, and the Alliance for Financial Inclusion. And indeed, there is good news. Since 2011, the number of adults owning a bank account worldwide has increased from 51 percent to 69 percent in 2017 (Demirgüç-Kunt et al., 2018). These are 1.2 billion adults who have gained access to financial services since 2011. The unprecedented surge in digital financial access contributes a fair share to this development. For example, studies related to digitized payment systems find more efficient service delivery and higher trust in the transfer providers and granting access to formal digital savings products indicates positive household welfare impacts (Demirgüç-Kunt et al., 2017; Karlan et al., 2016).

Nevertheless, about 1.7 billion people remain unbanked (Demirgüç-Kunt et al., 2018). The World Bank Group has, therefore, committed to the “Universal Financial Access Initiative” ambitiously envisioning that all adults worldwide will be able to access a formal bank account by the year 2020.

Simultaneously, a new frontier with respect to financial inclusion is emerging: It is to move beyond *access*, to reach an active and informed *usage* of financial products. Currently already, one fifth of formal bank accounts are dormant and two-thirds of mobile money accounts are not used regularly (Demirgüç-Kunt et al., 2018).

All initiatives for enhanced access to finance will fail to make a long-lasting impact, unless proponents of financial inclusion ensure that customers are able to make an informed and beneficial use of products, which are tailored to their needs and well regulated. Therefore, improved access to available financial products must be conjoined with capacity building on how to use them in order to help customers make informed decisions.

This dissertation contributes to advancing the understanding of financial inclusion with special focus on its consequences in emerging economies. First, it considers the effects of *access* to finance on business outcomes of micro and small enterprises (MSEs) (Chapter 2). It analyzes whether a financial product itself changes financial behavior

and business outcomes, thereby adding a new finding to the literature on cash transfers in the business realm. The subsequent chapter elaborates on the individual skill set that is beneficial for successful financial inclusion. On a macro level, it shows that financial literacy is an important driver of both access to *and* use of finance (Chapter 3). The last chapter focuses solely on the *usage* side and points out downsides of financial inclusion processes (Chapter 4). Due to their focus on the demand side, chapters 3 and 4 add an important and understudied aspect to the discussion of financial inclusion.

Road Map

Chapter 2, co-authored with Tevin Tafese, examines the effect of accessing formal finance for MSEs in Uganda. Providing access to finance for entrepreneurs follows the rationale that businesses have great growth potential because they likely realize high marginal returns to capital in response to the injection of financial means into the enterprises. Cash transfers are a way to alleviate possible constraints and have been thoroughly studied (see, for example, with respect to MSEs de Mel et al., 2008, 2012; Fafchamps et al., 2014; Berge et al., 2015; Blattman et al., 2016).

We add to the existing body of research by conducting a randomized controlled trial, which has a special twist. In addition to paying transfers, we vary the mode of receiving the money and analyze whether enterprises prosper relatively more upon obtaining a transfer into their bank account contrary to receiving the money in cash. Thereby, we implicitly assume the bank account to work as a commitment device, i.e. beneficiaries are more likely to use the money for business related expenses as opposed to on-the-go purchases, which might rather be made when the money is received in cash.

Hence, Chapter 2 ultimately evolves around the question if access to formal financial services and products themselves may drive positive changes in financial behavior. While we do not find any overall direct effects of the transfer on profit and capital stock, we detect positive treatment effects in the short term on more “upstream” business variables, specifically on inventories and sales. For these outcomes, the bank account indeed serves as a commitment device altering financial behavior because the effects are only traceable for the ‘account treatment’ group and not for those who received the money in cash.

Next, the dissertation turns to analyzing determinants of financial inclusion. Research at the country level has shown that better financial inclusion is related to characteristics such as more financial depth, clearer legal requirements, or low cost banking services (Allen et al., 2016). These determinants are solely related to the supply side of financial markets. Yet, functioning financial markets need not only adequate infras-

structure, but also informed customers, i.e. those with high financial literacy. Informed customers will more likely make sophisticated financial decisions and demand higher inclusion themselves. Therefore, Chapter 3 studies financial literacy and its role in advancing financial inclusion at the country level. This becomes feasible thanks to combining data on financial inclusion from the Global Findex Database with newly available macro data that contain information about the state of financial literacy in 143 countries (Klapper et al., 2015; Demirgüç-Kunt et al., 2018).

We contribute twofold to the literature. First, we indeed find a positive relation between financial literacy and four measures of financial inclusion. Two of these measures are rather concerned with providing access to financial services (account ownership, debit card ownership). The other two measure the extent to which these services and products are used (actively saving at a formal financial institution, using a debit card in the last year). The relation persists even when controlling for institutional and country characteristics. Second, financial literacy has a differing effect depending on the type of financial inclusion. Regarding the access to finance, the marginal effect of financial literacy decreases with higher financial depth, i.e. literacy can be substituted with a better institutional environment. However, with regard to the use of financial products, financial literacy and depth complement and even re-inforce each other. To respond to reverse causality concerns, we employ an IV strategy, which supports a causal interpretation of our results. In conclusion, we show that an informed client base equipped with sufficient financial literacy is as important to advancing the cause of financial inclusion as is increasing financial depth. The chapter is joint work with Antonia Grohmann and Lukas Menkhoff and has been published in *World Development*.

While Chapter 3 finds that the individual skill set matters to benefit from access to and use of finance, the last chapter points out a possible drawback of supply side led financial inclusion. Analyzing data from rural households in Thailand, it examines how overly positive expectations regarding future household income drive current debt levels and the likelihood to be over-indebted. Chapter 4 is joint work with Melanie Koch and Wiebke Stein.

Thailand is a prime example to study the consequences of financial access, as more than 80% of the population have quite recently gained access to bank accounts (Demirgüç-Kunt et al., 2018). We elicit extensive data that allows us to compile different debt indicators and to precisely calculate subjective expected household income, which includes measures on different moments of the expected income distribution.

We find a robust relationship between biased income expectations and (over)-indebtedness. The more income households expect to generate in the future, the more

debt they accumulate, even to a point where it becomes detrimental to their financial health. While accumulating debt with rising income is common and desirable, *expectations* about rising income do not account for the possibility that the future income will fail to rise as high as expected - making households more likely to become over-indebted.

The results vary with respect to different debt and expectations indicators. ‘Hard’ or objective debt measures, such as the debt service to income ratio, are more affected by the subjective expectation bias, which compares the amount of current and future expected income. In contrast, the qualitatively elicited financial forecast error is more strongly associated with ‘soft’ or subjective debt indicators measuring financial distress rather than actual debt. These results persist also with various robustness specifications. Additionally, certainty about future income affects debt holdings, too. The more certain respondents are about their income expectations, the more debt they accumulate. Paired with the uncertain environment the households are situated in, this increases the likelihood of falling into debt.

A lab-in-the-field experiment underpins these results by showing that overconfidence regarding income is systematically related to over-spending. However, it cannot establish causality because the exogenously varied level of self-confidence does not result in a significant difference regarding the propensity to over-borrow. The paper contributes to the small but growing literature on (over-)indebtedness and its drivers. From a policy perspective, the findings call for building and improving knowledge about financial products on the one hand, and about households’ financial situations on the other hand.

In conclusion, this dissertation offers insights into financial inclusion in emerging economies. There remain two contributions particularly noteworthy to the reader and for the advancement of access and use of financial services in general. First, not only the possibility to *access* finances bears positive outcomes on development but the *mode of accessing* these resources alters financial decisions themselves. This may have far reaching implications regarding the design of financial products. For example, government transfers could be rolled out on bank accounts such that households’ savings behavior is influenced positively. Moreover, formal financial service providers should develop products conducive to their clients’ ‘financial health’ - especially in the context of emerging economies, where access to finance has spread only recently (Demirgüç-Kunt et al., 2017). Second, the skills and financial behavior of customers matter for the *use* of financial infrastructure. Hence, from a demand side point of view, policies should center around fostering financial knowledge and transparently communicating benefits

and drawbacks of financial products, such that trust in the formal financial system is built and a deepening of financial inclusion beyond access becomes feasible.

Chapter 2

Rethinking the Effectiveness of Cash Transfers - Evidence from a Field Experiment in Uganda¹

with:
Tevin Tafese

¹ This paper builds upon the author's master thesis. We would like to thank seminar participants in Hannover and Hamburg for helpful comments and suggestions on this essay, in particular Lena Giesbert, Stephan Klasen, Jann Lay, Lukas Menkhoff, Sebastian Prediger, and Helke Seitz.

2.1 Introduction

Cash transfer programs have spread rapidly across over 120 countries in recent years and are a popular tool to fight poverty (World Bank, 2018a). The expansion of cash transfer programs has been accompanied by a growing body of studies evaluating their efficacy.² Indeed, most of them positively affect a broad range of outcomes, such as children’s educational attainment (Baird et al., 2014), access to and use of health care facilities (Cahyadi et al., 2018), nutritional status (Manley et al., 2013), and household consumption (Haushofer and Shapiro, 2016).

A special type of cash transfers are grant or loan focused programs aimed at individuals who want to establish micro or small enterprises (MSEs) or who already run them. While these programs vary in size and scope, they follow the rationale that the (potential) entrepreneurs have good ideas and stamina, hence, might realize high returns to capital, but are financially constrained to borrow, save, or invest (Banerjee and Duflo, 2012; Dupas and Robinson, 2013b).³ Cash transfers or grants may alleviate these financial frictions and help entrepreneurs grow their businesses and increase earned income (Blattman et al., 2018).⁴ Acknowledging the popularity and policy relevance of cash transfer programs, a substantial body of research discusses whether the mechanism of cash injections lifting capital constraints holds true, and if yes, under which conditions (see de Mel et al., 2008; McKenzie and Woodruff, 2008; de Mel et al., 2012; Fafchamps et al., 2014, among others). We build upon this existing work and examine whether a cash transfer of 300,000 Ugandan Shilling (UGX, about 100 PPP USD) to 96 micro and small entrepreneurs in Kampala, Uganda, affects their businesses’ profitability.

Adding to the well developed literature on cash transfers, we introduce a special twist in our randomized controlled trial: We additionally examine whether the impact of the cash transfer varies with the *mode* of receiving it. Research in behavioral economics and psychology shows that people’s actions often hinge on the default option, i.e. individuals do not make an active choice but silently agree to the default that is put in place for them (Thaler and Sunstein, 2008; Somville and Vandewalle, 2018). Following this logic, we test whether the mode (i.e. the default option) of receiving the unexpected cash transfer affects MSE owners differently due to their initial inertia. That is, half of our treatment group received the transfer directly in cash, while for the other half of

² For a comprehensive and rigorous review of the literature consult Bastagli, Hagen-Zanker, Harman, Barca, Sturge, and Schmidt (Bastagli et al.).

³ Baird et al. (2018) coin the channel through which these transfers work the “self-employment liquidity effect”.

⁴ We omit discussing the literature on microcredit here although it is based on a similar mechanism.

the treatment group, we deposited the respective amount on their bank accounts. We assume that business owners who received the transfer in cash are more likely to spend the money right away as it is immediately available. Receiving the money on the bank account, however, might work as a soft commitment device for these entrepreneurs and help them to use it in a more organized manner, such as for business investments. All treated businesses are located in the urban centre of Kampala and bank branches or ATMs are nearby. Thus we expect transaction costs for the account treatment arm to be negligible. Any impact of the treatment for the latter group would consequently be due to the “default-option” effect of depositing it in the account and not due to high transaction costs. Following de Mel et al. (2008) and using detailed survey information on both entrepreneurs’ businesses and households, we study the impacts of the cash transfer on business profits and capital stock. Furthermore we investigate whether inventories, sales, and (business) savings change in response to the positive shock. As the randomized trial is embedded in a longer term project on micro-enterprise growth in sub-Saharan Africa, we are able to trace entrepreneurs not only in the short term (i.e. 6 months after they receive the cash grant), but up to four years after the intervention.

Beneficiaries report a significantly different usage of the transfer by treatment arm when asked about the subsequent use of the money in the short-term follow-up six months after the intervention: Entrepreneurs in the account treatment save significantly more of the transfer compared to the cash receivers. In contrast, those in the cash treatment arm report to invest significantly more money into their businesses. These responses indicate that depositing the transfer on the bank account may possibly work as a soft-commitment device and that entrepreneurs in the cash treatment arm nevertheless use the money for business purposes despite money being fungible and the enumerators not framing what beneficiaries should do with the transfer.

Our intent-to-treat effects, however, do not mirror these self-reported survey results. Although we find weak evidence for positive treatment impacts on upstream business variables such as inventories and sales for the account treatment group in the short term, they do not translate into sustained higher capital stocks or profits. In line with Fiala (2018), we do not detect any overall direct effect of the cash transfer on profits and capital stocks for any of the treatment arms, in the studied time period. Further, our heterogeneous treatment effects analysis reveals that men and women are not affected differently by the grant, neither in the cash nor in the account group. We find evidence for heterogeneous treatment effects only with respect to the level of education and baseline business savings of the entrepreneur. In both treatment arms, entrepreneurs with higher baseline business savings generate higher monthly sales. Interacting the

treatment with high education at baseline (i.e. at least having completed the A-levels), results in higher savings for entrepreneurs in both treatment groups. However as before, profits and capital stocks remain unaffected. Summarized, the way the money is handed out to recipients does not affect its subsequent use. Perhaps more importantly our (non-)results fail to provide evidence for the cash transfer to have an effect on micro and small enterprises. In line with a number of recent studies, our findings call into question the effectiveness of cash transfers as a means to spur growth in micro and small enterprises.

Literature

The paper relates to two main strands of literature: (i) We add to the extensive body of research on micro enterprise growth by presenting heterogeneous results with respect to the initial capabilities the entrepreneurs have to run their business successfully. Furthermore we are able to show mid to long term results of cash transfers for micro and small businesses. (ii) Studying different modes of receiving the transfer allows us to examine whether the transfer on a bank account induces a default effect and hence influences financial decision making behavior.

(i) Cash transfer programs are typically seen as an effective tool to help firms overcome credit constraints and increase business profits in the short and medium term (e.g. de Mel et al., 2008, 2012). Among others, Blattman et al. (2016) vary the type of a cash transfer by additionally providing business training and supervision on top of a cash grant to “ultra-poor” women in Uganda and find large income gains after 18 months. In Sri Lanka and Ghana, micro and small business owners receive grants either in cash or in-kind (de Mel et al., 2012; Fafchamps et al., 2014). They result in positive effects on earnings for existing entrepreneurs in the short and medium term, whereby in Ghana, only female entrepreneurs benefit from the in-kind grant and there is no effect from providing cash. Other studies also fail to find overall significant effects of cash transfers: Berge et al. (2015) for example state that cash grants are often consumed quickly instead of being used for investments by the entrepreneur. They find positive heterogeneous results of the intervention with the effect being more pronounced for male-led enterprises. Studying a grant, loan, and training intervention, Fiala (2018) cannot report any income effects from the grant treatment arm on neither female or male led businesses. He also fails to find an impact of any treatment for female businesses. A cash grant for young men engaged in petty crime in Monrovia, Liberia, also does not find lasting effects on business outcomes, which may be due to the dire conditions the men live in as the authors suggest (Blattman et al., 2017). Newer studies explore whether cash transfer programs prevent the inter-generational

transmission of poverty. In a 10-year follow-up of a cash transfer in Ecuador, Araujo et al. (2017) report at most modest effects from the intervention on schooling outcomes of children. Blattman et al. (2014) and Haushofer and Shapiro (2016) present positive short-term results of two distinct cash transfer interventions on skilled self-employment and psychological well-being, respectively. Blattman et al. (2018) and Haushofer and Shapiro (2018), however, find that these effects dissipate over time. Blattman et al. (2018) point out that the convergence effect of the control group over time is often neglected in short term studies. Cash transfers might just shift entrepreneurial investments in time, but, as time goes by, non-treated entrepreneurs may also accumulate money and realize similar investments causing the initial effects on earned income to fade away. Our study adds to the longer term literature by accounting for changes up to four years after the intervention took place.

Another part of the literature on micro enterprise growth deals with the heterogeneity of the informal sector beyond the effect of entrepreneurs' gender. It distinguishes between a lower tier home to necessity entrepreneurs or survivalists and an upper tier consisting of high growth enterprises (Henrekson and Johansson, 2010). Grimm et al. (2012) develop the concept of "constrained gazelles" entrepreneurs, i.e. business owners who exhibit high growth entrepreneurial potential (e.g. a high education, yet young age of the firm) but are limited by the lack of capital. This group tries to reach the upper tier but fails to do so. Evidently, exactly targeting these gazelles would improve the effectiveness of cash transfer programs. More recent studies succeed in identifying this special subgroup of entrepreneurs either via community panel decisions (Hussam et al., 2018) or through business plan competitions (Fafchamps and Woodruff, 2016; Fafchamps and Quinn, 2017; McKenzie, 2017). Winners from these competitions are, among other things, more likely to be self-employed and to have more employees compared to those that rank second or third in the competition. Hence, cash transfers prove especially useful when targeted at the most promising group of (potential) entrepreneurs. Our heterogeneous findings are in line with this evidence as we check for characteristics that determine successful entrepreneurship such as the level of education and the amount of savings.

(ii) The intervention studied here exploits variation in the mode of payment. We hypothesize that the deposit in the bank account works as a soft commitment device and hence enables us to study financial behavior. Research on savings behavior in particular finds that opposed to standard models of decision making, individuals tend to stick to the default option more often as is predicted (Choi et al., 2003). These "default options" may be used to "nudge" people toward an outcome profitable for

them (Thaler and Sunstein, 2008). Empirical examples for this are for example the research on 401(k) savings plans (Madrian and Shea, 2001) and on (default) organ decisions (Johnson and Goldstein, 2003). More similar to the context our work is based in, Fafchamps et al. (2014) detect a “flypaper effect” when in-kind transfers are made into small businesses in urban Ghana. While there is no effect on business outcomes for the group that received cash, the in-kind transfers remain in the business and result in increased profits for larger enterprises. A similar mechanism may play a role in our intervention. Business owners’ inertia may keep them from directly consuming the transfer deposited in the account and rather help them use it for productive purposes.

Lastly, our paper is related to work by Somville and Vandewalle (2018) and Brune et al. (2017) who study the effect of a cash transfer on savings behavior. These papers provide money either in cash or on a bank account to households in India and Malawi. While Somville and Vandewalle (2018) confirm a direct and large impact of the repeated small transfers on household savings, Brune et al. (2017) find that a one-time-transfer only has limited effects on consumption and savings. Brune et al. (2017) explain these differing results by the fact that Somville and Vandewalle (2018) repeatedly deposit money in individual bank accounts and only find significantly different results after individuals have already received the transfers for several weeks. The work presented here examines a one-time cash transfer as in Brune et al. (2017) and differs from the two studies cited above mainly in terms of the target group. We specifically focus on business owners and do not only study the direct effects on the account savings balance but take into account business related outcomes such as profits, capital stock, sales, and business inventories.

The paper proceeds as follows: Section 2.2 introduces the study background and Section 2.3 the data used. It discusses the procedure of randomly allocating treatment and control groups, presents an attrition analysis and describes what respondents subjectively state to use the transfer for. Section 2.4 describes the estimation strategy along with main treatment results and heterogeneous treatment effects. Section 2.5 discusses limitations of our study and presents robustness tests before Section 2.6 concludes.

2.2 Background and Study Design

In this section we first briefly introduce the context the intervention was held in and then proceed to describe the experimental set-up in more detail.

Background

The study took place in urban Kampala which is the capital of Uganda. The country has seen considerable economic growth that slowed down from on average 7 percent annually during 1990-2010 to 4.5 percent p.a. in the five years prior to 2016, which also comprise the intervention period (World Bank, 2018b). In 2013, the time when the intervention set in, about 81 percent of the working-age population were self-employed, predominantly working in agriculture, followed by micro-enterprises. The latter cover about 90 percent of private sector production and employ over 2.5 million people (Fiala, 2018). While population growth and increased life expectancy will add more and more young people to the labor force in the future, they will face few wage employment opportunities and thus, self-employment is likely to expand. The majority of micro enterprises, however, face various constraints to business growth, of which lack of capital and missing skills are most important.

The intervention tackles capital constraints in particular and focuses on the difference between transferring an unconditional grant directly in cash versus on a bank account. In 2017, 59 percent of Ugandans owned some form of formal bank account compared to 44 percent in 2014 and 20 percent in 2011 (Demirgüç-Kunt et al., 2015, 2018). Account ownership has been boosted substantially by the spread of mobile money accounts which outweigh by far the sole ownership of a bank account at a financial institution. Hence, provided that the account treatment really induces a soft-commitment effect, using the financial infrastructure via (mobile) cashless banking could promise an avenue for scalable interventions lifting capital constraints from business owners in the future.⁵

Experimental Design

The intervention was conducted by the German Institute of Global and Area Studies (GIGA, Hamburg) in collaboration with the Centre for Basic Research in Kampala.

⁵ Somville and Vandewalle (2018) motivate their research by efforts of the Indian government to spread cashless banking throughout the sub-continent. While they do not examine the policy's consequences for micro enterprises, this paper shows possible implications for this sub-group.

The study is part of a wider research project on “Performance and Dynamics of Micro and Small Firms in Developing Countries”. Listing of businesses for the final sample was carried out together with the first baseline survey in October 2012. In April 2013, a second baseline survey was conducted followed by the implementation of the treatment. Trained enumerators interviewed the sample in a follow-up survey in October 2013 succeeded by further surveys in October 2014, 2015, 2016, and 2017. Entrepreneurs answered comprehensive questionnaires covering firm characteristics and performance, and provided information on personal and household characteristics including their financial situation and behavioral attitudes. We calculate a pooled treatment effect over all waves alongside a short-term effect including October 2013 and October 2014 and a rather long-term effect covering possible impacts from 2015 to 2017.

The overall baseline sample was drawn using a two-stage sampling procedure. A subset of this sample was eligible to participate in the intervention in either the control or the treatment groups. It was drawn in the following way: First, 16 out of 220 geographical business zones in Kampala were randomly selected and all firms were listed therein.⁶ Second, 450 micro or small enterprises were randomly drawn stratified by industry branches.⁷ Finally, a random sub-sample of entrepreneurs was chosen to participate in the cash transfer intervention. In order to effectively capture micro and small enterprises, the random sample excluded enterprises with more than five employees and the ten percent enterprises with highest capital stocks (measured in October 2012). Table 2.1 shows the distribution of businesses across industries. Note that businesses in the “hair dressing and beauty” industry were oversampled in order to balance out male over-representation in branches such as “retail electric, phones, household appliances and related services” or “manufacturing”.

[Table 2.1 about here.]

To avoid a possible “double-intervention”, only banked entrepreneurs were eligible for the money transfer: The treatment effect would otherwise not be distinguishable from either opening a bank account per se or depositing the cash grant on the account had non-banked entrepreneurs been included in our sample draw. In fact, baseline data reveal that non-banked entrepreneurs exhibit significantly lower profits and capital

⁶ We applied cluster sampling: The zones were selected with a probability proportionate to the number of enterprises listed within them.

⁷ Industries were divided in “hair dressing and beauty”, “manufacture of printing/paper products and related services”, “manufacture of textile/wearing apparel (tailors) and related services”, “manufacturing (remaining sectors)”, “retail and wholesale (remaining sectors)”, “retail clothing, footwear, and leather”, “retail electric, phones, household appliances and related services”, and “other”.

stock, and lower levels of education. We therefore cannot draw conclusions on the effect of the transfer on the overall population but have to restrict our contribution to banked persons who, as already mentioned, represent more than half of the Ugandan population.

The experiment had two treatment arms. Originally, 50 businesses were randomly drawn to receive the cash transfer directly while 50 other enterprises were assigned to receive the grant on their bank account. 80 entrepreneurs were sampled for the banked control group. Several businesses had stopped working or shifted premises or could not be tracked for other reasons at the time when the intervention was to be realized (April 2013). These entrepreneurs were randomly replaced with other business owners that had answered the baseline survey from October 2012 already. Furthermore, three businesses differed in their intended and actual treatment status.⁸ Thus, in the main part of this paper, we estimate *intent to treat estimates* based on assignment to treatment in October 2012.

The final sample for our analysis is shown in Table 2.2.⁹ Differences in planned and actual number of respondents arise due to the following reasons: One entrepreneur was listed twice and randomly selected for both the cash and the account transfer. We dropped these observations from our sample since receiving double the amount of money could distort our results. One business owner assigned for the account treatment was dropped from the sample because an employee instead of the actual owner had been interviewed. Furthermore, two entrepreneurs from the intended control and the cash groups, respectively, could not be interviewed at time of the intervention and did not get replaced. Last, baseline data for one entrepreneur in the account treatment group was deleted, because a wrong person had been interviewed. This explains why there is an additional respondent in the account treatment group in April 2013.

[Table 2.2 about here.]

The transfer amounted to 300,000 UGX which represents roughly monthly pre-intervention profits for the median firm in our sample. Receipt of the transfer was framed as remuneration for participation in the survey. Business owners were free to use the money as

⁸ One entrepreneur was assigned to receive the account transfer, but turned out not to have a bank account. Another business owner that was assigned to receive an account treatment actually received the money in cash. The same happened for a person who was assigned for cash but who received the money via a bank account transfer. These business owners differed in their intended and actual treatment status.

⁹ Table A.1 in Appendix A shows the distribution of respondents who actually received the transfer or were actually included in the control group. Higher numbers in the control group arise due to more entrepreneurs having a bank account than previously stated at baseline in October 2012.

they liked. In this, we follow de Mel et al. (2008) line of argument who state that not restricting the use of funds results in more honest reporting in subsequent waves. While the cash group received the money directly after they had responded to the April 2013 survey, the account treatment group received the money between one to fourteen days after the interview had taken place due to the enumerator team rechecking banking details and the banks' need for time to transfer the money. The control group received the standard remuneration of 7,000 UGX that was regularly paid out after answering the questionnaire.

2.3 Data

This section explains how our main dependent variables are measured and deals with differential attrition over time. It also provides baseline summary statistics and survey results on what entrepreneurs state to use the cash transfer for.

2.3.1 Measurement of Main Variables

In line with de Mel et al. (2008), our main outcome variables are the firm's monthly profits and capital stock. Basic treatment effects on these variables are calculated both in levels and logs to increase the robustness of the analysis. Furthermore, we test whether the transfer affected the amount of business inventories, sales, and total savings.

As de Mel et al. (2009b) recommend, profits were elicited directly from the respondent by asking a single survey question.¹⁰ According to them, this yields more precise estimates and performs better than calculating profits from detailed expenditure and revenue information. Field work experience confirms this view. Hence we are optimistic to use a realistic profit measure in our analysis. Similar to profits, total monthly sales are elicited directly from the entrepreneur.

Capital stock is derived from asking about business assets with respect to machinery, furniture, business tools, vehicles, land and business premises, and other remaining assets. Categorizing assets along these lines is supposed to help the entrepreneur recall assets as comprehensively as possible. Business owners did not only report the quantity but also the replacement value of assets at the time of the survey and whether they are rented or owned. These pieces of information are provided in each wave whereas the

¹⁰ Enumerators ask the following: "What was the total profit the business made in the last four weeks after paying all expenses including wages of employees, raw materials, items for resale, electricity, water, fuel, rental etc.?" A second question elicits whether stated profits include the entrepreneurs personal income. If this is not the case, personal income is added to the profit measure.

replacement value of the former wave is repeated in the preceding survey as a built-in anchor. Values are corrected for if the respondents state that the past replacement value for a specific item is unrealistic. This is particularly done so for the October 2012 survey. While the replacement value accounts for asset depreciation, inflation is not considered. The capital stock measure we use in our analysis comprises the various asset groups except for the value of land and buildings because respondents find it hard to state a specific value and tend to overstate these values.

More specifically, inventories include the value of all items held as consumables, raw materials and finished goods. For the case of a carpenter for example, this means raw materials being wooden planks and finished goods being a bed, window frame, door etc. As the value of all raw materials and finished goods is asked generally without precisely pinning down goods one by one, the values are likely prone to measurement error. However, entrepreneurs preponderantly reported to use the cash transfer to buy inventories and equipment, so we include this measure in the regression analysis nevertheless.

Last, total savings amount to the sum of savings at home, savings on a bank account, on a mobile money account, at a savings club, and savings that are kept with neighbors or trusted friends. Business savings comprise only the money which is labeled for specific use in the business. All monetary measures are deflated to price levels in 2011 and stated in 1,000 UGX.

2.3.2 Sample Attrition

Short-term attrition between April and October 2013 was 3.4 percent while longer-term attrition up to the latest survey in 2017 amounts to 36.6 percent. Table 2.2 suggests that a higher share of business owners from the control group could not be interviewed in the follow-up waves contrary to the treated entrepreneurs. To test for differential attrition in our sample, we regress interview status in the follow-up round on treatment assignment for each wave separately and over all waves at once. We estimate the regression in the following form (see Dupas and Robinson, 2013b):

$$y_i = \beta_0 + \beta_1 \text{AccountTreatment} + \beta_2 \text{CashTreatment} + \epsilon_i \quad (2.1)$$

The dependent variable y_i equals 1 if the person could not be surveyed in the respective follow up wave, *AccountTreatment* and *CashTreatment* are dummy variables turning one if the respondent received the grant on a bank account, or in cash, respectively. The constant β_0 reflects the likelihood of the omitted treatment group, i.e. the respondents assigned to the control group, to not be followed-up. Table 2.3 depicts the results.

[Table 2.3 about here.]

Entrepreneurs in both treatment arms were not more likely to be untracked in the follow-up waves than their control group counterpart. Even contrary, those who received the transfer in cash were more likely to be surveyed in the pooled regression over all waves (Table 2.3, column (6)). There is, however, differential attrition with respect to the control group as the coefficient for the constant is significantly positive throughout the waves (except for October 2016). Perhaps control entrepreneurs expected some kind of higher return for participation in the survey and refused to be interviewed upon noticing there was no reward besides the remuneration fee to be gained.

Business owners who left the sample exhibit different characteristics than those who stayed in the sample at baseline, i.e. during waves one and two. (for details see Table A.2 in Appendix A). They were generally better off than those who were interviewed in subsequent waves. Among other characteristics, these business owners were younger, more affluent, more likely to have a university degree and to speak English fluently. Their firms were younger, they had higher start-up capital and savings while they generated more profits and higher sales. Acknowledging these differences, we apply Lee-bounds to our analysis in Section 2.5.

2.3.3 Summary Statistics and Balance Tests

We first present a short summary of important baseline characteristics for the whole sample before we turn to evaluate the randomization procedure. McKenzie (2012) shows that using multiple pre- and post treatment rounds in impact evaluations increases estimate precision especially when outcome variables are likely to be noisy and relatively less auto-correlated such as - as he states - business profits, and household incomes and expenditures. Thus, we use data from the first two pre-treatment rounds as an average baseline and present summary statistics and results from balance tests regarding randomization into treatment and control groups in Tables 2.4, 2.5, and 2.6. Items that were only elicited in one of the pre-treatment waves are marked with an asterisk.

[Tables 2.4, 2.5, and 2.6 about here.]

At baseline, there are more male than female business owners in our sample (59 percent vs. 41 percent) who are mostly between 25 and 47 year old. 68 percent of entrepreneurs are married and the median household has 5 members. 95 percent of sampled business owners report to be literate. 10 percent did not finish primary school, 30 percent completed primary school, 24 percent finished middle school, 18 percent completed their

A-levels and 15 percent completed university. The median business owner responds correctly to 7 out of 10 questions testing financial literacy and works about twelve hours a day for six days a week in the business. 84 percent of business owners are rather impatient, i.e. they would prefer to receive 20,000 UGX in one week's time over receiving 30,000 UGX in five weeks' time. Regarding firms at baseline, the median firm age is about 5 years, it has one employee, and median monthly profits range at 350,000 UGX while the median capital stock lies at 738,000 UGX. The median business sells items worth about 1,705,500 UGX a month and incurs costs of about 1,175,000 UGX which are split into costs for raw materials, finished goods, and other expenses such as business rent or electricity payments. Only 21 percent of all micro enterprises are formally registered with the Ugandan revenue authority.

There are significant differences with regard to some baseline characteristics which are most probably due to the small sample size.¹¹ Specifically, entrepreneurs in the account treatment group are less likely to work in the trade sector, but more likely to work in the services or other business sectors (Table 2.4). Moreover, entrepreneurs in the account group have significantly more employees and work slightly less in their business than their control group counterpart. There is a higher share of business owners with completed O-levels in the account group. While their businesses had significantly less start-up capital at their command, their monthly value-added is higher than that of the control group.

Entrepreneurs in the cash treatment group also differ from control business owners in various ways (Table 2.5): On average, their households are significantly bigger and they are older than those in the control group. They are less likely to work in the central (and busy) division, but rather likely to operate in the Makindye Division a little more outside of the city center. They rather work in the services and other sectors as compared to the control group and - similar to the account treatment arm - there is a higher share of entrepreneurs with completed A-levels. Also, businesses in the cash treatment group produce more value added while having had less start-up capital at hand.

Lastly, cash and account treatment also exhibit significant differences with respect to the location of their work, the educational level, and entrepreneur's age (Table 2.6). At least, differences between the two treatment arms are few and there are no significant different business characteristics. This is good news since we are especially

¹¹ One reason for the randomization failing partly might be that the randomization only happened between treatment and control and not between the different treatment arms and the control group. Differences between the overall treatment and control group are less pronounced.

interested in whether the cash grant had a differential impact on either of the treatment arms. However, acknowledging these differences, we control for baseline values of the respective variables in each of the subsequent regressions.

2.3.4 Self-Reported Evidence on Use of Cash Transfer

In the first follow-up wave in October 2013, six months after the intervention had taken place, business owners are asked to state what they used the money for. 10.4 percent (5 entrepreneurs) of those in the account group had not withdrawn any money yet. 30 out of 48 account entrepreneurs withdrew at least some money from the transfer 1-2 weeks after arrival. Three business owners in the account treatment saved the money for a while and withdrew only in August 2013 or later. In the cash treatment arm almost half of treated business owners (22 out of 46 persons) deposited between 40,000 UGX and 300,000 UGX at their bank accounts after receiving the transfer.

[Figures 2.1 and 2.2 about here.]

Figures 2.1 and 2.2 summarize the subjectively stated use of the monetary transfer by treatment group and by gender, respectively. Generally, entrepreneurs state to use the money predominantly to invest in business inventory and equipment for current or new businesses.

[Tables 2.7 and 2.8 about here.]

According to the subjective data, business owners in the cash group spend significantly more money on business inventory and equipment than treated account entrepreneurs (212,045 UGX vs. 167,917 UGX) (Table 2.7). Most entrepreneurs increase their expenditures on stock depending on the business sector (e.g. electrical appliances, hair products, clothes, fabric, timber, food). Some use the money as “top-up” and invest larger amounts in, e.g. new machinery for wood workshops. Expenditures also include costs for business rent, and electricity. Conversely, respondents in the account treatment group save significantly more money than those in the cash group (42,708 UGX vs. 9,090 UGX). There are no other significant differences between the treatment groups. Asked for whether any of the entrepreneurs in the treatment groups introduce new or innovative products, two sided t-tests reveal no significant difference between the two treatment arms and between treatment and control group.

Splitting treated entrepreneurs by gender reveals that women invest higher amounts in business inventory and equipment for their current business as men while men invest

also in new businesses (Table 2.8). Contrary to our expectations, women do not state to use significantly more money for household expenditures than men. However, as money is fungible these descriptive data only provide some rough pattern on the possible use of the transfer.

2.4 Results

This section first explains the empirical strategy we employ before reporting the main results of the cash transfer. We then proceed to present results from a heterogeneity analysis with respect to the gender of the business owner, baseline educational status and baseline (business) savings.

2.4.1 Estimation Strategy

To test the hypotheses introduced in Section 2.1, we estimate the following intention to treat (ITT) model:

$$Y_{it} = \alpha + \beta_1 \text{Account}_{it} + \beta_2 \text{Cash}_{i,t} + \delta_t + \theta \bar{Y}_{i0} + \sum \gamma_i X_{i0} + e_{it} \quad (2.2)$$

where i refers to a specific business, t is time and $Y_{i,t}$ represents the outcome of interest. $\text{Account}_{i,t}$ takes the value one from the first post-treatment round on if the business owner was assigned to receive the transfer on the bank account. Likewise, $\text{Cash}_{i,t}$ is a dummy variable which indicates whether the entrepreneur belongs to the cash treatment arm. We include $\bar{Y}_{i,0}$ - the baseline outcome value of the dependent variable for firm i - in the regression as it increases statistical precision (McKenzie, 2012). Additionally, the matrix $X_{i,0}$ controls for those baseline characteristics that were different between treatment and control arms, i.e. the divisions and industries the businesses operate in, educational levels and age of the entrepreneur, initial wealth, the amount of start-up capital, the number of employees and firm age. In our analysis we make use of two available baselines and pool them to increase statistical power, because data on business outcomes are often found to be quite noisy (de Mel et al., 2009b). Moreover, δ_t are wave fixed effects, and $e_{i,t}$ is the error term. All standard errors are clustered at the firm level and robust.

To increase statistical power we present results from a pooled regression over all waves as well as short term results only using data from the first two post-treatment waves (October 2013 and October 2014) and longer term results using data from the

last three survey waves (October 2015, October 2016, October 2017). We explicitly include the long-term measure as recent research by e.g. Blattman et al. (2018) points out the need for longer term evaluation of cash transfers.

To further lower the impact of outliers on our results, we adapt the trimming procedure of de Mel et al. (2008) by trimming the top and bottom 5 percent of observations whose profits change most positively and most negatively between waves. We include both absolute as well as percentage changes in our measure.¹²

As already mentioned, we analyze heterogeneous treatment effects in a second step employing a similar estimation framework as above:

$$Y_{it} = \alpha + \beta_1 Account_{it} + \beta_2 Cash_{it} + \beta_3 male_{i0} + \beta_4 (Account * male_{i,0}) + \beta_5 (Cash * male_{i0}) + \delta_t + \theta \bar{Y}_{i0} + \sum \gamma_i X_{i0} + e_{it} \quad (2.3)$$

The coefficients β_4 and β_5 display the interaction effect of being male and the respective treatment. The coefficients β_1 and β_2 then measure the average effect of having received the 300,000 UGX either on the bank account or cash on hands for treated women. The overall impact of receiving the money by treatment group for males is measured by $\beta_1 + \beta_4$ and by $\beta_2 + \beta_5$, respectively. In further regressions, we exchange the interaction term *male* with baseline (business) savings and educational achievement.

2.4.2 Estimation of Basic Experimental Treatment Effects

We focus our main interest on business profitability variables in line with de Mel et al. (2008) and test whether the cash grant had an impact on the firm's capital stock and deflated monthly profits. Tables 2.9 and 2.10 present basic estimation results. Columns (1) to (3) in each of the respective tables show estimation results in levels, columns (4) to (6) depict results in logs.

[Tables 2.9 and 2.10 about here.]

Capital Stock and Profits Opposed to much of the literature on unconditional cash transfers, we do not find a short term effect of the grant on either treatment arm with respect to capital stock and monthly profits. Also, the various time dimensions do not reveal much: all coefficients are not statistically significantly different from zero, hence we cannot detect a possible short-term effect fading out in the longer term or vice versa, a longer term effect materializing when higher returns can be reaped from

¹² Running the regressions with an untrimmed sample reinforces the need for truncation as coefficients are unreasonably high and driven by outliers.

initial investments. As the coefficients in Tables 2.9 and 2.10 switch signs across the specifications, especially for profits and in the cash treatment arm, it seems reasonable to conclude that it is not the small sample size which causes our non-findings.

Furthermore, we include the number of inventories and the amount of savings as outcome variables, because the data on the self-reported use of the transfer indicates significant differences among the treatment groups for these variables (see Figures 2.1 and 2.2 in Section 2.3). We measure savings as total savings the business owner has, and savings specifically targeted at the business. Higher inventories might induce higher sales in the long run, so we include this variable as our last outcome measure as well. Tables 2.11 to 2.14 present regression results with respect to these four additional outcome variables. As above, columns (1) to (3) depict results in levels, and columns (4) to (6) show results in logs.

[Tables 2.11, 2.12, 2.13 and 2.14 about here.]

Inventories The transfer has a significant positive effect on inventories in logs in the short term for the account treatment arm (see Table 2.11, column (5)), which persists also when we run the respective regressions with the untrimmed sample. Results for the cash treatment group are also positive in the short term, but not significant. This is encouragingly in line with our self-reported results regarding the use of the cash transfer. Oddly, inventories decrease in the cash treatment arm in the long term.

Sales As presumed, higher inventories translate into higher sales (see Table 2.12, column (1) and (2)). The effect of the treatment on account receivers is consistently positive in all our specifications and significant in the short-term and pooled leveled regressions. However, the size of the coefficients in the level regressions is unrealistically high suggesting it is possibly driven by outliers. Yet, the results stay positive albeit not significant in the log regressions which makes us confident to say that there is indeed a positive effect of the transfer for the account treatment arm with respect to sales.

(Business) Savings Results with regards to total and business savings are inconclusive between the level and log regressions as coefficients change signs (see Tables 2.13 and 2.14). There is no direct and robust treatment effect of the cash transfer with respect to (business) savings.

Summarized, these (non-)results fail to provide evidence for the cash transfer to have a direct and robust effect on micro and small enterprise profits and capital stock. However, we find modest positive treatment effects on the upstream outcome variables inventories and sales which indicate that the account treatment arm benefited more from the transfer than the cash treatment group. That is in line with our hypothesis

of the account treatment arm working as a soft commitment device to use the cash transfer for business related purposes.

2.4.3 Heterogeneity of Treatment Impacts

In what follows, we ask whether the provided cash grant had differing effects on various subgroups. In particular, the heterogeneity analysis examines whether varying amounts of baseline savings affect business outcome variables differently. Furthermore, we present results of interacting the treatment with gender and the baseline level of education in the Appendix (Tables A.3 to A.14).

Interaction with baseline business savings

The high economic size of coefficients for most basic estimations (especially for capital stock and inventories) suggests that entrepreneurs might have topped up the transfer with own money to make bigger investments. To check for this, we interact the treatment with the amount of business and total savings at baseline. The values are deflated and demeaned such that a one unit change represents a deviation from mean baseline (business) savings.

[Tables 2.15 to 2.22 about here.]

For capital stock as dependent variable, the interaction effect between higher baseline business savings and the two treatment arms is negative throughout and significant for the pooled and short term specifications (Table 2.15). The economic size of the interaction is very small. This finding indicates that the capital stock of treated entrepreneurs with lower baseline business savings increases relatively more compared to those with higher baseline business savings. This negates the thought of entrepreneurs using their savings to top up the cash transfer and increase the businesses' capital stock.

However, having higher baseline business savings at one's disposal results in significantly higher monthly profits for the account treatment arm in the short term in the levels regression and for the cash treatment group in the long-term in the log regression (Table 2.16). The sign of coefficients stays stable across all specifications only for the account treatment group. We also find that entrepreneurs in the account group with higher baseline business savings accumulate more inventories than those with lower baseline business savings in the short term in the levels regression (Table 2.17). In the long term, there are no significant interaction effects in neither group.

With regards to sales, entrepreneurs in both treatment arms who have higher business savings sell more than those with lower savings (Table 2.18). The economic size of the effect is rather small, though.

There are two remarks to take away from the interaction of the treatment with baseline business savings: One, there is no clear differential treatment effect with regards to the treatment arms. The soft-commitment device does not work in this kind of heterogeneity analysis. Two, while higher business savings do not positively affect the capital stock of treated entrepreneurs, profits, inventories and sales are modestly and positively affected from having more savings at baseline.

Interaction with baseline total savings

As is the case with business savings, we also find a negative interaction effect for those business owners with higher savings at baseline for the account treatment arm with respect to capital stock (Table 2.19). It is significant, but economically small in size. The interaction term with respect to profits is similarly small and indicates a negative treatment effect for those with higher savings at baseline compared to those with lower savings at baseline, too.

Regarding inventories as dependent variable, interaction effects across both treatment arms are rather inconclusive. While entrepreneurs with higher savings in the account group benefit from the transfer and accumulate more inventories in the short run directly after the intervention took place, inventories for business owners in the cash group are positively affected in the short term, but negatively in the long term.

Last, the most conclusive results exist for the interaction of savings with treatment when we consider monthly sales as outcome variable (Table 2.18). For the account treatment arm only, the interaction is significant and positive indicating that entrepreneurs with higher savings at baseline are able to sell more due to the cash transfer compared to those with lower savings.

Similarly to business savings, capital stock and profits shrink the more savings entrepreneurs accumulate due to the cash transfer while sales, however, increase with the amount of savings entrepreneurs own. These findings do not clearly reveal whether savings were used to top up the cash transfer and invest. However, at least for sales as outcome variable, there is a positive interaction effect for the account treatment arm, which suggests modest positive evidence for the soft commitment device being at work.

In concluding the heterogeneity analysis, we find some evidence that the more savings entrepreneurs (especially in the account treatment group) have at baseline, the more they sell due to the transfer. If treated entrepreneurs have higher business savings in particular, monthly profits and inventories increase due to the cash transfer as well. Also, as we show in the Appendix, higher educated entrepreneurs in any treatment group save and sell relatively more compared to lower educated entrepreneurs. Last, we cannot confirm that men and women benefit differently from the transfer.

2.5 Robustness Tests and Discussion

In this section, we provide robustness results and discuss limitations of our study. We apply Lee-Bounds to deal with differential attrition in our sample (Lee, 2009). To construct these bounds, we trim the distribution of our outcome variables (i.e. here only monthly profits and capital stock) for the intended treatment group by the difference in attrition rates between treatment and control groups as a proportion of the retention rate of the group assigned to treatment. We document 675 firm-period observations out of 875 possible observations had there not been attrition. The attrition rate for the treatment and control groups are 14.59 percent and 30.63 percent, respectively. Thus, the difference between the two groups is 16.05 percentage points. Dividing this by the retention rate of the intended treatment group yields 18.79 percent. We then trim the treatment firm period observations by this proportion at the lower or the upper tail of the distribution to obtain lower and upper bounds of our treatment estimates.

We expect the treatment effect to increase using the upper bounds as this is based on the assumption that the least profitable entrepreneurs from the control group left the sample. Indeed, results show that the impact of the transfer increases numerically for all tested outcome variables, yet remains insignificant for both treatment groups over all time horizons (columns (1)-(3) in the following Tables).

[Tables 2.23 and 2.24 about here.]

The lower bound dependent variables assume that only the most profitable entrepreneurs in the control group left the sample. These variables trim the highest treatment observations in the sample. We expect the effect to diminish in our estimations, which we also find w.r.t. our outcome variables. The treatment effect stays insignificant over all specifications except one: In the pooled regression, the transfer seems to reduce capital stock significantly for both treatment groups (Table 2.24, column (4)). This result is somewhat troublesome as we know that there is a correlation between more affluent control entrepreneurs and attrition. On the other hand, the effect is only marginally significant and does not occur in the short term or long term specification on its own. Also, there is no economically meaningful explanation why the transfer should have caused a negative effect on capital stock. All in all, the Lee-bound results confirm that our overall null results hold and that it is not *solely* the most profitable control business owners who leave the sample.

As further robustness checks we run quantile and fixed effects regressions. Quantile regression offers a useful robustness check in our study because it is more robust against

outliers in our outcome measures. Given that randomization was not successful with respect to every variable, using firm fixed effects to further control for time-invariant differences between the control and treatment groups is sensible. Quantile regressions at the median as well as fixed effects regressions confirm our OLS (non)-findings by and large.¹³

We are aware that our study suffers from data limitations, notably the small sample size and noisy elicitation of business profits and other performance variables. To remedy the first caveat, we present pooled results for all follow-up waves throughout the analysis to increase statistical power. Nevertheless, we cannot fully rule out that the non-results are due to power issues. Regarding the elicitation of business performance variables, we face challenges common to this kind of survey work (de Mel et al., 2008, 2012): One, business owners may unintentionally misreport profits and business assets because they just do not know their exact value. As 76.8 percent of entrepreneurs report keeping at least basic records, we are confident to state that they have a good sense of how their businesses are doing, nevertheless. Further, we follow the best practice to directly elicit business profits from the entrepreneur as de Mel et al. (2009b) show that this yields more precise information than eliciting profits from revenue and cost data. Two, treated entrepreneurs may strategically misreport business performance variables because they expect more money to flow into their businesses. The cash transfer was, however, provided without labeling its use for business purposes, so we do not think this bias occurs in our data.

Another concern why we do not find robust direct effects of the treatment on the treated business owners is that many of them state to use the money for additional or new businesses of theirs. It is out of scope of our survey to follow up on these businesses. We cannot rule out the existence of a direct effect of the intervention on total business performance over all enterprises the business owner runs.

2.6 Conclusion

Cash transfer programs have seen a rise in popularity followed by expansion of these programs nearly worldwide. It is crucial to know who actually benefits from receiving them and which mode of payment may make the payments more effective. This paper studies these questions in the setting of micro and small businesses. We evaluate the impact of an unconditional one-time cash transfer on business profitability. The

¹³ The results are available upon request.

transfer amounted to 300,000 UGX and was handed out differently depending on the treatment arm: one group received the money in cash while the money was deposited in respondents' bank accounts for the other treatment arm. We hypothesize, one, that the cash grant helps to lift binding capital constraints and yields a positive impact on the treated businesses. Two, the transfer has a differential impact on business owners depending on their treatment status, because the deposit on the account acts as a soft-commitment device enabling the entrepreneur to spend the money on productive purposes rather than just consuming it away. So, is the provided transfer a good way to remove credit constraints for micro and small entrepreneurs and did the account treatment arm react differently to receiving the money than the cash treatment arm? Our analyses suggest that the transfer did not contribute significantly to alleviate credit constraints which could consequently have spurred firm growth. In line with Fiala (2018), we do not find direct effects of the grant on monthly profits, capital stock or total savings. Yet, in the short-term, the amount of inventories and sales increases significantly for the account treatment group. It therefore seems that more "upstream" business variables are affected by the transfer in the hypothesized way, but the effect does not trickle down to outcomes that are decisive for actual firm growth.

Our heterogeneity analysis reveals that men and women are not affected differently by the grant, neither in the cash nor in the account group. We further find that the more savings entrepreneurs in either treatment group have at baseline, the higher are their sales due to the cash transfer. If savings are especially targeted for usage in their businesses, having higher baseline business savings also affects the amount of monthly profits, inventories, and sales positively due to the cash transfer. Last, entrepreneurs with high education end up saving more of the transfer compared to those with lower education. Regarding the heterogeneity with respect to higher savings and high education at baseline, the results suggest that the transfer may rather help the anyways better off entrepreneurs and may not change the situation for businesses which are most credit constrained and/or self-employed out of necessity. Thus, the transfer possibly benefits those that might have access to alternative sources of funding anyways. Studies dealing with "constrained gazelles" (Grimm et al., 2012; McKenzie, 2017) find similar results. Further research should tackle the question whether the "gazelle"-mechanism is at work by examining a variety of business characteristics that determine entrepreneurial success - especially since we do not find this heterogeneity for the interaction with savings and capital stock as outcome variable. Here, results indicate the inverse - the lower the savings, the higher the impact of the transfer on capital stock will be compared to those with higher savings.

This paper adds to the literature on adequately targeting and designing cash transfer interventions and at the same time leaves room for further research. For example, what are the effects of digitizing payments at a larger scale going beyond the small scope of the grant in our experimental setting? This question is likely to gain relevance for policy makers as major emerging economies such as India are shifting to cashless banking (Somville and Vandewalle, 2018). Higher powered impact evaluations are also needed to confirm the different beneficial effect of cash transfers depending on business owner's ability. Lastly, while it is important to know who to target, research should not forget to examine why capital constraints seem to remain for necessity entrepreneurs and what ways there are to remedy them.

Tables and Figures

Table 2.1: Industry Sampling Categories

Industry	Frequency	Percent
Hair dressing and beauty	19	10.86
Manufacture of printing/ paper products and related services	17	9.71
Manufacture of textile/ wearing apparel (tailors) and related services	16	9.14
Manufacturing (remaining sectors)	42	24.00
Other	11	6.29
Retail and wholesale (remaining sectors)	25	14.29
Retail clothing, footwear and leather (incl. 2nd hand)	27	15.43
Retail electric, phones, household appliances and related services	18	10.29
<i>N</i>	175	100.00

Source: Own calculations

Table 2.2: Assigned Treatment for Baseline and Follow-up Waves

	Oct.12	Apr.13	Oct.13	Oct.14	Oct.15	Oct.16	Oct.17	Total
Account								
Treatment	47	48	48	46	43	38	34	304
Cash								
Treatment	48	48	47	45	38	36	35	297
Control Group	79	79	74	59	50	49	42	432
<i>N</i>	174	175	169	150	131	123	111	1,033

Source: Own calculations

Table 2.3: Analysis of Attrition - Intended Treatment

	not surveyed in Oct. 13	not surveyed in Oct. 14	not surveyed in Oct. 15	not surveyed in Oct. 16	not surveyed in Oct. 17	not surveyed in at least one wave
Account Treatment	-0.063** (0.028)	-0.148*** (0.054)	-0.104* (0.062)	0.096* (0.053)	-0.038 (0.071)	-0.101 (0.079)
Cash Treatment	-0.042 (0.035)	-0.147*** (0.055)	-0.014 (0.074)	0.033 (0.042)	-0.115** (0.058)	-0.147** (0.065)
Constant	0.063** (0.028)	0.189*** (0.046)	0.169*** (0.049)	0.020 (0.020)	0.143*** (0.051)	0.322*** (0.058)
<i>N</i>	175	169	150	131	123	1033

Note: The dependent variable turns one if the firm was not tracked in the specified period, zero otherwise. Regressions are OLS models with robust standard errors clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively.

Table 2.4: Baseline Balance - Account vs. Control Group

	Obs.	Account Mean	Control Mean	Difference	p-value
Central Division	253	0.56	0.48	0.077	0.238
Kawempe Division	253	0.15	0.09	0.059	0.150
Makindye Division	253	0.02	0.05	-0.030	0.244
Nakawa Division	253	0.13	0.22	-0.089*	0.077
Rubaga Division	253	0.15	0.16	-0.017	0.718
Manufacturing	253	0.46	0.44	0.020	0.757
Trade	253	0.28	0.47	-0.180***	0.004
Services	253	0.15	0.06	0.084**	0.027
Other	253	0.11	0.03	0.080***	0.007
Male	253	0.63	0.58	0.049	0.440
Married*	127	0.62	0.65	-0.033	0.712
No primary Education	253	0.08	0.11	-0.030	0.453
Completed Primary Edu.	253	0.23	0.27	-0.041	0.476
Completed O-Level	253	0.34	0.21	0.130**	0.024
Completed A-Level	253	0.17	0.15	0.017	0.728
Completed University	253	0.15	0.19	-0.043	0.390
Owner Speaks English	246	0.76	0.71	0.050	0.396
1=Impatient	251	0.87	0.85	0.019	0.678
1=Present Biased*	127	0.52	0.65	-0.120	0.167
Household Size	253	4.76	4.63	0.120	0.681
Age	250	34.30	33.70	0.630	0.543
Household Wealth*	126	0.07	0.06	-0.001	0.907
Own-Account Worker	253	0.37	0.39	-0.024	0.705
Number of Employees	253	1.76	1.15	0.610**	0.020
Owner Labor Hours	241	302.1	321.7	-19.700**	0.040
Employee Labor Hours	253	309.1	266.8	42.300	0.375
Firm Age*	124	6.87	6.87	0.002	0.998
1=Formal Business*	126	0.28	0.22	0.061	0.438
1=Credit Constrained*	144	0.82	0.78	0.037	0.589
1=Demand Formal Loan*	126	0.79	0.77	0.015	0.845
Profits	249	1459.5	1156.0	303.500	0.576
Sales	249	6953.2	6373.4	579.700	0.716
Costs	253	5626.2	6431.4	-805.200	0.669
Capital Stock	251	1916.7	1995.8	-79.000	0.851
Value Added	249	1690.6	-114.0	1804.600*	0.080
Start-up Capital	248	2300.9	4824.6	-2523.800**	0.041
Inventories	236	7185.3	9311.7	-2126.400	0.392
Business Savings	240	1267.4	1207.5	60.000	0.908
Total Savings*	127	1493.2	1795.2	-302.000	0.635
<i>N</i>	253				

Note: Characteristics denoted with an asterisk use values from one of the baseline surveys only, all other measures depict the average of both baselines (October 2012 and April 2013). P-values for tests of equality between account and control group. *, **, and *** on p-values denote significant differences at the 10, 5, and 1 percent levels, respectively. All values are given in 1000 UGX.

Table 2.5: Baseline Balance - Cash vs. Control Group

	Obs.	Cash Mean	Control Mean	Difference	p-value
Central Division	254	0.31	0.48	-0.170***	0.008
Kawempe Division	254	0.10	0.09	0.016	0.682
Makindye Division	254	0.15	0.05	0.095***	0.009
Nakawa Division	254	0.29	0.22	0.076	0.170
Rubaga Division	254	0.15	0.16	-0.019	0.693
Manufacturing	254	0.38	0.44	-0.068	0.288
Trade	254	0.40	0.47	-0.073	0.261
Services	254	0.15	0.06	0.083**	0.029
Other	254	0.08	0.03	0.058**	0.035
Male	254	0.63	0.58	0.043	0.503
Married*	129	0.76	0.65	0.110	0.213
No primary Education	254	0.06	0.11	-0.051	0.176
Completed Primary Edu.	254	0.35	0.27	0.082	0.169
Completed O-Level	254	0.19	0.21	-0.021	0.682
Completed A-Level	254	0.25	0.15	0.098*	0.053
Completed University	254	0.13	0.19	-0.065	0.179
Owner Speaks English	252	0.65	0.71	-0.059	0.328
1=Impatient	252	0.80	0.85	-0.054	0.271
1=Present Biased*	127	0.60	0.65	-0.041	0.643
Household Size	254	5.27	4.63	0.640**	0.021
Age	252	36.80	33.70	3.050***	0.005
Household Wealth*	127	0.07	0.07	0.004	0.759
Own-Account Worker	254	0.35	0.39	-0.038	0.544
Number of Employees	254	1.36	1.15	0.210	0.226
Owner Labor Hours	246	306.90	321.70	-14.900	0.118
Employee Labor Hours	254	319.80	266.80	53.000	0.282
Firm Age*	124	6.98	6.87	0.110	0.927
1=Formal Business*	126	0.23	0.22	0.019	0.807
1=Credit Constrained*	147	0.86	0.78	0.080	0.220
1=Demand Formal Loan*	127	0.85	0.77	0.082	0.263
Profits	247	1217.2	1156.0	61.200	0.878
Sales	249	9264.7	6373.4	2891.300	0.199
Costs	254	7244.2	6431.4	812.800	0.713
Capital Stock	250	2176.1	1995.8	180.300	0.671
Value Added	249	1912.7	-114.0	2026.800*	0.078
Start-up Capital	252	2228.6	4824.6	-2596.100**	0.036
Inventories	235	22239.4	9311.7	12927.700	0.115
Business Savings	238	761.9	1207.5	-445.500	0.326
Total Savings*	127	3273.1	1795.2	1477.900	0.217
<i>N</i>	254				

Note: Characteristics denoted with an asterisk use values from one of the baseline surveys only, all other measures depict the average of both baselines (October 2012 and April 2013). P-values for tests of equality between account and control group. *, **, and *** on p-values denote significant differences at the 10, 5, and 1 percent levels, respectively. All values are given in 1000 UGX.

Table 2.6: Baseline Balance - Account vs. Cash Group

	Obs.	Cash Mean	Account Mean	Difference	p-value
Central Division	191	0.31	0.56	-0.250***	0.001
Kawempe Division	191	0.10	0.15	-0.043	0.370
Makindye Division	191	0.15	0.02	0.120***	0.002
Nakawa Division	191	0.29	0.13	0.170***	0.005
Rubaga Division	191	0.15	0.15	-0.002	0.976
Manufacturing	191	0.38	0.46	-0.088	0.219
Trade	191	0.40	0.28	0.11	0.105
Services	191	0.15	0.15	-0.002	0.976
Other	191	0.08	0.11	-0.022	0.606
Male	191	0.63	0.63	-0.007	0.926
Married*	96	0.76	0.62	0.140	0.148
No primary Education	191	0.06	0.08	-0.022	0.567
Completed Primary Edu.	191	0.35	0.23	0.120*	0.063
Completed O-Level	191	0.19	0.34	-0.150**	0.019
Completed A-Level	191	0.25	0.17	0.082	0.168
Completed University	191	0.13	0.15	-0.022	0.654
Owner Speaks English	186	0.65	0.76	-0.110	0.104
1=Impatient	189	0.80	0.87	-0.072	0.181
1=Present Biased*	96	0.60	0.52	0.083	0.416
Household Size	191	5.27	4.76	0.510	0.160
Age	190	36.80	34.30	2.420**	0.048
Household Wealth*	95	0.07	0.06	0.006	0.682
Own-Account Worker	191	0.35	0.37	-0.014	0.839
Number of Employees	191	1.36	1.76	-0.390	0.232
Owner Labor Hours	183	306.90	302.10	4.800	0.648
Employee Labor Hours	191	319.80	309.10	10.700	0.858
Firm Age*	94	6.98	6.87	0.110	0.938
1=Formal Business*	94	0.23	0.28	-0.043	0.640
1=Credit Constrained	115	0.86	0.82	0.043	0.530
1=Demand Formal Loan*	95	0.85	0.79	0.067	0.400
Profits	188	1217.2	1459.5	-242.3	0.689
Sales	188	9264.7	6953.2	2311.6	0.398
Costs	191	7244.2	5626.2	1618.0	0.443
Capital Stock	185	2176.1	1916.7	259.3	0.531
Value Added	188	1912.7	1690.6	222.1	0.863
Start-up Capital	188	2228.6	2300.9	-72.3	0.885
Inventories	175	22239.4	7185.3	15054.1	0.149
Business Savings	180	761.9	1267.4	-505.5	0.247
Total Savings*	96	3273.1	1493.2	1779.9	0.207
<i>N</i>	191				

Note: Characteristics denoted with an asterisk use values from one of the baseline surveys only, all other measures depict the average of both baselines (October 2012 and April 2013). P-values for tests of equality between account and control group. *, **, and *** on p-values denote significant differences at the 10, 5, and 1 percent levels, respectively. All values are given in 1000 UGX.

Table 2.7: Use of Cash Transfer - Account vs. Cash Group

	Obs.	Cash Treatment	Account Treatment	Difference	p-value
Business Inventory/Equipment	92	212045.5	167916.7	44128.8*	0.0935
Inv./Equip. for New Business	92	18181.8	15833.3	2348.5	0.865
Inv./Equip. for Add. Business	92	13636.4	14166.7	-530.3	0.966
Hired Labour for Business	92	4545.5	0	4545.5	0.299
Money Saved	92	9090.9	42708.3	-33617.4**	0.0353
Household Expenditures	92	9772.7	30000	-20227.3	0.107
Other	92	6818.2	10208.3	-3390.2	0.736
<i>N</i>	92				

Note: All values depict average shares of the 300,000 UGX transfer. P-values for tests of equality of means between account and cash treatment group. *, **, and *** on the difference denote significant differences at the 10, 5, and 1 percent levels, respectively.

Table 2.8: Use of Cash Transfer - Men vs. Women

	Obs.	Women	Men	Difference	p-value
Business Inventory/Equipment	92	221714.3	168947.4	52766.9*	0.0506
Inv./Equip. for New Business	92	5714.3	23859.6	-18145.4	0.200
Inv./Equip. for Add. Business	92	0	22456.1	-22456.1*	0.0769
Hired Labour for Business	92	0	3508.8	-3508.8	0.436
Money Saved	92	28571.4	25438.6	3132.8	0.851
Household Expenditures	92	20571.4	20175.4	396.0	0.976
Other	92	0	13859.6	-13859.6	0.178
<i>N</i>	92				

Note: All values depict average shares of the 300,000 UGX transfer. P-values for tests of equality of means between males and females. *, **, and *** on the difference denote significant differences at the 10, 5, and 1 percent levels, respectively.

Table 2.9: Impact of Cash Transfer on Capital Stock

	Capital Stock			Log Capital Stock		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	598.652 (544.166)	702.046 (611.179)	554.273 (714.803)	0.191 (0.224)	0.032 (0.249)	0.316 (0.264)
Cash Treatment	100.036 (543.069)	-40.238 (400.226)	109.371 (741.714)	-0.134 (0.179)	-0.207 (0.158)	-0.109 (0.245)
Observations	851	547	609	851	547	609
Control Mean	1944.87	1845.17	2011.23	6.53	6.57	6.49
Equality of Treatments	0.41	0.22	0.57	0.14	0.32	0.09

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time. P-values for equality of treatments are stated in the last row.

Table 2.10: Impact of Cash Transfer on Monthly Profits

	Monthly Profits			Log Monthly Profits		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	86.320 (269.217)	333.783 (425.634)	7.376 (411.767)	0.010 (0.149)	-0.055 (0.183)	0.067 (0.216)
Cash Treatment	21.524 (331.120)	-80.637 (544.961)	0.167 (433.246)	-0.096 (0.166)	-0.153 (0.193)	-0.115 (0.239)
Observations	828	534	590	765	501	545
Control Mean	810.91	960.45	888.66	5.98	6.08	5.97
Equality of Treatments	0.80	0.26	0.99	0.52	0.62	0.42

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time. P-values for equality of treatments are stated in the last row.

Table 2.11: Impact of Cash Transfer on Inventories

	Inventories			Log Inventories		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	1719.317 (1749.614)	3152.738 (2019.263)	740.353 (2728.343)	0.210 (0.207)	0.480** (0.214)	-0.043 (0.288)
Cash Treatment	-3662.463 (3773.704)	1073.090 (2050.764)	-7585.315 (6763.685)	-0.226 (0.237)	0.219 (0.222)	-0.647** (0.320)
Observations	710	477	500	678	457	476
Control Mean	5945.82	6089.42	6825.35	6.91	6.97	7.02
Equality of Treatments	0.18	0.24	0.25	0.05	0.17	0.07

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time. P-values for equality of treatments are stated in the last row.

Table 2.12: Impact of Cash Transfer on Total Sales

	Total Sales			Log Total Sales		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	3631.872* (2127.288)	3033.344* (1709.910)	4323.376 (3359.568)	0.231 (0.155)	0.233 (0.178)	0.264 (0.206)
Cash Treatment	-311.076 (2003.229)	-1462.372 (1574.047)	-268.358 (2991.575)	-0.106 (0.168)	-0.147 (0.177)	-0.182 (0.227)
Observations	833	537	594	832	537	593
Control Mean	5418.41	5695.97	5591.17	7.58	7.65	7.51
Equality of Treatments	0.14	0.00	0.29	0.06	0.03	0.07

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time. P-values for equality of treatments are stated in the last row.

Table 2.13: Impact of Cash Transfer on Total Savings

	Total Savings			Log Total Savings		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	560.642 (361.279)	610.106 (510.892)	780.552* (469.501)	-0.151 (0.195)	-0.079 (0.220)	-0.065 (0.259)
Cash Treatment	213.846 (568.771)	27.267 (576.763)	555.349 (981.067)	-0.205 (0.229)	-0.157 (0.263)	-0.193 (0.303)
Observations	701	391	453	601	350	379
Control Mean	1284.40	1444.30	1208.23	6.29	6.39	6.21
Equality of Treatments	0.52	0.30	0.79	0.81	0.77	0.66

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time. P-values for equality of treatments are stated in the last row.

Table 2.14: Impact of Cash Transfer on Total Business Savings

	Total Business Savings			Log Total Business Savings		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	22.332 (186.403)	-145.770 (252.088)	197.369 (245.671)	-0.154 (0.265)	-0.738** (0.317)	0.567 (0.387)
Cash Treatment	-66.704 (210.077)	-49.126 (223.917)	-86.773 (303.922)	0.049 (0.231)	-0.266 (0.283)	0.459 (0.358)
Observations	809	521	574	352	252	247
Control Mean	879.00	1015.66	932.70	6.22	6.31	6.19
Equality of Treatments	0.69	0.67	0.36	0.34	0.07	0.69

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time. P-values for equality of treatments are stated in the last row.

Table 2.15: Het. Treatment Effects w.r.t. Bus. Savings - Capital Stock

	Capital Stock			Log Capital Stock		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	204.657 (588.765)	511.005 (581.939)	-213.727 (887.747)	0.058 (0.233)	-0.034 (0.261)	0.094 (0.288)
Business Savings	-0.041 (0.078)	-0.019 (0.042)	-0.025 (0.080)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Account \times Bus. Savings	-0.655* (0.351)	-0.624** (0.241)	-1.075 (0.708)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Cash Treatment	-1122.331** (542.010)	-1116.225* (576.173)	-1376.441* (786.021)	-0.261 (0.181)	-0.381** (0.179)	-0.285 (0.252)
Cash \times Bus. Savings	-1.189** (0.497)	-1.598** (0.735)	-1.415* (0.748)	-0.000 (0.000)	-0.000** (0.000)	-0.000 (0.000)
Constant	-1208.789 (859.809)	-686.218 (562.709)	-1075.216 (860.904)	1.494*** (0.451)	0.642** (0.324)	1.476*** (0.429)
Observations	768	493	549	768	493	549
R squared	0.43	0.70	0.41	0.64	0.76	0.67

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Monthly Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table 2.16: Het. Treatment Effects w.r.t. Bus. Savings - Profits

	Monthly Profits			Log Monthly Profits		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	164.388 (299.347)	568.626 (472.636)	51.000 (541.253)	0.012 (0.151)	-0.010 (0.192)	0.103 (0.239)
Business Savings	0.010 (0.070)	-0.084 (0.074)	-0.014 (0.061)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Account \times Bus. Savings	0.175 (0.183)	0.356** (0.168)	0.085 (0.429)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Cash Treatment	342.421 (494.438)	-322.328 (788.657)	428.214 (661.508)	0.097 (0.224)	-0.151 (0.282)	0.135 (0.311)
Cash \times Bus. Savings	0.782 (0.507)	-0.418 (0.610)	1.115* (0.665)	0.000 (0.000)	-0.000 (0.000)	0.001* (0.000)
Constant	1175.002*** (336.487)	391.654 (392.749)	1165.088*** (368.260)	3.089*** (0.413)	1.678*** (0.273)	2.504*** (0.464)
Observations	766	493	546	711	464	507
R squared	0.38	0.59	0.48	0.38	0.50	0.46

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table 2.17: Het. Treatment Effects w.r.t. Bus. Savings - Inventories

	Inventories			Log Inventories		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	3802.823* (1996.165)	4337.515** (1805.668)	-71.291 (3792.388)	0.283 (0.219)	0.516** (0.220)	0.011 (0.432)
Business Savings	1.389 (0.892)	1.145 (0.908)	1.112 (0.827)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Account \times Bus. Savings	4.463*** (1.409)	6.350*** (1.558)	-3.469 (4.580)	0.000 (0.000)	0.000 (0.000)	0.000 (0.001)
Cash Treatment	809.360 (1949.254)	1581.202 (2849.444)	-3752.149 (3280.006)	-0.258 (0.281)	0.338 (0.241)	-1.012* (0.529)
Cash \times Bus. Savings	3.346 (2.253)	4.306 (3.631)	-4.228 (4.263)	-0.000 (0.000)	0.000 (0.000)	-0.001 (0.001)
Constant	3460.884 (3048.690)	2323.190 (2006.749)	3314.202 (3806.025)	1.656*** (0.462)	1.279*** (0.312)	1.273** (0.521)
Observations	646	432	454	618	415	433
R squared	0.54	0.71	0.56	0.68	0.77	0.67

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table 2.18: Het. Treatment Effects w.r.t. Bus. Savings - Sales

	Monthly Sales			Log Monthly Sales		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	3432.281* (1781.555)	3642.305** (1647.568)	-325.061 (4224.477)	0.287* (0.147)	0.310* (0.183)	0.283 (0.277)
Business Savings	0.800** (0.380)	0.133 (0.246)	0.651* (0.377)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Account \times Bus. Savings	2.474 (2.402)	4.499** (1.786)	-7.080 (10.508)	0.000*** (0.000)	0.000*** (0.000)	0.000 (0.000)
Cash Treatment	5861.896** (2575.368)	-749.374 (1904.364)	8171.299** (3312.646)	0.283 (0.214)	-0.044 (0.221)	0.408 (0.309)
Cash \times Bus. Savings	13.130*** (3.707)	2.684 (2.314)	17.739*** (4.300)	0.001*** (0.000)	0.000 (0.000)	0.001*** (0.000)
Constant	11873.43** (4643.515)	4381.01*** (1539.676)	12743.10** (5844.409)	3.244*** (0.456)	2.204*** (0.342)	2.435*** (0.483)
Observations	761	490	542	760	490	541
R squared	0.35	0.53	0.40	0.55	0.64	0.59

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Shilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table 2.19: Het. Treatment Effects w.r.t. Any Savings - Capital Stock

	Capital Stock			Log Capital Stock		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	337.741 (513.104)	361.081 (586.663)	363.771 (667.675)	0.088 (0.212)	-0.111 (0.269)	0.256 (0.247)
Any Savings	-0.029 (0.022)	-0.011 (0.010)	-0.029 (0.023)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Account \times Any Sav.	-0.343*** (0.123)	-0.388** (0.167)	-0.313* (0.174)	-0.000* (0.000)	-0.000 (0.000)	-0.000 (0.000)
Cash Treatment	78.722 (545.666)	-53.897 (393.083)	103.659 (749.913)	-0.135 (0.176)	-0.235 (0.152)	-0.115 (0.243)
Cash \times Any Sav.	-0.148 (0.100)	-0.069 (0.054)	-0.173 (0.123)	-0.000 (0.000)	-0.000** (0.000)	-0.000 (0.000)
Observations	851	547	609	851	547	609
R squared	0.34	0.62	0.34	0.63	0.78	0.65

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table 2.20: Het. Treatment Effects w.r.t. Any Savings - Profits

	Monthly Profits			Log Monthly Profits		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	38.408 (286.164)	147.355 (425.012)	38.617 (421.225)	0.006 (0.163)	-0.120 (0.188)	0.121 (0.239)
Any Savings	0.009 (0.030)	-0.013 (0.031)	-0.000 (0.029)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Account \times Any Sav.	-0.043 (0.057)	-0.233** (0.101)	0.058 (0.080)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Cash Treatment	-4.333 (331.440)	-199.185 (545.656)	-5.426 (432.228)	-0.095 (0.169)	-0.236 (0.198)	-0.079 (0.243)
Cash \times Any Sav.	-0.023 (0.056)	-0.172 (0.133)	0.005 (0.050)	-0.000 (0.000)	-0.000* (0.000)	0.000 (0.000)
Constant	1387.317*** (407.283)	727.019 (449.616)	1418.396*** (440.035)	2.932*** (0.356)	1.542*** (0.247)	2.456*** (0.403)
Observations	828	534	590	765	501	545
R squared	0.39	0.60	0.49	0.42	0.54	0.48

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Savings are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table 2.21: Het. Treatment Effects w.r.t. Any Savings - Inventories

	Inventories			Log Inventories		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	2431.981 (3064.108)	6108.474*** (1901.590)	-607.463 (5281.838)	0.275 (0.214)	0.605*** (0.210)	-0.003 (0.309)
Any Savings	-0.216 (0.371)	-0.878 (0.586)	-0.323 (0.400)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Account \times Any Sav.	1.573 (1.947)	3.998*** (0.587)	-0.658 (3.538)	0.000 (0.000)	0.000*** (0.000)	0.000 (0.000)
Cash Treatment	-1838.197 (2148.135)	1327.950 (1683.438)	-4205.552* (2504.456)	-0.186 (0.236)	0.221 (0.216)	-0.573* (0.316)
Cash \times Any Sav.	-2.742** (1.271)	1.367*** (0.521)	-4.371*** (1.238)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)
Constant	5747.518* (3353.684)	2473.916 (2083.185)	5378.743 (4106.241)	1.817*** (0.478)	1.136*** (0.326)	1.664*** (0.534)
Observations	710	477	500	678	457	476
R squared	0.82	0.93	0.85	0.68	0.76	0.68

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table 2.22: Het. Treatment Effects w.r.t. Any Savings - Sales

	Monthly Sales			Log Monthly Sales		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	7018.26*** (2667.66)	4683.18** (1976.11)	9222.736* (4774.735)	0.320* (0.163)	0.267 (0.177)	0.408* (0.238)
Any Savings	-0.24 (0.43)	-0.50 (0.48)	-0.276 (0.446)	0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)
Account \times Any Sav.	4.92*** (1.43)	3.08** (1.25)	6.749** (2.671)	0.000** (0.000)	0.000 (0.000)	0.000* (0.000)
Cash Treatment	-391.97 (1914.99)	-2304.81 (1518.18)	218.470 (2925.596)	-0.117 (0.167)	-0.231 (0.163)	-0.169 (0.228)
Cash \times Any Sav.	-0.22 (0.34)	-0.60 (0.66)	0.055 (0.223)	0.000 (0.000)	-0.000* (0.000)	0.000** (0.000)
Constant	6257.46* (3302.56)	2567.65* (1448.09)	7103.374* (4232.830)	3.155*** (0.433)	1.877*** (0.298)	2.658*** (0.466)
Observations	833	537	594	832	537	593
R squared	0.46	0.65	0.48	0.57	0.66	0.59

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table 2.23: Impact of Cash Transfer on Monthly Profits - Lee Bounds

	Upper Bound Monthly Profits			Lower Bound Monthly Profits		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	175.121 (278.821)	501.472 (422.613)	97.429 (473.651)	99.281 (281.267)	330.732 (414.395)	98.008 (366.780)
Cash Treatment	154.690 (373.235)	32.051 (618.689)	150.803 (490.752)	-98.475 (378.255)	-160.841 (562.787)	-117.035 (514.367)
Observations	771	512	556	789	523	563
Control Mean	810.91	960.45	888.66	810.91	960.45	888.66
Equality of Treatments	0.95	0.29	0.92	0.45	0.21	0.56

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time and applies Lee bounds to the dependent variables. P-values for equality of treatments are stated in the last row.

Table 2.24: Impact of Cash Transfer on Capital Stock - Lee Bounds

	Upper Bound Capital Stock			Lower Bound Capital Stock		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	917.003 (619.260)	1018.432 (732.553)	901.300 (824.380)	-654.877* (338.036)	-446.921 (327.737)	-736.595 (481.101)
Cash Treatment	301.553 (594.782)	100.256 (458.044)	309.358 (779.782)	-607.105* (340.162)	-422.078 (319.045)	-776.088 (503.283)
Observations	792	517	580	797	526	576
Control Mean	1944.87	1845.17	2011.23	1944.87	1845.17	2011.23
Equality of Treatments	0.40	0.22	0.53	0.84	0.92	0.90

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time and applies Lee bounds to the dependent variables. P-values for equality of treatments are stated in the last row.

Table 2.25: Impact of Cash Transfer on Total Savings - Lee Bounds

	Upper Bound Total Savings			Lower Bound Total Savings		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	861.329* (446.368)	753.580 (564.780)	1223.713* (623.221)	-262.286 (267.466)	-144.569 (409.482)	-82.907 (346.998)
Cash Treatment	772.815 (696.516)	283.018 (564.762)	1456.116 (1376.355)	-596.940 (407.948)	-315.167 (575.064)	-737.882 (573.008)
Observations	636	377	402	653	372	424
Control Mean	1284.40	1444.30	1208.23	1284.40	1444.30	1208.23
Equality of Treatments	0.88	0.39	0.83	0.39	0.72	0.22

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time and applies Lee bounds to the dependent variables.. P-values for equality of treatments are stated in the last row.

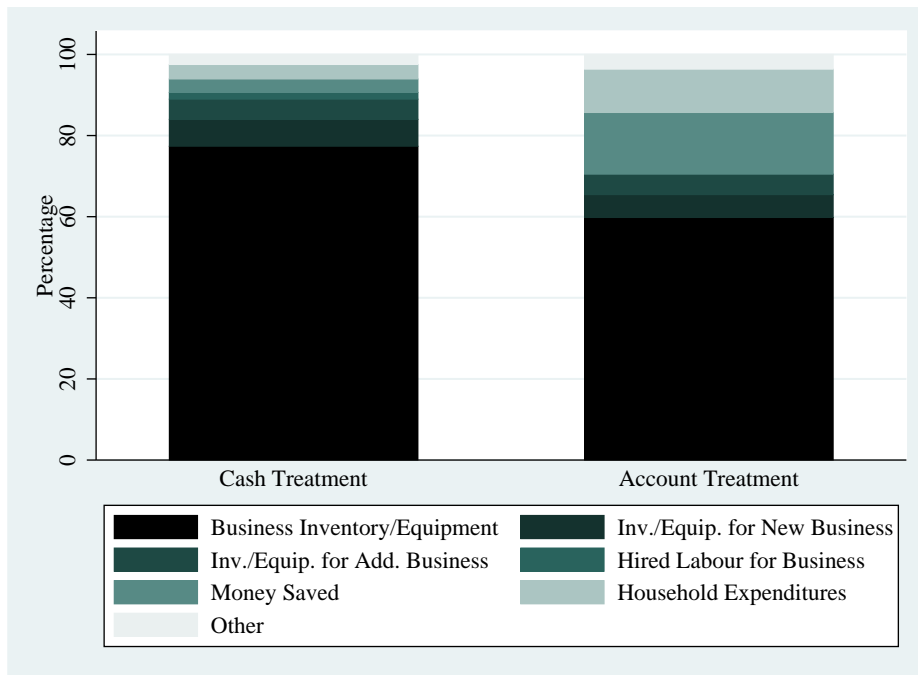


Figure 2.1: Use of Transfer - Cash vs. Account

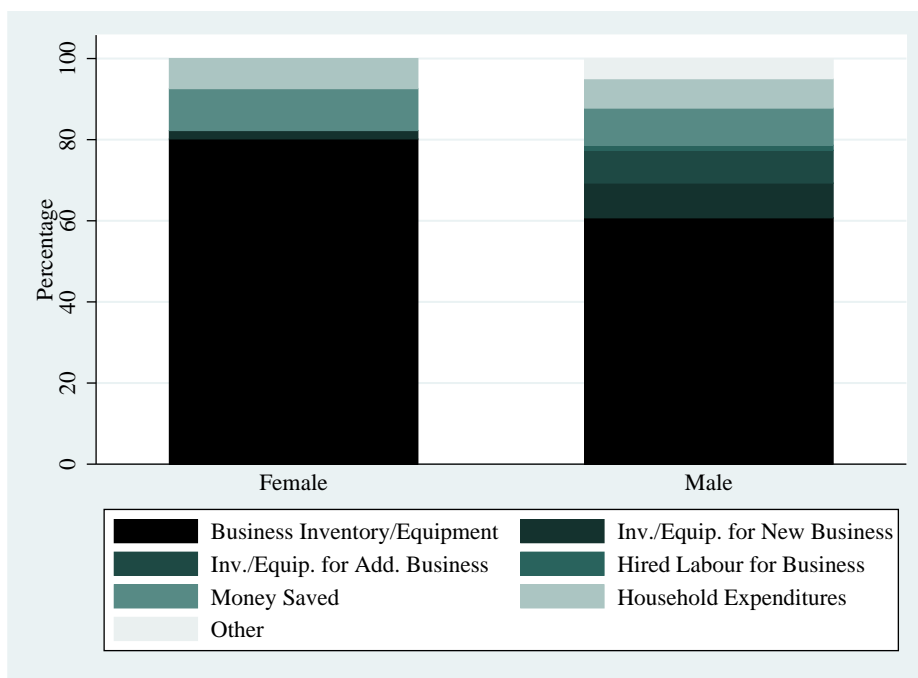


Figure 2.2: Use of Transfer - Men vs. Women

Chapter 3

Does Financial Literacy Improve Financial Inclusion? Cross Country Evidence¹

with:

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Chapter 4

Don't Expect too Much: The Effect of Biased Expectations on (Over)-Indebtedness¹

with:

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4.1 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 4.1). 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.² A major contribution to the lit-

² 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.

erature 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 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. The results vary with respect to different debt and expectations indicators. 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, which describes financial distress and not so much indebtedness. The results are robust to various specifications and become more precise if we exclude parts of the sample that may have 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 behav-

ioral 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”³ 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 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 4.2 presents the survey data we use and explains how our variables of interest are constructed. In section 4.3, the estimation

³ Western, educated, industrialized, rich and democratic

strategy is outlined and survey results are presented. Section 4.4 describes the experiment and its results, while Section 4.5 concludes.

4.2 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.

4.2.1 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).⁴ 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 4.2 and 4.3). Our sample

⁴ See <https://www.tvsep.de/overview-tvsep.html>

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 4.2 and 4.3 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 4.2.3), savings, financial literacy, borrowing behavior in general, optimism, and income expectations (see Sub-Section 4.2.2). 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 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).⁵ Adjusted to the low numeracy within the sample, we add a phrase to each numerical value on questions involving scales.

4.2.2 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.

⁵ 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.

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 calculating 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.⁶ 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.⁷

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

⁶ Studies dealing with these kind of expectation elicitation 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.

⁷ The average respondent in our sample only attended school for six years.

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.⁸ Respondents distribute their ten marbles based on how certain they are that their future monthly income will lie in each specific bin.⁹ 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) 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 4.1 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 4.1. Additionally, Figure 4.4 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 4.4 about here]

⁸ 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.

⁹ 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.

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).¹⁰

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 Hyttinen and Putkuri (2018) apply them.

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

$$Biased (= 1) \text{ if } Inc_{i,t+1} - Inc_{i,t} > 0 \quad (4.1)$$

While we cannot formally test rationality of expectations with our subjective expected income data,¹¹ 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).

¹⁰ 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.

¹¹ 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.

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 ($A_{i,t}$).¹² As in Hyytinen and Putkuri (2018), the difference between these two questions is coined financial forecast error:

$$\text{Financial Forecast Error} = E_{i,t-1} - A_{i,t} \quad (4.2)$$

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 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.

¹² 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.

4.2.3 (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.¹³ 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 D.

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 household feels it has too much debt, whether it has difficulties paying them off, and the so-called “sacrifice index”.¹⁴ The index and its components are explained in detail in Appendix D. 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

¹³ Among others, D’Alessio and Iezzi (2013) provide a summary on different indebtedness indicators, their usage, and possible drawbacks.

¹⁴ We closely follow Schicks (2013) in constructing the sacrifice index.

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 D. All indices we derive point to accumulating more debt the higher the household scores.

4.2.4 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 4.3, 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 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 (Demirgüç-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,¹⁵ 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 4.5 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 4.5 about here]

A descriptive overview of our main variables of interest is provided in Table 4.2. The first part represents the two bias measures explained in Sub-Section 4.2.2. 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 develop-

¹⁵ 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)

ment match well in our sample: The median respondent does not make any forecast error (i.e. the difference is zero).

The second part of Table 4.2 depicts our previously derived objective and subjective debt measures (see Sub-Section 4.2.3). 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 4.2 about here]

Furthermore, Table 4.3 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 4.3 about here]

Another important variable for our study is financial literacy. Our financial literacy index (described in Sub-Section 4.2.1) 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 4.6 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 4.6 about here]

4.3 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 4.4.

4.3.1 Estimation Strategy

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

$$DebtMeasure_i = \beta_0 + \beta_1 Bias_i + X_i' \beta_2 + \epsilon_i \quad (4.3)$$

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,¹⁶ the subjective debt index,¹⁷ the debt service to income ratio, the sacrifice index, and an over-indebtedness index.¹⁸

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).

4.3.2 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

¹⁶ 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

¹⁷ Standardized average of the sacrifice index, answers to questions on debt position and whether the household has difficulties paying off debt

¹⁸ 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.

with each other, namely the bias derived from the expected median income and the financial forecast error. Tables 4.4, 4.5, and 4.6 provide results on the expected income bias measure and Tables 4.7, 4.8, and 4.9 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 4.4 - 4.9 about here]

We find a strong statistically significant relation between both bias indicators and the objective debt measure. The objective debt index¹⁹ 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 4.4). 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,²⁰ the objective debt index increases by 0.11 - 0.14 standard deviation units (columns (1) and (2), Table 4.7). 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-

¹⁹ 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.

²⁰ This means households are more optimistic regarding their future income development than what was actually realized and recalled later on

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 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 4.8). 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 measures²¹ 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.²² 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 4.9). The over-indebtedness index increases by 0.10 - 0.13 standard deviation units, when the financial forecast error increases by one unit. Both index

²¹ The Big Five comprise the following personality traits: openness, conscientiousness, extraversion, agreeableness, and neuroticism. More details on their construction are found in Appendix D.

²² 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.

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 4.9). 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 4.6).

In an additional exercise, we add an income certainty measure as a control variable to our main specifications in order to investigate whether the certainty about future household income development affects (over-)indebtedness status.²³ Tables 4.10, 4.11, and 4.12 present results.

[Tables 4.10 - 4.12 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 4.10) and a higher “debt position” (columns (3) and (4), Table 4.11).

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 4.12). 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.²⁴

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,

²³ Details on how the certainty measure is constructed are found in Appendix D.

²⁴ 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.

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.

4.3.3 Robustness

Excluding Possibly Confounding Observations. Before eliciting the subjective expected income of respondents, we ask two questions testing their understanding of probability. 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 D.1, D.2, and D.3 in the Appendix. The effects for this subsample 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 D.4, D.5, and D.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 Hyttinen 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 D.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 D.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.

4.4 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.

4.4.1 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.²⁵ 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.²⁶ Participants ranked 1-4 do not earn anything additionally to their endowment, 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 peo-

²⁵ 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.

²⁶ 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 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.

ple 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 4.7).²⁷ We subsequently measure confidence as difference between expected rank and actual rank:

$$confidence = rank_{exp} - rank_{act} \quad (4.4)$$

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.²⁸

[Figure 4.7 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.²⁹ 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

²⁷ 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.

²⁸ 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.

²⁹ Still, there are participants who had serious difficulties in understanding the game such that we exclude them from the main analysis

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 is available upon request.

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.³⁰

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” (D.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

³⁰ 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.

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.

4.4.2 Experimental Results

Overall, 604 respondents participated in the game. Since participation is self-selected, participants and non-participants are compared in Table D.9 in the Appendix. As can be seen, participants and non-participants significantly differ in some variables.³¹ 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 D.10). Since the time of day at which we visited households for the interviews is mostly exogenous,³² 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

³¹ A complete list of all variables and their explanation is provided in the Appendix.

³² 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.

analysis because they are outliers in income or the debt service to income ratio (see Section 4.2.4).³³ Additionally, 84 observations are excluded because it can be inferred from the data that comprehension was insufficient³⁴ 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 4.13 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 4.13 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 4.8 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.³⁵ Indeed, if we compare the distributions of the “second” expectations that are elicited after respondents actually took the main quiz, we find a much larger shift (see Appendix Figure D.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

³³ The results are robust to this exclusion.

³⁴ 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.

³⁵ We focus on the expected rank in our analysis but everything holds analogously for expected earnings.

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 4.11).

[Figure 4.8 about here]

The difference in self-confidence is larger than the difference in expected rank (see Figure 4.9). 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 4.9 and 4.10 about here]

As seen in Figure 4.10, 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.³⁶ 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 4.12.

[Figures 4.11 and 4.12 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 4.14).

[Table 4.14 about here]

³⁶ On this discussion see Merkle and Weber (2011).

A similar pattern emerges if we look explicitly at spending behavior (see Table 4.15). 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.³⁷

[Table 4.15 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 4.16). Likewise, our regressions results from Section 4.3.2 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 4.16 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.

4.4.3 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.³⁸ 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:

³⁷The 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.

³⁸This 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.

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 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.³⁹ The rural Thai population may have more persistent beliefs than Western populations, which makes 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.

4.5 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

³⁹ For example, Grohmann et al. (2019) only use four questions they frame as “example questions” and find larger treatment effects on expectations.

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.

Tables and Figures

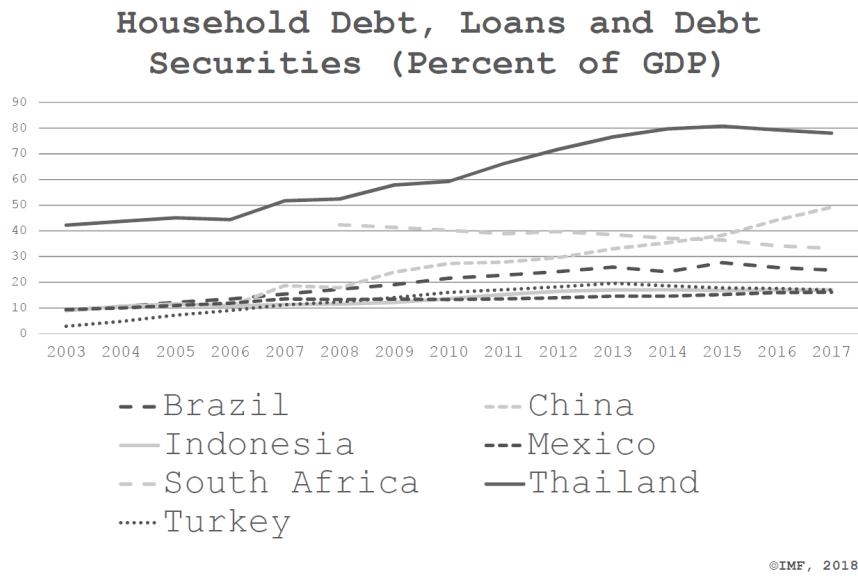


Figure 4.1: Household Debt to GDP Ratio, Selected Emerging Markets

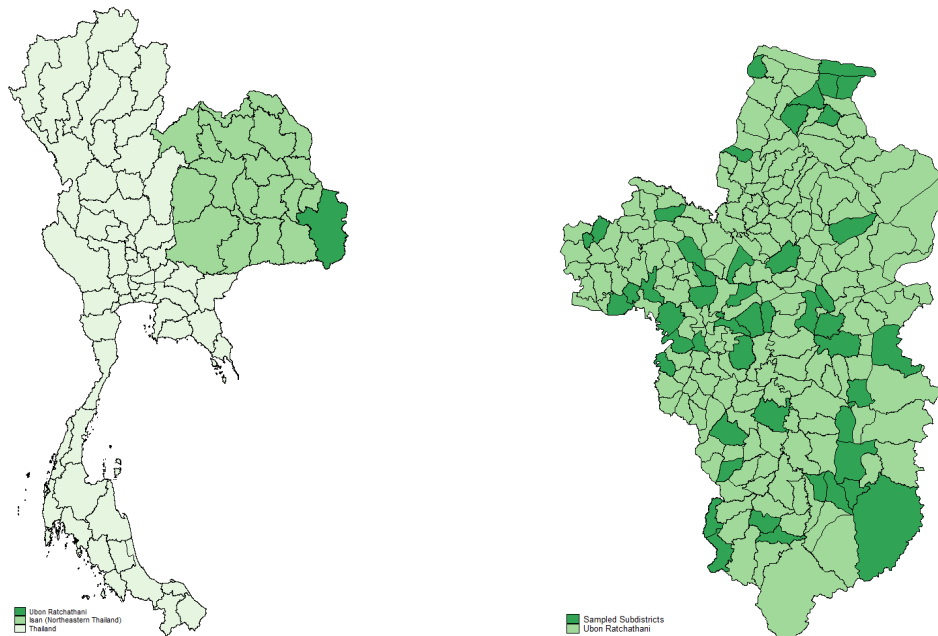


Figure 4.2: Study Site, Ubon Ratchathani
Figure 4.3: Sampled Subdistricts Thailand

Table 4.1: 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

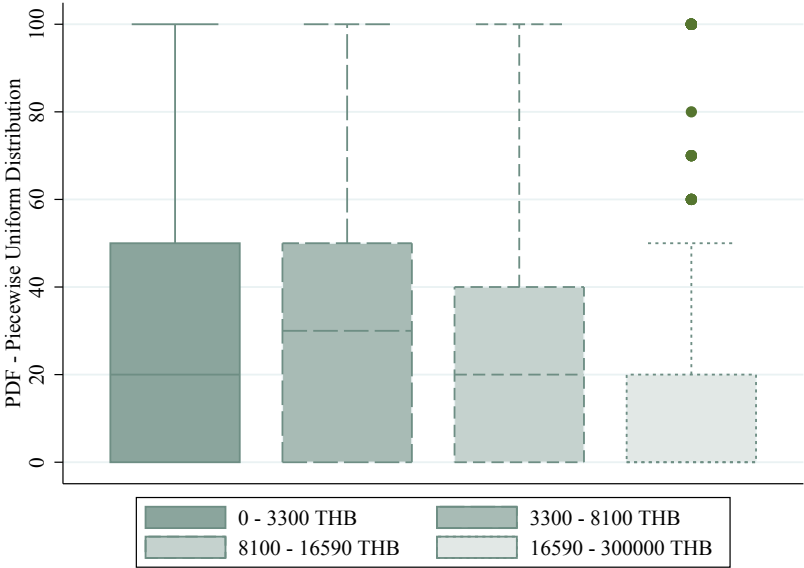


Figure 4.4: Probability Density Function of Expected Income

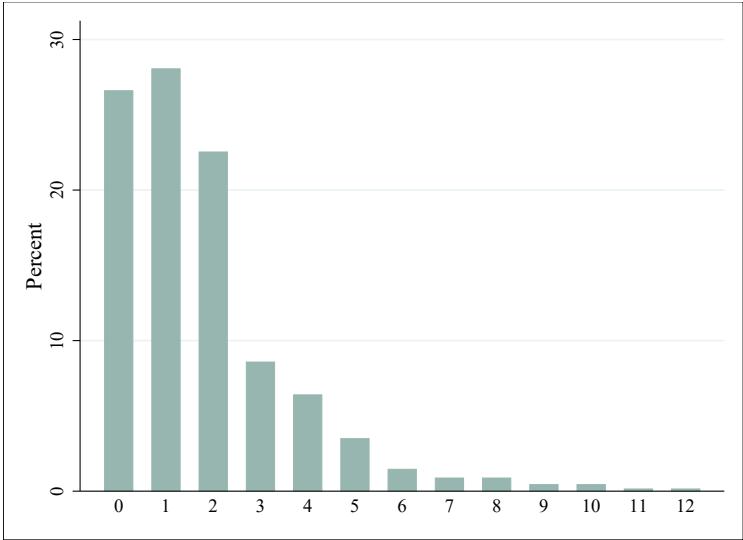


Figure 4.5: Number of Loans

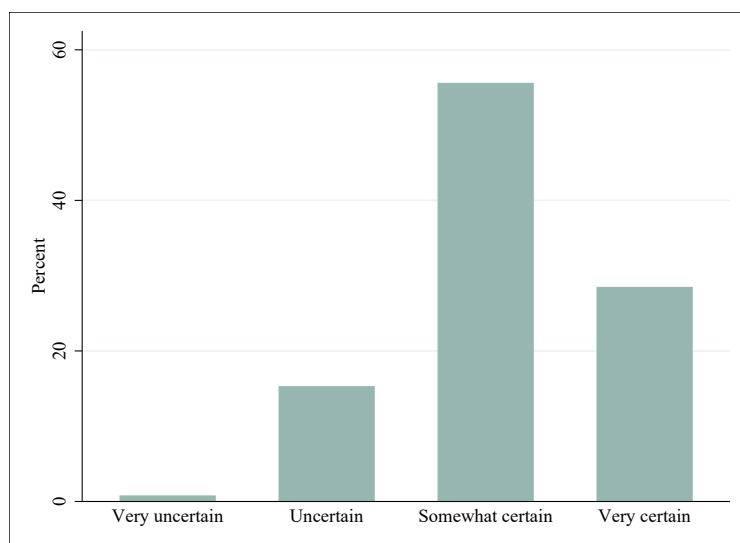


Figure 4.6: Income Certainty

Table 4.2: Summary Statistics - Main Variables

	Mean	SD	Minimum	Maximum	Observations
<i>Expectation Bias Indices</i>					
Expected Income Bias (=1)	0.24	0.43	0	1	686
Financial Forecast Error	0.17	0.95	-3	3	674
<i>Debt Variables</i>					
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 4.3: Correlation Matrix - Debt Variables

	Obj. Debt	DSR 2017	RD to Inc.	Paid Late/ Default	Over- indebted	=1 if DSR > 40%	=1 if Loans > 4	Subj. Debt	Sacrifice Index	Debt Position	Diff. Pay. Debt
Obj. Debt	1										
Index											
DSR 2017	0.694***	1									
Remain. Debt to Income	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.

Table 4.4: 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*** (0.107)	0.357*** (0.110)	0.205*** (0.055)	0.149** (0.057)	0.197*** (0.072)	0.162** (0.077)	0.058* (0.032)	0.070** (0.032)
Monthly Inc. 2017		-0.000** (0.000)		-0.000*** (0.000)		-0.000*** (0.000)		-0.000 (0.000)
Age		0.051*** (0.018)		0.015* (0.009)		0.028*** (0.010)		0.013* (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* (0.040)		0.013 (0.022)		0.040** (0.020)		0.024 (0.019)
FL-Score		-0.001 (0.015)		0.012* (0.006)		0.009 (0.012)		-0.009* (0.005)
Risk Aversion		0.030 (0.020)		0.012 (0.009)		0.005 (0.017)		0.009 (0.008)
Self-Control		0.006 (0.005)		0.001 (0.002)		-0.001 (0.004)		0.003* (0.002)
Main Inc. Farming		-0.327* (0.168)		-0.142 (0.091)		0.116 (0.093)		-0.106* (0.057)
Main Inc. Employed		-0.378** (0.169)		-0.228*** (0.082)		-0.015 (0.088)		-0.057 (0.062)
Main Inc. Self-Emp.		-0.242 (0.208)		-0.217** (0.090)		0.191 (0.179)		-0.029 (0.077)
Main Inc. Remitt.		-0.395** (0.162)		-0.195** (0.083)		-0.003 (0.092)		-0.092 (0.060)
Children (0-6 yrs)		-0.044 (0.051)		-0.021 (0.028)		-0.102*** (0.026)		0.005 (0.022)
Children (7-10 yrs)		0.038 (0.077)		0.014 (0.034)		0.033 (0.044)		0.003 (0.031)
Children (11-16 yrs)		0.070 (0.066)		0.002 (0.030)		0.023 (0.034)		0.028 (0.027)
No. of Elders		0.062 (0.053)		0.008 (0.030)		0.019 (0.040)		0.027 (0.020)
No. of Working Mem.		0.012 (0.043)		0.010 (0.021)		0.002 (0.024)		-0.001 (0.015)
Total HH Education		0.002 (0.005)		0.002 (0.002)		-0.001 (0.002)		0.001 (0.002)
Constant	-0.098** (0.047)	-1.011* (0.584)	0.185*** (0.021)	-0.114 (0.273)	0.294*** (0.035)	-0.304 (0.385)	0.141*** (0.017)	-0.066 (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 4.5: 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 (0.093)	0.100 (0.093)	0.072 (0.083)	0.119 (0.085)	0.013 (0.053)	0.023 (0.054)	0.054 (0.104)	0.078 (0.104)
Monthly Inc. 2017		-0.000 (0.000)		-0.000 (0.000)		-0.000 (0.000)		-0.000 (0.000)
Age		0.062*** (0.018)		0.062*** (0.016)		0.022** (0.011)		0.045** (0.020)
Age Squared		-0.001*** (0.000)		-0.001*** (0.000)		-0.000*** (0.000)		-0.000** (0.000)
No. of Shocks		0.102** (0.042)		0.083** (0.033)		0.020 (0.024)		0.136** (0.054)
FL-Score		-0.034** (0.015)		0.006 (0.013)		-0.020** (0.009)		-0.066*** (0.016)
Risk Aversion		0.050** (0.021)		0.055*** (0.018)		0.027** (0.013)		0.013 (0.024)
Self-Control		0.010** (0.004)		0.004 (0.003)		0.005* (0.003)		0.015*** (0.005)
Main Inc. Farming		-0.187 (0.147)		-0.096 (0.127)		0.009 (0.087)		-0.397** (0.191)
Main Inc. Employed		-0.033 (0.160)		-0.029 (0.138)		0.054 (0.094)		-0.158 (0.203)
Main Inc. Self-Emp.		-0.016 (0.168)		-0.000 (0.136)		0.053 (0.103)		-0.149 (0.219)
Main Inc. Remitt.		-0.209 (0.151)		-0.157 (0.127)		-0.066 (0.090)		-0.241 (0.188)
Children (0-6 yrs)		-0.045 (0.064)		-0.084 (0.053)		0.013 (0.039)		-0.033 (0.077)
Children (7-10 yrs)		-0.067 (0.075)		0.071 (0.068)		-0.064 (0.044)		-0.166* (0.094)
Children (11-16 yrs)		0.070 (0.071)		0.045 (0.054)		-0.005 (0.045)		0.141 (0.092)
No. of Elders		0.003 (0.050)		0.027 (0.046)		0.014 (0.032)		-0.052 (0.061)
No. of Working Mem.		0.122*** (0.042)		0.114*** (0.039)		0.014 (0.028)		0.159*** (0.056)
Total HH Education		-0.010** (0.005)		-0.007 (0.004)		-0.002 (0.003)		-0.016*** (0.006)
Constant	-0.016 (0.050)	-1.349** (0.563)	-0.043 (0.043)	-1.993*** (0.468)	1.367*** (0.031)	1.041*** (0.350)	-0.100* (0.060)	-0.537 (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 4.6: Income Expectation Bias Dummy - Over-Indebtedness Indicators

	Over-indebtedness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Bias Dummy	0.188** (0.090)	0.133 (0.094)	0.131*** (0.037)	0.093** (0.037)	-0.007 (0.028)	-0.005 (0.030)
Monthly Inc. 2017		-0.000 (0.000)		-0.000*** (0.000)		0.000 (0.000)
Age		0.033** (0.015)		0.006 (0.008)		0.014*** (0.004)
Age Squared		-0.000*** (0.000)		-0.000 (0.000)		-0.000*** (0.000)
No. of Shocks		0.038 (0.045)		0.001 (0.018)		0.022 (0.014)
FL-Score		0.020 (0.015)		0.008 (0.006)		0.005 (0.005)
Risk Aversion		0.047*** (0.018)		0.013* (0.007)		0.016** (0.006)
Self-Control		-0.003 (0.004)		-0.002 (0.002)		-0.000 (0.001)
Main Inc. Farming		-0.090 (0.142)		-0.066 (0.062)		0.006 (0.047)
Main Inc. Employed		-0.266* (0.147)		-0.114* (0.060)		-0.055 (0.050)
Main Inc. Self-Emp.		-0.261 (0.173)		-0.100 (0.076)		-0.064 (0.059)
Main Inc. Remitt.		-0.236 (0.165)		-0.073 (0.067)		-0.074 (0.053)
Children (0-6 yrs)		-0.056 (0.063)		-0.014 (0.025)		-0.021 (0.022)
Children (7-10 yrs)		0.059 (0.085)		0.013 (0.034)		0.023 (0.029)
Children (11-16 yrs)		-0.033 (0.061)		-0.018 (0.028)		-0.003 (0.022)
No. of Elders		-0.056 (0.062)		0.002 (0.023)		-0.035* (0.020)
No. of Working Mem.		0.052 (0.038)		0.019 (0.016)		0.014 (0.014)
Total HH Education		-0.000 (0.005)		-0.000 (0.002)		0.000 (0.002)
Constant	-0.044 (0.057)	-0.901* (0.542)	0.150*** (0.019)	0.088 (0.250)	0.145*** (0.020)	-0.304* (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 4.7: 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*** (0.045)	0.114** (0.045)	0.040** (0.019)	0.031 (0.019)	0.110*** (0.038)	0.098** (0.039)	0.031* (0.018)	0.023 (0.018)
Monthly Inc. 2017		-0.000*** (0.000)		-0.000*** (0.000)		-0.000*** (0.000)		-0.000 (0.000)
Age		0.051*** (0.019)		0.016* (0.008)		0.029*** (0.010)		0.012* (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 (0.045)		0.008 (0.023)		0.026 (0.019)		0.021 (0.020)
FL-Score		0.003 (0.015)		0.015** (0.006)		0.013 (0.012)		-0.009 (0.006)
Risk Aversion		0.028 (0.020)		0.012 (0.010)		0.004 (0.017)		0.007 (0.008)
Self-Control		0.007 (0.004)		0.001 (0.002)		-0.001 (0.004)		0.003* (0.002)
Main Inc. Farming		-0.380** (0.172)		-0.166* (0.091)		0.094 (0.093)		-0.115* (0.058)
Main Inc. Employed		-0.475*** (0.170)		-0.264*** (0.080)		-0.050 (0.085)		-0.081 (0.064)
Main Inc. Self-Emp.		-0.312 (0.207)		-0.239*** (0.088)		0.165 (0.181)		-0.049 (0.078)
Main Inc. Remitt.		-0.442*** (0.162)		-0.219*** (0.083)		-0.030 (0.095)		-0.098 (0.061)
Children (0-6 yrs)		-0.038 (0.051)		-0.017 (0.028)		-0.100*** (0.025)		0.005 (0.023)
Children (7-10 yrs)		0.026 (0.081)		0.012 (0.034)		0.038 (0.047)		-0.004 (0.032)
Children (11-16 yrs)		0.051 (0.066)		0.000 (0.031)		0.017 (0.035)		0.019 (0.027)
No. of Elders		0.047 (0.052)		-0.001 (0.029)		0.009 (0.039)		0.027 (0.019)
No. of Working Mem.		-0.022 (0.046)		-0.002 (0.022)		-0.016 (0.027)		-0.008 (0.016)
Total HH Education		0.005 (0.005)		0.003 (0.003)		0.000 (0.002)		0.001 (0.002)
Constant	-0.023 (0.040)	-0.852 (0.609)	0.230*** (0.020)	-0.071 (0.260)	0.326*** (0.029)	-0.298 (0.392)	0.148*** (0.015)	-0.008 (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 4.8: 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*** (0.045)	0.109** (0.045)	0.140*** (0.037)	0.097** (0.038)	0.057** (0.028)	0.042 (0.028)	0.139*** (0.048)	0.091* (0.046)
Monthly Inc. 2017		-0.000 (0.000)		-0.000 (0.000)		-0.000 (0.000)		-0.000* (0.000)
Age		0.057*** (0.020)		0.057*** (0.017)		0.020 (0.012)		0.043** (0.022)
Age Squared		-0.001*** (0.000)		-0.001*** (0.000)		-0.000** (0.000)		-0.000** (0.000)
No. of Shocks		0.084* (0.043)		0.065* (0.034)		0.014 (0.024)		0.122** (0.055)
FL-Score		-0.033** (0.015)		0.008 (0.013)		-0.022** (0.009)		-0.061*** (0.017)
Risk Aversion		0.047** (0.021)		0.051*** (0.018)		0.026** (0.013)		0.010 (0.024)
Self-Control		0.011*** (0.004)		0.004 (0.003)		0.004* (0.003)		0.017*** (0.005)
Main Inc. Farming		-0.183 (0.141)		-0.104 (0.122)		0.015 (0.085)		-0.387** (0.185)
Main Inc. Employed		-0.036 (0.157)		-0.036 (0.133)		0.054 (0.094)		-0.156 (0.199)
Main Inc. Self-Emp.		-0.021 (0.162)		-0.017 (0.130)		0.046 (0.100)		-0.127 (0.216)
Main Inc. Remitt.		-0.199 (0.143)		-0.168 (0.119)		-0.057 (0.087)		-0.218 (0.183)
Children (0-6 yrs)		-0.041 (0.067)		-0.082 (0.055)		0.015 (0.042)		-0.028 (0.078)
Children (7-10 yrs)		-0.074 (0.077)		0.070 (0.071)		-0.072 (0.045)		-0.169* (0.095)
Children (11-16 yrs)		0.064 (0.070)		0.045 (0.054)		-0.007 (0.043)		0.129 (0.091)
No. of Elders		-0.003 (0.050)		0.016 (0.046)		0.014 (0.031)		-0.055 (0.061)
No. of Working Mem.		0.098** (0.041)		0.091** (0.038)		0.006 (0.028)		0.139** (0.055)
Total HH Education		-0.008* (0.005)		-0.004 (0.004)		-0.001 (0.003)		-0.015*** (0.006)
Constant	-0.019 (0.043)	-1.170* (0.630)	-0.041 (0.037)	-1.784*** (0.516)	1.364*** (0.027)	1.145*** (0.389)	-0.102* (0.053)	-0.520 (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 4.9: Fin. Forecast Error - Over-Indebtedness Indicators

	Over-indebtedness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Fin. Forecast Error	0.133*** (0.035)	0.108*** (0.039)	0.037*** (0.014)	0.032** (0.015)	0.045*** (0.014)	0.035** (0.014)
Monthly Inc. 2017		-0.000* (0.000)		-0.000*** (0.000)		0.000 (0.000)
Age		0.033** (0.015)		0.008 (0.007)		0.012*** (0.004)
Age Squared		-0.000*** (0.000)		-0.000* (0.000)		-0.000*** (0.000)
No. of Shocks		0.020 (0.050)		-0.004 (0.020)		0.016 (0.015)
FL-Score		0.023 (0.015)		0.010 (0.006)		0.005 (0.005)
Risk Aversion		0.046** (0.018)		0.013* (0.007)		0.016** (0.006)
Self-Control		-0.003 (0.004)		-0.002 (0.002)		-0.000 (0.001)
Main Inc. Farming		-0.103 (0.141)		-0.077 (0.063)		0.009 (0.045)
Main Inc. Employed		-0.288** (0.140)		-0.133** (0.059)		-0.051 (0.050)
Main Inc. Self-Emp.		-0.271 (0.171)		-0.112 (0.075)		-0.060 (0.060)
Main Inc. Remitt.		-0.250 (0.163)		-0.085 (0.067)		-0.071 (0.052)
Children (0-6 yrs)		-0.048 (0.064)		-0.009 (0.025)		-0.020 (0.022)
Children (7-10 yrs)		0.058 (0.086)		0.015 (0.034)		0.021 (0.029)
Children (11-16 yrs)		-0.031 (0.062)		-0.019 (0.029)		-0.001 (0.022)
No. of Elders		-0.063 (0.062)		-0.005 (0.023)		-0.033 (0.021)
No. of Working Mem.		0.031 (0.040)		0.010 (0.016)		0.009 (0.014)
Total HH Education		0.002 (0.005)		0.000 (0.002)		0.001 (0.002)
Constant	-0.021 (0.049)	-0.858 (0.540)	0.176*** (0.017)	0.064 (0.236)	0.135*** (0.017)	-0.257 (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 4.10: 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*** (0.113)		0.147** (0.059)		0.151* (0.080)		0.070** (0.032)	
Fin. Forecast Error		0.118** (0.045)		0.033* (0.019)		0.098** (0.039)		0.025 (0.018)
Overprecision	0.034 (0.065)	0.030 (0.066)	0.057* (0.032)	0.057* (0.033)	-0.007 (0.047)	-0.011 (0.046)	-0.014 (0.023)	-0.015 (0.023)
Constant	-0.891 (0.669)	-0.761 (0.695)	-0.216 (0.332)	-0.199 (0.314)	-0.226 (0.506)	-0.266 (0.504)	0.049 (0.244)	0.115 (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 4.11: 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 (0.094)		0.114 (0.086)		0.004 (0.053)		0.057 (0.106)	
Fin. Forecast Error		0.109** (0.045)		0.098** (0.039)		0.042 (0.028)		0.091* (0.047)
Overprecision	-0.004 (0.068)	-0.006 (0.069)	0.092* (0.054)	0.092* (0.053)	-0.054 (0.041)	-0.055 (0.042)	-0.031 (0.084)	-0.036 (0.085)
Constant	-1.330** (0.611)	-1.229* (0.673)	-2.357*** (0.534)	-2.204*** (0.576)	1.242*** (0.378)	1.311*** (0.417)	-0.367 (0.751)	-0.429 (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 4.12: Certainty Measure - Over-Indebtedness Indicators

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

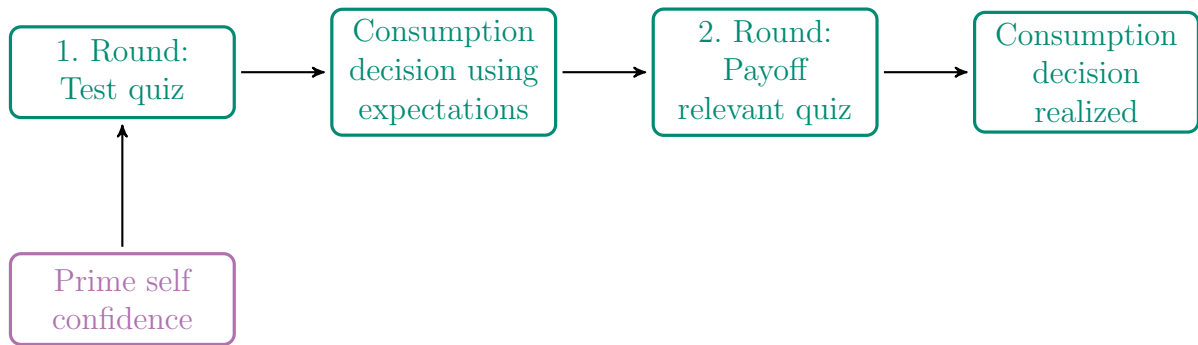


Figure 4.7: Experimental Flow

Table 4.13: 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

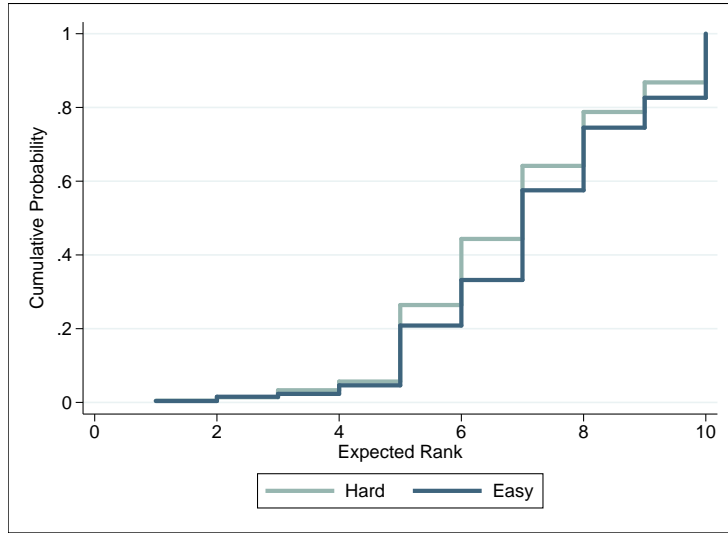


Figure 4.8: Cumulative Density Distribution of Expected Rank by Treatment

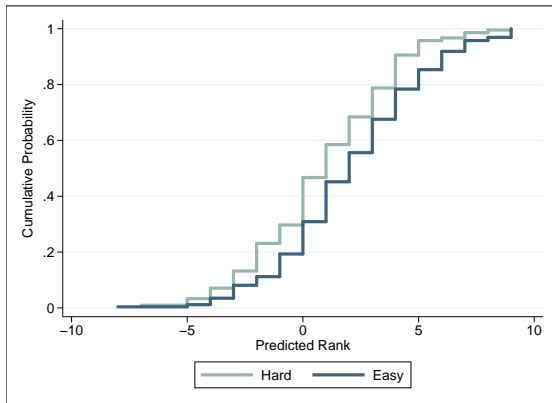


Figure 4.9: CDFs of Self-Confidence

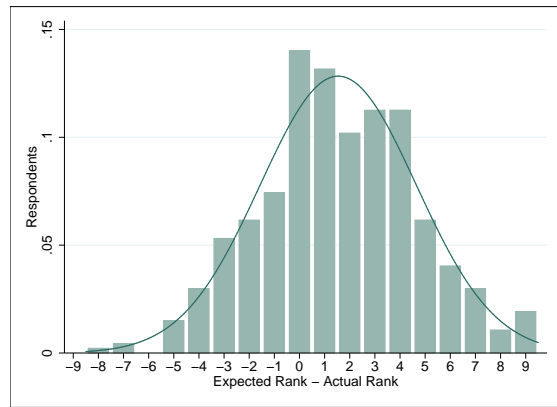


Figure 4.10: Histogram for Self-Confidence

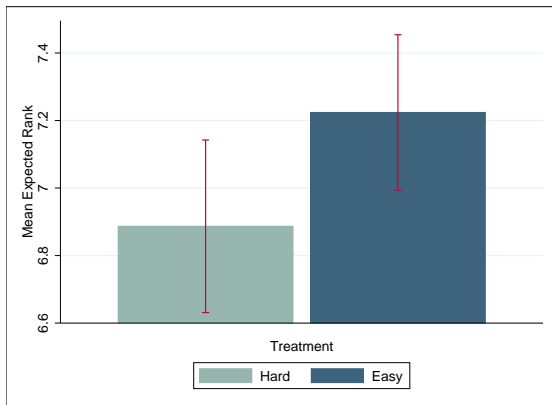


Figure 4.11: Mean Expected Rank by Treatment

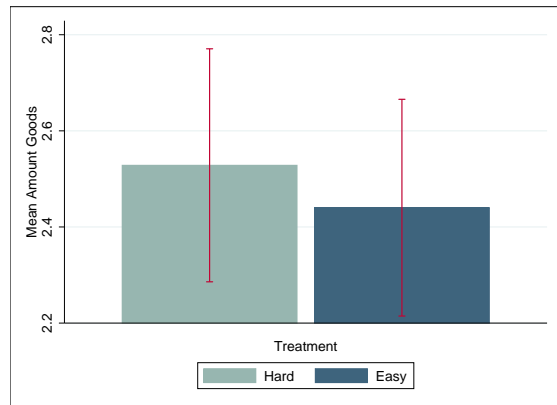


Figure 4.12: Mean Consumption by Treatment

Table 4.14: Consumption Decision

	Exp. Rank		No. Goods	
	(1)	(2)	(3)	(4)
Treatment	0.371** (0.175)	-0.143 (0.173)		-0.200 (0.171)
Exp. Rank			0.147*** (0.046)	0.152*** (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 4.15: Overborrowing and Overspending

	Overconfidence		Overborrowing		Overspending	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	1.216*** (0.282)	0.009 (0.019)	-0.008 (0.019)		-0.035 (0.045)	
Overconfidence			0.014*** (0.004)		0.044*** (0.007)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	470	470	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 4.16: 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 (0.076)			0.001 (0.009)			0.039* (0.022)		
Subj. Debt Index		0.137* (0.080)			-0.010 (0.008)			0.054** (0.022)	
Over-Indebtedness Index			0.081 (0.079)			0.008 (0.010)			0.046** (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 4.13.

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Appendix A - Appendix for Chapter 2

Table A.1: Actual Treatment for Baseline and Follow-up Waves

	Oct.12	Apr.13	Oct.13	Oct.14	Oct.15	Oct.16	Oct.17	Total
Account Treatment	47	48	48	46	43	38	34	304
Cash Treatment	48	48	47	45	38	36	35	297
Control Group	92	92	88	71	63	58	49	513
<i>N</i>	187	188	183	162	144	132	118	1,114

Source: Own calculations

Table A.2: Analysis of Attrition - Baseline Balance

	Obs.	Not Surveyed	Surveyed	Difference	p-value
Central Division	349	0.41	0.48	-0.078	0.160
Kawempe Division	349	0.19	0.063	0.120***	0.0003
Makindye Division	349	0.078	0.063	0.015	0.600
Nakawa Division	349	0.19	0.23	-0.039	0.395
Rubaga Division	349	0.14	0.16	-0.022	0.581
Manufacturing	349	0.25	0.53	-0.280***	0.0000
Trade	349	0.58	0.29	0.280***	0.0000
Services	349	0.094	0.12	-0.024	0.491
Age	346	33.8	35.3	-1.520*	0.0983
Male	349	0.53	0.65	-0.120**	0.0265
Married*	176	0.64	0.69	-0.047	0.527
No primary education	349	0.047	0.12	-0.071**	0.0273
Completed primary school	349	0.23	0.31	-0.078	0.121
Completed O-level	349	0.28	0.21	0.069	0.148
Completed A-Level	349	0.16	0.20	-0.043	0.320
Completed University	349	0.25	0.11	0.140***	0.0004
Speaks English fluently	342	0.77	0.66	0.110**	0.0272
Household Size	349	2.89	2.93	-0.037	0.895
Assetindex	174	0.089	0.054	0.035***	0.0016
Total Savings	175	2580.5	1851.0	729.5	0.418
1=Credit Constrained*	203	0.77	0.84	-0.066	0.246
1=Impatient	346	0.77	0.89	-0.120***	0.0039
1=Present Biased*	175	0.55	0.63	-0.084	0.279
Firm Age*	171	5.03	7.94	-2.900***	0.0034
1=Formal Business*	173	0.30	0.20	0.095	0.158
Start-up Capital	344	6752.5	1502.1	5250.500***	0.0000
Own-Account Worker	349	0.35	0.39	-0.038	0.486
Owner Labor Hours	335	310.8	316.3	-5.500	0.510
Employee Labor Hours	349	293.0	292.9	0.120	0.998
Business Savings	329	1879.1	658.2	1220.900***	0.0020
Sales	343	10429.2	5499.3	4929.900***	0.0057
Costs	349	9890.7	4434.8	5455.900***	0.0013
Profits	342	1885.9	890.1	995.800**	0.0184
<i>N</i>	349				

Note: Characteristics denoted with an asterisk use values from one of the baseline surveys only, all other measures depict the average of both baselines (October 2012 and April 2013). P-values for tests of equality between account and control group. *, **, and *** on p-values denote significant differences at the 10, 5, and 1 percent levels, respectively. All values are given in 1000 UGX.

Heterogeneity Analysis - Interaction with Gender

Descriptive baseline results reveal that male and female led businesses differ significantly from each other in various ways: Male-led businesses are older on average (7.9 vs. 5.8 years) and less credit constrained (46% vs. 67%). Accumulated capital stock is much higher (2,308,000 UGX vs. 1,309,000 UGX), their firms sell more (8,711,000 UGX vs. 3,659,000 UGX) and yield higher profits (1,383,000 UGX vs. 785,000 UGX).⁴⁰ Moreover, the finding that cash transfers and loans are used differently by gender is established in the related literature (e.g. de Mel et al., 2009a; Fafchamps et al., 2014; Fiala, 2018).

We present results from interacting the treatment variables with gender in Tables A.3 to A.8. The tables provide estimates with respect to capital stock, monthly profits, inventories, sales, and (business) savings in both levels and logs. As in our main equation, all regressions control for the baseline value of the respective outcome variable, dummies for the division and the industry the business operates in, business owner's level of education and age, the amount of available baseline start-up capital and household wealth, the number of employees, and age of the firm. We further interact gender with the respective wave.

Similar to our main regression results, there is no robust and significant heterogeneous treatment effect on capital stock across both treatment groups (Table A.3). Results in levels for monthly profits and inventories suggest a positive treatment effect for men compared to women in the account group. However, this effect vanishes when looking at the log-regressions which might be due to these specifications accommodating extreme values better (Tables A.4 and A.5). Sales for female and male led businesses are not significantly differently affected by the cash transfer, but, nevertheless, coefficients for the interactions consistently display positive (though insignificant) treatment results for men in both treatment arms (Table A.6).

Last, there is no conclusive finding regarding heterogeneous treatment effects when we examine the interactions with respect to savings and business savings as outcome variables (see Tables A.7 and A.8.) Hence, we are cautious in interpreting our interaction results as they are often only marginally significant or change coefficient signs depending on whether the regression is presented in levels or logs. In conclusion, the transfer does not seem to affect women and men differently.

⁴⁰ The difference for firm profits is significant at the 10% level, for firm age it is significant at the 5% level, all other differences mentioned are significant at the 1% level (results from two-sided t-tests).

Table A.3: Het. Treatment Effects w.r.t. Gender - Capital Stock

	Capital Stock			Log Capital Stock		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	61.979 (513.261)	-34.925 (364.063)	271.608 (582.112)	0.124 (0.277)	-0.116 (0.249)	0.280 (0.351)
1=Male	356.830 (357.734)	225.537 (212.406)	319.914 (346.471)	0.072 (0.106)	0.054 (0.066)	0.059 (0.098)
Account \times Male=1	906.240 (939.226)	1174.523 (980.642)	532.109 (1249.163)	0.110 (0.415)	0.241 (0.443)	0.061 (0.495)
Cash Treatment	368.757 (418.771)	-156.395 (297.030)	739.707 (686.337)	-0.211 (0.243)	-0.357* (0.205)	-0.186 (0.366)
Cash \times Male=1	-407.953 (989.359)	243.511 (742.145)	-949.991 (1343.279)	0.133 (0.374)	0.278 (0.311)	0.120 (0.514)
Constant	-964.611 (1161.354)	-583.546 (695.166)	-990.540 (1125.603)	1.419*** (0.530)	0.608* (0.328)	1.386*** (0.530)
Observations	851	547	609	851	547	609
R squared	0.34	0.62	0.34	0.63	0.78	0.65

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Monthly Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.4: Het. Treatment Effects w.r.t. Gender - Profits

	Monthly Profits			Log Monthly Profits		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	-338.989 (212.073)	-253.219 (274.538)	-722.436** (312.146)	0.097 (0.249)	-0.243 (0.263)	0.203 (0.400)
1=Male	500.478 (438.963)	276.556 (321.891)	413.441 (368.826)	-0.236 (0.163)	-0.170 (0.146)	-0.252* (0.148)
Account × Male=1	676.393* (361.248)	932.117 (605.779)	1128.918* (584.544)	-0.095 (0.301)	0.299 (0.355)	-0.139 (0.455)
Cash Treatment	-232.465 (244.256)	-390.397 (432.128)	-415.411 (296.259)	0.078 (0.277)	-0.045 (0.288)	0.064 (0.432)
Cash × Male=1	410.763 (542.157)	481.865 (933.240)	647.544 (683.047)	-0.254 (0.349)	-0.193 (0.391)	-0.230 (0.511)
Constant	1120.680*** (336.399)	665.788 (434.913)	1237.754*** (347.598)	2.912*** (0.385)	1.707*** (0.275)	2.429*** (0.420)
Observations	828	534	590	765	501	545
R squared	0.40	0.60	0.49	0.42	0.53	0.50

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Monthly Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.5: Het. Treatment Effects w.r.t. Gender - Inventories

	Inventories			Log Inventories		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	-1942.023 (1185.128)	-1666.602 (1773.342)	-2281.373 (1811.518)	0.330 (0.297)	0.357 (0.275)	0.322 (0.502)
1=Male	1105.785 (1977.169)	-595.793 (1966.200)	-565.548 (2006.453)	-0.282 (0.205)	-0.186 (0.185)	-0.417** (0.202)
Account \times Male=1	6121.514** (2993.999)	7831.638** (3303.041)	5049.989 (4513.140)	-0.197 (0.394)	0.215 (0.422)	-0.598 (0.595)
Cash Treatment	-986.131 (1398.802)	-809.807 (1630.414)	-1376.420 (2650.048)	-0.051 (0.292)	0.134 (0.251)	-0.296 (0.483)
Cash \times Male=1	-4578.801 (6362.427)	3269.615 (3819.721)	-9536.247 (10526.771)	-0.306 (0.444)	0.157 (0.418)	-0.582 (0.626)
Constant	4550.782 (3775.907)	3260.877 (2628.610)	5649.567 (4759.889)	2.055*** (0.500)	1.295*** (0.359)	1.987*** (0.548)
Observations	710	477	500	678	457	476
R squared	0.61	0.91	0.63	0.67	0.76	0.67

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Monthly Profits and capital stocks are measured in Ugandan Shilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.6: Het. Treatment Effects w.r.t. Gender - Sales

	Monthly Sales			Log Monthly Sales		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	894.952 (1826.096)	257.015 (2108.367)	1060.239 (1905.872)	-0.037 (0.271)	-0.107 (0.310)	-0.050 (0.353)
1=Male	-1756.575 (1698.508)	-1592.309 (1537.470)	-1863.007 (1728.719)	-0.137 (0.168)	-0.093 (0.146)	-0.156 (0.159)
Account \times Male=1	4174.302 (3747.444)	4255.613 (3160.647)	4854.764 (5501.406)	0.413 (0.327)	0.537 (0.365)	0.473 (0.440)
Cash Treatment	-2243.320 (1796.234)	-2388.795 (1843.752)	-3886.890 (2440.640)	-0.370 (0.271)	-0.417 (0.285)	-0.528 (0.363)
Cash \times Male=1	2967.963 (3131.661)	1174.215 (3099.070)	5549.573 (4496.421)	0.408 (0.362)	0.429 (0.367)	0.527 (0.484)
Constant	8730.259* (4547.850)	4466.280*** (1536.389)	9810.598 (6037.445)	3.179*** (0.428)	2.103*** (0.310)	2.621*** (0.465)
Observations	833	537	594	832	537	593
R squared	0.36	0.58	0.37	0.56	0.65	0.58

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Monthly Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.7: Het. Treatment Effects w.r.t. Gender - Any Savings

	Total Savings			Log Total Savings		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	25.688 (380.848)	131.150 (563.602)	233.088 (398.362)	0.398 (0.306)	0.512 (0.338)	0.364 (0.463)
1=Male	640.337 (560.990)	862.277* (377.203)	237.845 (561.494)	0.163 (0.205)	0.207 (0.175)	0.108 (0.188)
Account \times Male=1	871.195 (614.731)	823.032 (926.334)	860.717 (694.333)	-0.832** (0.376)	-0.943** (0.427)	-0.626 (0.548)
Cash Treatment	-320.986 (516.087)	-749.257 (789.967)	33.021 (612.328)	0.057 (0.378)	0.284 (0.415)	-0.208 (0.553)
Cash \times Male=1	853.255 (1008.115)	1385.683 (1236.958)	801.145 (1524.206)	-0.413 (0.483)	-0.761 (0.575)	0.033 (0.672)
Constant	879.968 (1391.253)	-992.997 (988.990)	2214.295 (2125.340)	3.456*** (0.627)	2.678*** (0.621)	2.959*** (0.688)
Observations	701	391	453	601	350	379
R squared	0.19	0.61	0.21	0.33	0.43	0.41

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Monthly Profits and capital stocks are measured in Ugandan Shilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.8: Het. Treatment Effects w.r.t. Gender - Business Savings

	Total Business Savings			Log Total Business Savings		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	-38.779 (300.642)	-365.060 (433.208)	262.183 (300.656)	-0.027 (0.479)	-0.900* (0.528)	1.155* (0.614)
1=Male	-43.694 (533.982)	47.778 (525.432)	-114.242 (531.894)	-0.274 (0.307)	-0.267 (0.293)	-0.219 (0.315)
Account \times Male=1	91.214 (378.165)	341.284 (526.391)	-108.999 (437.282)	-0.223 (0.596)	0.299 (0.671)	-0.944 (0.801)
Cash Treatment	58.900 (275.619)	-174.322 (420.511)	264.698 (280.116)	0.121 (0.301)	-0.207 (0.398)	0.917** (0.397)
Cash \times Male=1	-213.143 (406.250)	214.286 (508.664)	-562.105 (533.386)	-0.180 (0.463)	-0.051 (0.551)	-0.787 (0.655)
Constant	-686.858 (580.507)	-652.270 (494.878)	-595.967 (730.093)	3.022*** (0.687)	2.634*** (0.611)	2.449*** (0.772)
Observations	809	521	574	352	252	247
R squared	0.33	0.41	0.36	0.50	0.59	0.61

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Monthly Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Heterogeneity Analysis - Interaction with Education

We turn to providing results of interacting the treatment with a dummy for high education at baseline which changes to one if the respondent has finished her A-Levels or graduated from university. We expect the interaction effect between the treatment groups and the education dummy to be positive as higher educated business owners might use the cash transfer for more productive purposes.

While the interaction effects are mostly positive for capital stock as dependent variable, they are insignificant such that we cannot draw the conclusion that the effect of the cash transfer is higher depending on the kind of education achieved (see Table A.9). We also fail to find significant interaction results when we examine monthly profits and inventories (see Tables A.10 and A.11). However, entrepreneurs with high education significantly sell and save more in the long term (valid for both treatment groups, see Tables A.12 and A.13). For business savings, entrepreneurs with high education in both treatment arms save more than entrepreneurs with less education (see Table A.14).

Table A.9: Het. Treatment Effects w.r.t. High Education - Capital Stock

	Capital Stock			Log Capital Stock		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	513.720 (624.634)	271.720 (469.853)	650.563 (927.761)	0.101 (0.269)	-0.030 (0.287)	0.195 (0.323)
High Education	-10.639 (550.907)	-143.694 (318.847)	45.113 (573.403)	0.142 (0.250)	0.131 (0.159)	0.057 (0.233)
Account \times High Edu.	178.462 (1380.140)	1384.438 (1525.475)	-657.706 (1903.341)	0.310 (0.479)	0.185 (0.550)	0.470 (0.588)
Cash Treatment	-37.739 (562.344)	-74.591 (364.212)	-140.783 (816.592)	-0.158 (0.197)	-0.257 (0.180)	-0.119 (0.272)
Cash \times High Edu.	290.325 (1086.072)	5.989 (881.400)	496.430 (1655.306)	0.046 (0.412)	0.119 (0.347)	0.016 (0.571)
Constant	-509.149 (943.702)	-282.019 (540.352)	-535.842 (951.891)	1.458*** (0.455)	0.643** (0.296)	1.407*** (0.455)
Observations	851	547	609	851	547	609
R squared	0.34	0.62	0.34	0.63	0.78	0.65

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Monthly Profits and capital stocks are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.10: Het. Treatment Effects w.r.t. High Education - Profits

	Monthly Profits			Log Monthly Profits		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	36.499 (360.256)	470.939 (600.318)	-150.101 (530.303)	-0.079 (0.171)	-0.108 (0.227)	-0.104 (0.240)
High Education	-940.263** (391.585)	-368.881 (445.672)	-990.196** (387.364)	-0.358 (0.222)	-0.218 (0.170)	-0.466** (0.231)
Account \times High Edu.	225.671 (417.969)	-387.536 (672.751)	683.178 (624.588)	0.341 (0.364)	0.191 (0.370)	0.722 (0.596)
Cash Treatment	-97.898 (488.518)	-248.120 (821.715)	-169.526 (628.149)	-0.195 (0.219)	-0.247 (0.267)	-0.283 (0.304)
Cash \times High Edu.	298.279 (535.621)	465.562 (868.419)	398.457 (682.380)	0.291 (0.316)	0.296 (0.346)	0.491 (0.471)
Constant	1690.195*** (504.686)	998.987** (466.218)	1620.207*** (513.751)	2.948*** (0.362)	1.741*** (0.260)	2.411*** (0.389)
Observations	828	534	590	765	501	545
R squared	0.40	0.60	0.50	0.41	0.52	0.49

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Savings are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.11: Het. Treatment Effects w.r.t. High Education - Inventories

	Inventories			Log Inventories		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	9.495 (1372.384)	2774.057 (2092.205)	-1920.759 (1512.933)	0.189 (0.255)	0.581** (0.257)	-0.159 (0.340)
High Education	3322.639 (3017.272)	-1378.868 (3503.472)	4452.888 (3042.438)	0.219 (0.309)	0.318 (0.269)	0.060 (0.312)
Account \times High Edu.	7295.349 (5644.509)	563.339 (4461.758)	13399.708 (9814.827)	0.044 (0.472)	-0.444 (0.435)	0.486 (0.711)
Cash Treatment	584.145 (1372.753)	940.362 (2547.820)	209.583 (1652.516)	-0.108 (0.310)	0.264 (0.285)	-0.519 (0.418)
Cash \times High Edu.	-12194.112 (10068.021)	-249.281 (4419.509)	-20858.772 (17886.996)	-0.386 (0.478)	-0.261 (0.408)	-0.327 (0.660)
Constant	6926.850** (3125.603)	3578.404 (2280.937)	6739.636* (3716.267)	1.983*** (0.465)	1.269*** (0.344)	1.705*** (0.515)
Observations	710	477	500	678	457	476
R squared	0.62	0.91	0.64	0.67	0.76	0.67

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.12: Het. Treatment Effects w.r.t. High Education - Sales

	Monthly Sales			Log Monthly Sales		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	943.914 (1461.558)	3355.034* (1864.941)	-741.184 (1776.596)	0.060 (0.172)	0.182 (0.217)	-0.026 (0.207)
High Education	-2347.081 (2610.269)	-1965.091 (2485.565)	-2296.543 (2438.712)	-0.262 (0.248)	-0.201 (0.210)	-0.312 (0.231)
Account \times High Edu.	10853.459 (7396.598)	-1189.821 (3850.140)	22414.238* (13112.586)	0.653 (0.403)	0.176 (0.377)	1.202* (0.612)
Cash Treatment	112.566 (2566.639)	-1113.627 (1867.326)	-18.340 (3852.421)	-0.284 (0.217)	-0.203 (0.239)	-0.464* (0.279)
Cash \times High Edu.	-601.121 (3702.240)	-1577.557 (3539.972)	1584.889 (5327.710)	0.566 (0.342)	0.160 (0.330)	0.941* (0.485)
Constant	7527.556* (3889.621)	4019.458** (1718.206)	7321.404 (4479.959)	3.101*** (0.440)	2.133*** (0.335)	2.443*** (0.448)
Observations	833	537	594	832	537	593
R squared	0.37	0.59	0.40	0.56	0.65	0.58

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.13: Het. Treatment Effects w.r.t. High Education - Any Savings

	Total Savings			Log Total Savings		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	392.953 (422.558)	768.477 (654.033)	232.601 (468.405)	-0.204 (0.232)	-0.033 (0.281)	-0.322 (0.277)
High Education	412.437 (944.470)	780.387 (748.201)	-240.147 (1145.279)	0.681** (0.335)	0.433 (0.293)	0.606* (0.326)
Account \times High Edu.	596.428 (881.838)	-645.079 (1106.987)	2101.340* (1264.611)	0.175 (0.428)	-0.184 (0.419)	0.920 (0.615)
Cash Treatment	-456.559 (520.574)	-514.525 (777.737)	-528.602 (736.408)	-0.406 (0.249)	-0.116 (0.272)	-0.767** (0.342)
Cash \times High Edu.	1915.239 (1283.957)	1481.028 (1177.993)	3235.007 (2215.030)	0.598 (0.502)	-0.130 (0.605)	1.737*** (0.661)
Constant	918.782 (1211.217)	-668.469 (1062.247)	1802.234 (1738.579)	3.454*** (0.629)	2.848*** (0.632)	2.687*** (0.665)
Observations	701	391	453	601	350	379
R squared	0.19	0.61	0.22	0.32	0.42	0.42

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Table A.14: Het. Treatment Effects w.r.t. High Education - Business Savings

	Business Savings			Log Business Savings		
	Pooled (1)	Short Term (2)	Long Term (3)	Pooled (4)	Short Term (5)	Long Term (6)
Account Treatment	-15.750 (197.784)	-262.153 (263.215)	154.585 (239.965)	-0.357 (0.347)	-1.224*** (0.400)	0.447 (0.529)
High Education	568.009 (659.229)	464.514 (696.653)	556.123 (689.490)	0.506 (0.395)	0.616 (0.373)	0.245 (0.405)
Account \times High Edu.	108.023 (529.883)	340.996 (631.182)	150.880 (718.921)	0.657 (0.561)	1.344** (0.646)	0.413 (0.837)
Cash Treatment	-207.005 (167.431)	-329.555 (223.666)	-181.912 (208.583)	-0.077 (0.295)	-0.673* (0.383)	0.401 (0.447)
Cash \times High Edu.	325.180 (515.373)	745.238 (558.092)	176.333 (766.699)	0.366 (0.519)	0.940* (0.562)	0.240 (0.805)
Constant	-777.876 (474.052)	-811.229** (394.794)	-690.136 (554.818)	2.610*** (0.647)	2.135*** (0.583)	2.222*** (0.712)
Observations	809	521	574	352	252	247
R squared	0.33	0.41	0.36	0.49	0.59	0.60

Note: Regressions control for the baseline value of the dependent variable, wave fixed effects, the interaction between wave and the heterogeneity measure, and other baseline covariates. Robust standard errors are in parentheses, clustered at the firm level. *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively. Inventories and sales are measured in Ugandan Schilling, deflated by the CPI to reflect 2011 price levels. All values are given in 1000 UGX. The sample trims the top and bottom 5 percent of changes in profits over time.

Appendices B and C - Appendices for Chapter 3

Appendix B

Table B.1: Financial Literacy Questions and Response Options

Concepts	No. of Questions	Question(s)	Answer Options
Risk Diversification	1	Suppose you have some money. Is it safer to put your money into one business or investment, or to put your money into multiple businesses or investments?	a) one business or investment; b) multiple businesses or investments; c) don't know; d) refuse to answer
Inflation	1	Suppose over the next 10 years the prices of things you buy double. If your income also doubles, will you be able to buy less than you buy today, the same as you can buy today, or more than you can buy today?	a) less; b) the same; c) more; d) don't know; e) refuse
Interest	1	Suppose you need to borrow \$100. Which is the lower amount to pay back: \$105 or \$100 plus three percent?	a) 105 US dollars; b) 100 US dollars plus three percent; c) don't know; d) refuse
Interest Compound-ing	2	Suppose you put money in the bank for two years and the bank agrees to add 15 percent per year to your account. Will the bank add more money to your account in the second year than it did in the first year, or will it add the same amount of money in both years?	a) more; b) the same; c) don't know; d) refuse
		Suppose you had \$100 in a savings account and the bank adds 10 percent per year to the account. How much money would you have in the account after five years if you did not remove any money from the account?	a) more than 150 US dollars; b) exactly 150 US dollars; c) less than 150 US dollars; d) don't know; refused

Financial Literacy Questions and Response Options - continued.

Concepts	No. of Questions	Question(s)	Answer Options
Financial Literacy	1	Proportion of people that can answer questions on 3 out of 4 concepts correctly.	
Financial Literacy Men, Women	1	Proportion of men/women that can answer questions on 3 out of 4 questions correctly.	

Notes: This table reports the four financial literacy concepts, the corresponding questions and the answer options. These questions are used to generate the proportion of the population that is regarded as financially literate in a country. If a person can answer questions on three out of four questions correctly, this person can be regarded as financially literate.

Table B.2: Control Variables Summary Statistics and Sources

	Mean	SD	Min	Max	Count	Description	Source
GDP per capita	18230.11	17862.26	711	91368	136	GDP per capita purchasing power parity, constant 2011 USD	World Bank, World Development Indicators 2014
Population share between 15-64 years	63.68	6.85	47	85	141	Proportion of the population that is between 15 and 64 year old	World Bank, World Development Indicators 2014
Secondary education	0.51	0.16	0	1	142	Proportion of population that has completed secondary school	World Bank, Global Findex 2014
Tertiary education	0.16	0.14	0	1	142	Proportion of population that has completed tertiary education	World Bank, Global Findex 2014
Private credit to GDP	60.50	48.06	4	260	126	Private credit by deposit money banks and other financial institutions to GDP, designed to measure financial depth	World Bank, Global Financial Development 2014
Strength of legal rights index	5.14	2.89	0	12	141	Strength of legal rights index measures the degree to which collateral and bankruptcy laws protect the rights of borrowers and lenders and thus facilitate lending.	World Bank, World Development Indicators 2014
Ease of doing business index	85.39	55.41	1	187	140	Ease of doing business ranks economies from 1 to 190, with first place being the best. A high ranking (a low numerical rank) means that the regulatory environment is conducive to business operation.	World Bank, World Development Indicators 2014
Political Risk Rating ICRG 2012	64.57	12.77	23	90	121	Measures the political stability of a country along 12 dimensions, e.g. corruption, government stability, and bureaucracy quality.	ICRG 2012

Control Variables Summary Statistics and Sources - continued

	Mean	SD	Min	Max	Count	Description	Source
Branches of commercial banks per 1,000 km ²	37.55	137.83	0	1382	129	Number of branches per 1000 km ²	IMF, Financial Access Survey 2014
Automated Teller Machines (ATMs) per 1,000 km ²	89.91	362.60	0	3870	129	ATMs per 1000 km ²	IMF, Financial Access Survey 2014

Table B.3: Correlations between Control Variables

	Log GDP p.c. (PPP)	Population Share between 15 and 64	Secondary Education	Tertiary Education	Private Credit to GDP	Bank Branches per 1000 km ²	Strength of Legal Rights Index	Ease of Doing Business Index
Log GDP p.c. (PPP)	1							
Population Share between 15 and 64	0.718***	1						
Secondary Education	0.500***	0.293***	1					
Tertiary Education	0.708***	0.488***	0.277***	1				
Private Credit to GDP	0.629***	0.491***	0.0201	0.380***	1			
Bank Branches per 1000 km ²	0.292***	0.221**	-0.276***	-0.0551	0.381***	1		
Strength of Legal Rights Index	0.0153	0.0224	-0.0193	0.0702	0.165*	0.0429	1	
Ease of Doing Business Index	-0.773***	-0.673***	-0.329***	-0.617***	-0.626***	-0.228***	-0.366***	1

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: List of Countries in OLS and IV Regressions

Country	OLS Sample	IV Sample	Country	OLS Sample	IV Sample	Country	OLS Sample	IV Sample
Afghanistan	x		Greece	x	x	Pakistan	x	
Albania	x	x	Guatemala	x	x	Panama	x	x
Algeria	x	x	Guinea	x		Peru	x	x
Angola	x		Honduras	x	x	Philippines	x	x
Argentina	x	x	Hungary	x	x	Poland	x	x
Armenia	x	x	India	x		Portugal	x	x
Australia	x	x	Indonesia	x	x	Romania	x	
Austria	x	x	Iraq	x		Russian Federation	x	x
Azerbaijan	x	x	Ireland	x	x	Saudi Arabia	x	x
Bangladesh	x	x	Israel	x	x	Senegal	x	
Belgium	x	x	Italy	x	x	Serbia	x	
Belize	x		Jamaica	x		Slovak Republic	x	x
Benin	x		Japan	x	x	Slovenia	x	x
Bhutan	x		Jordan	x	x	South Africa	x	x
Bolivia	x	x	Kazakhstan	x	x	Spain	x	x
Bosnia and Herz.	x	x	Kenya	x	x	Sri Lanka	x	
Botswana	x	x	Korea, Rep.	x	x	Sudan	x	
Brazil	x	x	Kuwait	x	x	Sweden	x	x
Bulgaria	x	x	Kyrgyz Rep.	x	x	Switzerland	x	x
Burundi	x		Latvia	x	x	Tanzania	x	x
Cambodia	x		Lebanon	x	x	Thailand	x	x
Cameroon	x	x	Luxembourg	x	x	Togo	x	x
Chad	x	x	Macedonia, FYR	x	x	Tunisia	x	x
Chile	x	x	Madagascar	x	x	Turkey	x	x
China	x	x	Malawi	x	x	Uganda	x	x
Colombia	x	x	Malaysia	x	x	Ukraine	x	x
Congo, Dem. Rep.	x		Mali	x	x	United Arab Emirates	x	
Congo, Rep.	x		Malta	x		United States	x	x
Costa Rica	x	x	Mauretania	x	x	Uruquay	x	x
Cote d'Ivoire	x		Mauritius	x	x	Venezuela, RB	x	x
Croatia	x	x	Mexico	x	x	Vietnam	x	
Cyprus	x	x	Moldova	x	x	West Bank and Gaza	x	
Czech Republic	x	x	Mongolia	x		Yemen, Rep.	x	x
Denmark	x	x	Montenegro	x	x	Zambia	x	x
Dom. Republic	x	x	Namibia	x	x			
Ecuador	x	x	Nepal	x		Total	119	93
Egypt, Arab Rep.	x	x	Netherlands	x	x			
El Salvador	x	x	New Zealand	x	x			
Estonia	x	x	Nicaragua	x	x			
Finland	x	x	Niger	x	x			
France	x	x	Nigeria	x	x			
Georgia	x	x						
Germany	x	x						
Ghana								

Table B.5: Correlations between Financial Literacy and Outcome Variables

	Financial Literacy	Risk Diversification	Inflation	Interest Compounding	Account Ownership	Debit Card Ownership	Saved at Formal Fin. Institution	Used Debit Card in the Last Year
Financial Literacy	1							
Risk Diversification	0.725***	1						
Inflation	0.628***	0.120	1					
Interest Compounding	0.745***	0.244***	0.577***	1				
Account Ownership	0.696***	0.595***	0.182**	0.324***	1			
Debit Card Ownership	0.652***	0.422***	0.462***	0.548***	0.295***	1		
Saved at Formal Fin. Institution	0.690***	0.481***	0.490***	0.554***	0.939***	0.960***	1	
Used Debit Card in the Last Year	0.753***	0.615***	0.374***	0.503***	0.831***	0.839***	0.839***	1
	0.739***	0.553***	0.466***	0.567***	0.874***	0.960***	0.839***	1

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Basis for Imputations for Numeracy in Primary School

	(1) Numeracy in Primary School
Numeracy in Secondary School	0.749*** (0.072)
Constant	2.162 (3.754)
R-squared	0.658
Observations	58

Note: This table shows the relationship that is the basis for our imputation of numeracy in primary school using numeracy in secondary schools. If numeracy in primary school is missing, but numeracy in secondary school is available the following equation was used to generate an imputation for numeracy in primary school. Numeracy in primary school = $2.162 + 0.749$ numeracy in secondary school.

Table B.7: First Stage Regressions for IV Results

	(1) Financial Literacy
Math Education in Primary School	0.539*** (0.173)
Log GDP p.c. (PPP)	9.982*** (1.801)
Population Share between 15 and 64	-1.248*** (0.204)
Secondary Education	-0.014 (0.085)
Tertiary Education	0.043 (0.102)
Private Credit to GDP	-0.001 (0.028)
Bank Branches per 1000 km ²	-0.025 (0.031)
Strength of Legal Rights Index	0.796* (0.404)
Ease of Doing Business Index	-0.007 (0.041)
Constant	0.529 (19.763)
Observations	93
F-Test for First Stage Regression	15.24
F-Test for Weak Instruments	9.67

Note: This table reports the first stage regression of the IV regressions shown in this paper with robust standard errors in parentheses. The F-statistics reports the F-stat for the first stage regression. The F-test for weak instruments denotes passing the Stock-Yogo test at 15%. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table B.8: Financial Literacy and Access to Financial Services - OLS and IV Results

	Account Ownership		Debit Card Ownership	
	(1)	(2)	(3)	(4)
Financial Literacy	0.471*** (0.158)	1.885*** (0.650)	0.535*** (0.184)	1.636*** (0.493)
Log GDP p.c. (PPP)	16.412*** (3.183)	2.686 (7.839)	16.925*** (3.073)	6.230 (5.576)
Population Share between 15 and 64	-0.701* (0.390)	1.063 (0.875)	-0.754** (0.365)	0.621 (0.679)
Secondary Education	-0.046 (0.135)	-0.089 (0.170)	0.023 (0.136)	-0.011 (0.161)
Tertiary Education	-0.201 (0.152)	-0.364** (0.178)	-0.106 (0.177)	-0.233 (0.175)
Private Credit to GDP	0.140*** (0.031)	0.119** (0.046)	0.092** (0.045)	0.076* (0.042)
Bank Branches per 1000 km ²	0.055 (0.043)	0.064 (0.058)	0.023 (0.047)	0.030 (0.054)
Strength of Legal Rights Index	0.175 (0.625)	-1.078 (0.971)	-0.187 (0.640)	-1.162 (0.772)
Ease of Doing Business Index	-0.097* (0.055)	-0.048 (0.091)	-0.102* (0.061)	-0.064 (0.084)
Constant	-67.524** (28.475)	-96.144** (40.375)	-85.607*** (27.491)	-107.907*** (36.845)
R-squared	0.824	0.640	0.811	0.702
Observations	93	93	93	93

Note: The table reports OLS and IV regression results with robust standard errors in parentheses. Columns (1) and (2) show results for the proportion of people that have a bank account. Columns (3) and (4) show results for the proportion that has a debit card as the outcome variable. Numeracy in primary schools acts as an instrument in these regressions. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table B.9: Financial Literacy and Use of Financial Services - OLS and IV Results

	Saved at Formal Fin. Institution		Used Debit Card in the Last Year	
	(1)	(2)	(3)	(4)
Financial Literacy	0.537*** (0.108)	1.117*** (0.387)	0.747*** (0.183)	1.630*** (0.478)
Log GDP p.c. (PPP)	7.020*** (2.129)	1.388 (4.509)	14.092*** (2.913)	5.520 (4.814)
Population Share between 15 and 64	-0.838*** (0.303)	-0.114 (0.568)	-0.776** (0.356)	0.326 (0.698)
Secondary Education	-0.024 (0.109)	-0.042 (0.120)	0.008 (0.127)	-0.019 (0.142)
Tertiary Education	0.008 (0.134)	-0.059 (0.139)	0.132 (0.170)	0.030 (0.172)
Private Credit to GDP	0.110** (0.046)	0.101*** (0.038)	0.043 (0.043)	0.030 (0.040)
Bank Branches per 1000 km ²	0.071* (0.036)	0.075** (0.038)	0.011 (0.055)	0.016 (0.055)
Strength of Legal Rights Index	0.028 (0.538)	-0.486 (0.694)	0.171 (0.626)	-0.611 (0.755)
Ease of Doing Business Index	-0.031 (0.048)	-0.011 (0.056)	-0.069 (0.069)	-0.038 (0.086)
Constant	-13.922 (22.582)	-25.666 (25.695)	-81.949** (33.905)	-99.822** (41.527)
R-squared	0.745	0.675	0.771	0.698
Observations	93	93	93	93

Note: The table reports OLS and IV regression results with robust standard errors in parentheses. Columns (1) and (2) show results for the proportion of people that have a bank account. Columns (3) and (4) show results for the proportion that has a debit card as the outcome variable. Numeracy in primary schools acts as an instrument in these regressions. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table B.10: First Stage Regression for Placebo IV
Results – Literacy as an Instrument

	(1) Financial Literacy
Literacy Education in Primary School	0.191 (0.235)
Log GDP p.c. (PPP)	9.866*** (2.602)
Population Share between 15 and 64	-1.430*** (0.247)
Secondary Education	-0.017 (0.100)
Tertiary Education	0.083 (0.120)
Private Credit to GDP	0.002 (0.027)
Bank Branches per 1000 km ²	-0.032 (0.032)
Strength of Legal Rights Index	0.708 (0.508)
Ease of Doing Business Index	-0.055 (0.044)
Constant	31.132 (22.951)
Observations	77
F-Test for First Stage Regression	11.02
F-Test for Weak Instruments	0.66

Note: This table reports the first stage regression of the IV regressions using literacy instead of numeracy as an instrument with robust standard errors in parentheses. The F-statistics reports the F-stat for the first stage regression. The F-test for weak instruments denotes not passing the Stock-Yogo test. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table B.11: First Stage Regression for Placebo IV Results – Literacy as an Instrument

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	1.667 (2.198)	1.913 (2.404)	0.896 (1.091)	2.635 (2.803)
Log GDP p.c. (PPP)	3.292 (23.678)	2.720 (25.505)	2.932 (11.779)	−4.387 (29.556)
Population Share between 15 and 64	0.835 (3.218)	1.064 (3.461)	−0.416 (1.571)	1.875 (4.031)
Secondary Education	0.004 (0.189)	0.055 (0.218)	0.081 (0.119)	0.018 (0.244)
Tertiary Education	−0.240 (0.266)	−0.231 (0.292)	0.106 (0.162)	−0.015 (0.334)
Private Credit to GDP	0.122*** (0.041)	0.079* (0.047)	0.098** (0.044)	0.036 (0.051)
Bank Branches per 1000 km ²	0.121 (0.091)	0.088 (0.109)	0.106** (0.049)	0.064 (0.122)
Strength of Legal Rights Index	−1.093 (1.786)	−1.240 (1.892)	−0.227 (1.058)	−1.141 (2.217)
Ease of Doing Business Index	−0.038 (0.175)	−0.025 (0.193)	0.009 (0.090)	0.054 (0.234)
Constant	−88.998 (85.466)	−121.919 (92.652)	−25.625 (42.570)	−151.750 (113.761)
R-squared	0.679	0.623	0.736	0.448
Observations	77	77	77	77

Note: The table reports IV regression results with robust standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. The imputed literacy rate in primary school is used as an instrument for financial literacy.

Table B.12: Financial Literacy and Access to Finance - OLS Results,
Proportion of Banks that are State Owned

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.453*** (0.153)	0.556*** (0.187)	0.529*** (0.097)	0.674*** (0.192)
Log GDP p.c. (PPP)	14.734*** (2.847)	15.055*** (2.660)	6.635*** (1.883)	12.642*** (2.850)
Population Share between 15 and 64	-0.891** (0.419)	-0.841** (0.390)	-0.729** (0.289)	-1.090*** (0.403)
Secondary Education	0.206 (0.134)	0.181 (0.136)	0.069 (0.089)	0.128 (0.125)
Tertiary Education	-0.083 (0.163)	-0.108 (0.203)	0.045 (0.154)	0.181 (0.199)
Private Credit to GDP	0.145*** (0.037)	0.090* (0.049)	0.088* (0.048)	0.068 (0.049)
Bank Branches per 1000 km ²	0.065*** (0.021)	0.043** (0.017)	0.040 (0.024)	0.039* (0.021)
Strength of Legal Rights Index	0.289 (0.610)	-0.155 (0.536)	0.276 (0.506)	0.184 (0.611)
Ease of Doing Business Index	-0.066 (0.050) (9.995)	-0.104 (0.069) (9.411)	-0.015 (0.051) (6.737)	-0.082 (0.086) (9.744)
Constant	-58.035* (32.604)	-72.440** (34.607)	-22.615 (25.537)	-53.790 (43.954)
R-squared	0.829	0.826	0.740	0.784
Observations	86	86	86	86

Note: The table reports OLS regression results with robust standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table B.13: Financial Literacy and Access to Finance - OLS Results,
Proportion of State Owned Assets Below the Median Only

	Account Ownership (1)	Debit Card Ownership (2)	Saved at Formal Fin. Institution (3)	Used Debit Card in the Last Year (4)
Financial Literacy	0.425** (0.196)	0.523* (0.283)	0.528*** (0.141)	0.785*** (0.280)
Log GDP p.c. (PPP)	16.894** (6.781)	17.622*** (5.504)	10.335*** (2.930)	14.960*** (3.510)
Population Share between 15 and 64	-1.130* (0.629)	-0.928 (0.551)	-0.960** (0.408)	-0.908* (0.491)
Secondary Education	0.199 (0.233)	0.082 (0.193)	0.189* (0.112)	-0.065 (0.167)
Tertiary Education	-0.045 (0.291)	-0.233 (0.343)	0.119 (0.174)	-0.170 (0.332)
Private Credit to GDP	0.090** (0.042)	0.036 (0.054)	0.042 (0.048)	0.043 (0.055)
Bank Branches per 1000 km ²	0.047** (0.019)	0.040** (0.018)	0.011 (0.014)	0.019 (0.019)
Strength of Legal Rights Index	0.201 (0.916)	-0.215 (0.718)	0.094 (0.599)	-0.719 (0.661)
Ease of Doing Business Index	-0.075 (0.110)	-0.148 (0.107)	0.014 (0.066)	-0.167* (0.097)
Constant	-55.456 (74.413)	-74.366 (55.763)	-46.665 (40.779)	-60.373 (47.842)
R-squared	0.849	0.840	0.849	0.831
Observations	45	45	45	45

Note: The table reports OLS regression results with robust standard errors in brackets. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. The regressions only include countries with the proportion of assets held at state owned banks below the median.

Table B.14: Financial Literacy and Access to Finance - IV Results,
Controlling for Government Expenditure and Total Education Expenditure

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	1.899** (0.713)	1.721*** (0.575)	1.132*** (0.337)	1.298** (0.502)
Log GDP p.c. (PPP)	0.426 (8.734)	2.364 (7.523)	-0.672 (3.722)	7.467 (5.834)
Population Share between 15 and 64	0.243 (0.955)	0.716 (1.103)	-1.102** (0.532)	0.351 (1.032)
Secondary Education	0.063 (0.303)	-0.294 (0.444)	0.508*** (0.176)	-0.497 (0.426)
Tertiary Education	-0.303 (0.250)	-0.457 (0.369)	0.228 (0.148)	-0.299 (0.364)
Private Credit to GDP	0.155*** (0.051)	0.097* (0.056)	0.075** (0.031)	0.041 (0.052)
Bank Branches per 1000 km ²	0.105 (0.075)	0.034 (0.087)	0.126*** (0.040)	-0.064 (0.083)
Strength of Legal Rights Index	-1.892* (1.010)	-1.976** (0.880)	-1.385* (0.688)	-1.021 (0.944)
Ease of Doing Business Index	-0.120 (0.124)	-0.212* (0.109)	-0.094 (0.061)	-0.200** (0.098)
Gov. Consumption Expenditure	-0.728 (1.013)	0.095 (0.799)	-0.447 (0.433)	0.694 (0.726)
Gov. Spending On Education	0.180 (0.804)	-0.337 (0.899)	1.101** (0.495)	-0.743 (0.872)
Constant	-17.877 (63.478)	-45.824 (57.357)	23.609 (26.374)	-59.824 (46.379)
R-squared	0.786	0.817	0.866	0.835
Observations	49	49	49	49

Note: The table reports IV regression results with robust standard errors in parentheses. Government consumption expenditure is government expenditure on consumption as a percent of GDP, whereas government expenditure on education is percent of government expenditure on education as a percent to total government expenditure. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table B.15: First Stage Regression - Using Numeracy of Individuals Born in 1960 as Instrument

	(1) Financial Literacy
Numeracy in 1960	0.378** (0.169)
Log GDP p.c. (PPP)	8.560*** (2.348)
Population Share between 15 and 64	-1.385*** (0.297)
Secondary Education	0.004 (0.125)
Tertiary Education	0.065 (0.162)
Private Credit to GDP	0.060 (0.042)
Bank Branches per 1000 km ²	-0.094 (0.057)
Strength of Legal Rights Index	0.836 (0.515)
Ease of Doing Business Index	-0.011 (0.064)
Constant	3.163 (33.197)
R-squared	0.720
Observations	47
F-test for First Stage	15.55
F-test for Weak Instruments	4.145

Note: The table reports the first stage of the IV regression using historic numeracy as an instrument. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table B.16: Financial Literacy and Access to Finance - IV Results, Using 1960s Numeracy

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	1.452 (0.918)	0.647 (0.594)	0.281 (0.508)	0.111 (0.475)
Log GDP p.c. (PPP)	0.532 (8.603)	7.265 (5.103)	3.509 (4.052)	11.041** (4.727)
Population Share between 15 and 64	0.733 (1.243)	-0.777 (0.794)	-0.740 (0.685)	-1.865** (0.738)
Secondary Education	0.124 (0.240)	0.205 (0.158)	0.047 (0.149)	0.121 (0.176)
Tertiary Education	-0.439 (0.273)	-0.361 (0.216)	0.011 (0.187)	0.097 (0.285)
Private Credit to GDP	0.117 (0.103)	0.200** (0.077)	0.254*** (0.078)	0.150** (0.073)
Bank Branches per 1000 km ²	0.138 (0.143)	-0.032 (0.080)	-0.015 (0.097)	-0.167 (0.120)
Strength of Legal Rights Index	-1.291 (1.277)	-0.727 (0.763)	0.036 (0.599)	0.086 (0.775)
Ease of Doing Business Index	-0.137 (0.114)	-0.223*** (0.076)	-0.044 (0.062)	-0.256*** (0.091)
Constant	-37.762 (61.571)	5.147 (36.740)	14.871 (34.478)	47.005 (44.259)
R-squared	0.769	0.904	0.832	0.857
Observations	47	47	47	47

Note: The table reports IV regression results with robust standard errors in parentheses. Numeracy in 1960 with imputations from 1950 is used as an instrument ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Appendix C

This appendix on robustness tests addresses four issues: It shows further evidence for the causal relationship between financial literacy and financial inclusion (Section C.1), it shows that our main findings also hold for various sub-groups within countries (Section C.2), that they are robust to various changes in variable definitions or considerations (Section C.3), and that different estimation techniques confirm results (Section C.4).

C.1 Within-Model Generated Instruments.

In addition to running conventional IV-regressions, we also apply the recent approach developed by Lewbel (2012) to examine causality. We here explain its basic intuition. Instead of relying on external instruments and needing the exclusion restriction to hold, this method uses instruments that are generated from within the model. For this to be possible, two conditions need to hold: First, the error term of the first stage of the potentially endogenous variable (financial literacy in our case) on (a sub-set of) the potentially exogenous regressors Z , has to be heteroscedastic, i.e. the error term of the first stage regression is $Cov(Z, \epsilon^2) \neq 0$. The second condition that needs to hold for the Lewbel (2012) model is that the products of the idiosyncratic errors of the first and second stage are uncorrelated with the subset of variables Z used in the first stage regression, hence $Cov(Z, \epsilon, u) = 0$. To check that the first condition holds we run the Breusch-Pagan test for heteroscedasticity on the first stage regression and homoscedasticity is rejected at 1%. The second condition holds by assumption. If these two conditions hold, instruments are then generated using $(Z - \bar{Z})\hat{\epsilon}$ where \bar{Z} is the mean of Z and $\hat{\epsilon}$ is the estimated residuals in the first stage. Hence there is the same number of instruments as exogenous variables are included in the first stage. The generated instruments can be used by themselves or in combination with traditional instruments that are taken from outside the model.

We do not run the regression with the same full set of control variables as in the other regressions instead focus on a subset. We here only include those variables that can be argued to be determined outside the model i.e. the proportion of the population that has completed secondary school, the proportion of the population that has completed tertiary education, the number of banks per 1000 km², strength of legal rights and ease of doing business.

Results of the Lewbel model examining the potential impact of financial literacy on access to finance are shown in Table C.1, whereas the results regarding the use of

financial services are shown in Table C.2. Both tables present regression results employing numeracy in primary school as an instrument, using the generated instruments only and applying a combination of external and generated instruments.

The results confirm our earlier finding that financial literacy has a positive and significant impact on all our measures of financial inclusion that we use in this paper. Furthermore, the positive and significant coefficient, no matter whether we use just the generated instrument or a combination of generated and traditional instrument, confirm that the relationship between financial literacy and financial inclusion is causal.

Another advantage of the Lewbel model is that we can test for overidentification, which is not possible in IV regressions with only one instrument. The Hansen-J-statistic shows that overidentification is not a problem in our regression. The tables also provide the F-statistic of the first stage regression, which is consistently above 10 and so confirms that the instruments are not weak.

C.2 The Effect of Financial Literacy for Various Sub-Groups

It is possible that financial literacy works differently for various groups across and within countries. Thus, we perform three kinds of analyses to test whether the overall results are robust and can be applied for policy purposes in various kinds of circumstances.

Interactions With Different Income Levels Across Countries. We expect that the relationship between financial literacy and access to financial services will be stronger for lower income countries. There are several reasons that make us form this hypothesis. First, on an individual level as well as in our descriptive statistics we can see that high income is correlated with high financial literacy. The marginal effect of financial literacy in poor countries may hence be larger. Kaiser and Menkhoff (2017), indeed, also find that financial literacy trainings are less effective when baseline levels of financial literacy are high. At the same time, as we are here looking at simple financial services, access and use of financial services is already fairly high in higher income countries, there is therefore less “room for improvement”. For these reasons we expect the marginal effect of financial literacy to be higher in low GDP per capita countries.

In order to test if a larger proportion of the population being financially literate has heterogeneous effects depending on the income level of the population, we introduce an interaction term between financial literacy and GDP per capita in the regressions described above. Table C.3 shows results of OLS regressions that include the interaction term. The outcome variables are the same as above. Columns (1) and (2) show results

for access to finance, whereas columns (3) to (4) show the results for use of financial services. The dependent variables in the table were centered and hence the interaction term shows the effect of an increase in financial literacy at the mean GDP per capita. To increase clarity and give the effect of a change in financial literacy at all levels of GDP per capita we include figures that show the average marginal effect of financial literacy at each level of GDP. These can be seen in Figure C.1 - there is one picture for each outcome variable.

Table C.3 and Figure C.1 demonstrate that increasing the level of financial literacy of the population would have the strongest effect on account ownership in countries that have lower levels of GDP per capita, as hypothesized above. Increasing financial literacy would have the largest marginal effect on account ownership at levels of GDP per capita below the mean. The interaction term between financial literacy and log GDP per capita is negative but not significant, indicating that the effect of financial literacy on debit card ownership is similar at different levels of GDP.

Interestingly, the interaction between financial literacy and our measures of use of financial services is positive rather than negative. Further, Figure C.1 clearly shows that the average marginal effect of financial literacy is higher at higher levels of GDP. This pattern can also be seen when looking at the proportion of people that has used a debit card during the last year.

In this section we learn that increasing the level of financial literacy in the population has heterogeneous effects for countries with different levels of GDP per capita. Interestingly, the effect of increasing financial literacy on access to finance would have the largest effect in countries with low levels of GDP per capita. The effect of increased financial literacy on use of financial services, however, is larger at higher levels of GDP per capita.

Interactions With Different Levels on Bank Branch Penetration. In a next step we look at the interaction between financial literacy and physical access to financial services by introducing an interaction term, analogous to the procedure in Section ?? and the one shown above. The results are shown in Table C.4. Graphical presentations of the average marginal effects of financial literacy at different levels of bank branch penetration (again centered at their means) are presented in Figure C.2.

The patterns that we see in these regressions are different from the patterns that we find above. The interaction term between financial literacy and bank branch penetration is insignificant and close to zero for all our measures of financial inclusion. This shows that the average marginal effect of financial literacy is constant for all levels of bank branch penetration. However, departing from the mean there is a positive yet de-

creasing marginal effect of financial literacy on bank account ownership for lower bank branch penetration. Also, the effect of financial literacy on the percentage of people that saved during the last year is positive and increasing (Figure C.2). This makes sense since financial literacy should be more effective where actual banking facilities require people to apply good financial knowledge.

Different Income Groups Within Countries. We now analyze whether the link between financial literacy and financial inclusion is stronger for certain groups of the population than for other ones. To do this, we use data that show the proportion of the poorest 40% and richest 60% of the country that can answer questions on three out of four financial literacy concepts correctly. As the outcome variable we use respective measures of financial inclusion, i.e. also of the poorest 40% and richest 60% of the population. We rerun the regressions above, but this time broken down by within country income groups. Results are presented in Table C.5. They show that the coefficient on the level of financial literacy of the richest 60% is larger than the coefficient on the level of financial literacy of the poorest 40% of the population. This may indicate at first sight that the link between financial literacy and financial inclusion is tentatively stronger for the richer part of the population. However, when we test the difference between the coefficients, there is no significant difference between the two regression coefficients.

Excluding Islamic Countries. Countries in which the majority of the population follows the Islamic religion, may have different levels of financial literacy. This may especially apply to the questions on interest and interest compounding due to the prohibition of interest in Islamic law. It is possible that these questions are answered particularly badly in majority Muslim countries, but that people here have good financial literacy regarding other financial topics. This would introduce measurement error into our regression. To check this, we ran our main models shown in Tables ?? and ?? again, excluding all countries where more than half of the population is Muslim. Results are shown in Table C.6 and Table C.7. This shows no change to our main model. All results remain significant and effect sizes are of a similar size.

C.3 Changes in Variable Definitions and Further Considerations

We here test whether the main results change when (i) looking at the effect of financial literacy on inclusion with respect to borrowing or (ii) high frequency of account use, (iii) adding political risk, ATM penetration and cost of bank account as further control variables, (iv) omitting the share of people aged 15-64 and secondary education from

the estimation, (v) using disaggregated financial literacy items as variables of interest, and (vi) modifying the definition of income.

Financial Literacy and Borrowing. In this study we deliberately focus on the relationship of financial literacy and financial inclusion on the asset side of the balance sheet. As it is harder to determine the desired level of financial inclusion on the borrowing side, we do not study this form of inclusion in the main text (Schicks, 2014). However, we look at the relationship between inclusion w.r.t to borrowing and financial literacy here, by running our regressions with the proportion of the population that borrowed from a formal financial institution, the proportion that borrowed from an informal financial institution and the proportion that has used a credit card within the last year. Results for OLS and IV regressions are presented in Table C.8. The results confirm the link between financial literacy and financial inclusion, also on the borrowing side: There is no significant relationship between financial literacy and borrowing at a formal financial institution. We find, however, a negative and significant relationship between financial literacy and the proportion of people that borrowed from an informal financial institution, indicating that there is a link between financial literacy and financial inclusion on the borrowing side. Lastly, the link between financial literacy and the proportion that used a credit card in the last year is positive and significant. All the OLS results are confirmed by the IV regressions.

Financial Literacy and High Frequency of Account Use. Our measures of financial access and use do not take into account the intensity to which certain financial services are actually utilized. The variable “high frequency of account use” alleviates this constraint by indicating the share of people that took money out of a formal bank account three or more times in a typical month. OLS and IV regression results are presented in Table C.9. Estimation results are positively related and significant at the one percent level. The effect of a one percentage point higher share of adults being financial literate translates into an about 0.59 percentage point higher share of people using their account intensively. Interestingly, the coefficient for tertiary education turns significant indicating that above and beyond financial literacy, adults with higher education use their accounts more frequently.

Considering Other Control Variables. Although we already control for a number of variables in our main regression, we here expand the number of control variables and see if our results still hold. The results of the exercise are shown in Table C.10 for account ownership as the outcome variable. First, we introduce a political risk index into the regressions. This considerably reduces the sample size. The relationship between financial literacy and account ownership remains significant. Next, we intro-

duce ATMs per 1000 km², as an additional measure of physical access to finance into the regressions; again the coefficient on the relationship between financial literacy and account ownership remains positive and significant.

Fees levied on holding and using financial products constitute barriers to accessing finance. In fact, data from the Micro Findex data base (Demirgüç-Kunt and Klapper, 2012) show that 29% of respondents without a bank account state the high costs hindering them to acquire one. Thus, we consider the annual cost of checking accounts as additional control variable in our OLS regressions. As we did not get access to more recent World Bank data, as a second-best approach we use data presented in Beck et al. (2008) that are available for 68 countries and were collected in 2004. In order to enlarge the sample size, we impute the cost data for 43 other countries by estimating the annual fees of a checking account with the following explanatory variables: the share of population above 15 years and financial institutional variables such as private credit to GDP, bank return on assets, and bank return on equity.

Using this information as proxy for the cost of financial services and products, we re-run the OLS regressions. The results are shown in Table C.10, column (3). Notably, there is no great difference in the point estimate or significance level of financial literacy compared to the regressions run without the cost data.

Column (4) shows results from regressions only with countries for which the original bank account cost data by Beck et al. (2008) are available. The sample size shrinks in these estimations and so the significant effect of financial literacy on the financial access variables vanishes. Further, running the regressions without the cost variable but with the decreased sample size still yields non-significant effects of financial literacy. Hence, we conclude that the missing effects of financial literacy are due to the specific small sample and are not related to the inclusion of a bank account cost covariate. We here only show the results for the regressions explaining account ownership, but performed this exercise with all outcome variables and found similar results. Financial literacy remains significant, also when controlling for additional variables.

These checks make us confident to say that the cost of financial products should not be neglected in such estimations, however, financial literacy, nevertheless, remains to have a distinct effect on financial inclusion. Controlling additionally for dummies that classify our sample according to the World Bank definition into low, lower middle, upper middle and high income countries sheds light on whether financial literacy still has a distinct effect on financial access and use despite varying income levels. OLS regression results are depicted in Table C.11. The statistically significant coefficient estimates of financial literacy range between 0.35 and 0.52 and are thus of comparable

yet smaller size as the coefficients of our preferred OLS estimation (Tables ?? and ??). Except for the savings specification, the coefficients of the country group dummies show negative signs implying that access and use of financial services is more pronounced in high income countries.

Disaggregating Financial Literacy. As already mentioned, the variable of interest, financial literacy, depicts the share of a country's population that is able to answer 3 out of 4 financial literacy topics correctly. Disaggregating this measure and inserting the actual shares of correctly answered risk diversification, inflation, interest and interest compounding questions as explanatory variables, and running the OLS regression lets us disentangle which field of knowledge is most important in supporting financial access and financial use. At the same time, these measures set a lower standard of financial literacy than the rather harsh cut off of being able to answer questions on at least three out of four concepts. Considering that multicollinearity could endanger the results, we calculated the variance inflation factor for each of the explanatory variables and find that it never exceeds 10. Based on this rule of thumb, we rule out multicollinearity in our case.

We find that there is no clear pattern about knowledge on a single financial literacy concept affecting financial access or use more than others. Rather, knowledge about interest rates influences the share of people that have an account or a debit card positively, whereas knowledge on inflation or interest compounding does not change the share of people with financial access. The financial use variables are also affected differently by the disaggregated financial literacy measures: Knowledge on interest compounding has a highly significant effect on saving at a formal financial institution which hints to the conclusion that more sophisticated financial products may require more sophisticated financial knowledge. On the contrary, using a debit card is affected significantly and positively by financial literacy on risk. Regression results are available upon request.

Omitting Control Variables. Even though the variance inflation factors of all control variables lie far below the threshold of 10, we re-run our OLS estimations omitting two variables to rule out biases possibly caused by high correlation between the control variables: The share of population aged 15-64 and secondary education (Tables C.12 and C.13). The results remain similar to the full specification in terms of significance and size. Multicollinearity seems to be no problem for the analysis.

Adding Proxies for Culture. Culture may influence the degree to which people access and use financial services. For this reason, we add three different types of variables that proxy cultural institutions as control variables in our OLS regressions. We control for (i) religion. We use data from the World Religion Dataset (Maoz and

Henderson, 2013) from the year 2010. The variables display percentage shares of the population that adhere to the respective faiths. Results from this analysis are found in Table C.14. Overall, the coefficient of financial literacy on all financial inclusion measures remains positive and significant.

Second, we employ (ii) Hofstede's cultural dimensions (Hofstede et al., 2010) as proxy for culture which includes power distance, the degree of individualism, masculinity, uncertainty avoidance, long-term orientation, and indulgence (Table C.15). These dimensions of culture are measured on a scale from 0-100 with 100 displaying the specific dimension exactly and zero portraying its counterpart. Due to a big drop in observations, standard errors go up while coefficients remain largely stable compared to earlier Table C.10 so that there is no significant relationship between financial literacy and financial inclusion in these regressions. With regards to the cultural dimensions, a more masculine, i.e. more competitive, society tends to be negatively correlated with financial inclusion whereas a high degree of long-term orientation and individualism positively affects access and use of financial services.

Lastly, Table C.16 shows results from regressions including dummy variables for (iii) Scandinavian, French, and German legal origins (La Porta et al., 2008) as additional control variables. British legal origin serves as reference category. The level of financial literacy in a country continues to have a distinct significant and positive effect on all measures of financial inclusion. Furthermore, countries with Scandinavian and German legal origins have a higher positive effect on financial access and use as compared to countries with British legal origin. However, as is often found in the literature, countries with French legal origin do not perform as well compared to British legal origin countries.

Applying Different Income Definitions. In our standard regression specification, we use log GDP per capita in PPP constant 2011 international US-dollars as the measure of income. As expected, repeating this exercise with other income definitions such as log GNI per capita (as it is used by the World Bank for the derivations of its country classifications) does not change the results significantly. Rather, the size of the effect of financial literacy on the respective access and use measures is higher in these OLS regressions. Results are available upon request.

C.4 Different Estimation Techniques

Fractional Response Regressions. All our dependent variables reflect proportions of aggregated binary outcome questions. Papke and Wooldridge (1996) propose

functional forms for regressions with such fractional variables that take into account their specific discrete characteristics. Thus, in order to check for the validity of the OLS results, we run probit regressions considering the fractional response nature of the data. Table C.17 presents the marginal effects regarding the financial access and use variables and shows that they are similar in magnitude to the OLS results discussed earlier. Countries with a higher level of financial literacy have higher access to and higher use of financial products. Furthermore, higher financial depth affects access to accounts, owning a debit card and saving at formal financial institutions positively and significantly.

Quantile Regressions. We also employ quantile regression analysis since this estimation strategy is more robust to outliers and provides a richer characterization of data so that the effect of a control variable along the distribution of the dependent variable may be measured and not just its conditional mean. We run quantile regressions at the 25th, 50th, and 75th quantile for our four outcome variables. Results are presented in Table C.18. The impact of financial literacy on account ownership is highest for those countries at the median level of account ownership, however, an equality of coefficients test cannot rule out that these differences are significantly different from zero. Interestingly, the specifications with dependent variables describing the use of finance, show that the effect of financial literacy is significant at all estimated quantiles of the distribution albeit higher for the 75th percentile – again an equality of quantile estimates test cannot rule out that they are statistically different from each other. Thus, we conclude that the effect of financial literacy on our four outcome variables is positive and significant at all levels of financial inclusion.

Interaction Analysis with Instrumental Variable. As a last robustness check, we re-estimate all OLS regressions with interactions in an instrumental variable setting. As above, numeracy among primary school children is used as instrument for financial literacy. Results for our three interaction terms are presented in Tables C.19 to C.21 and Figures C.3 to C.5. These interaction terms show a similar pattern to the OLS results above, with signs on the interaction term in the regression being mostly the same. However, the results tend to be insignificant, most probably because of the larger confidence interval caused by the lower number of observations.

Table C.1: Financial literacy and Access to Finance: IV Results using Lewbel (2012)

	Account Ownership			Debit Card Ownership		
	Standard IV (1)	Lewbel Generated Instruments (2)	Combined Model (3)	Standard IV (4)	Lewbel Generated Instruments (5)	Combined Model (6)
Financial Literacy	2.201*** (0.823)	1.249*** (0.307)	1.428*** (0.270)	1.831*** (0.622)	1.750*** (0.302)	1.717*** (0.247)
Secondary Education	0.011 (0.225)	0.073 (0.161)	0.062 (0.170)	0.115 (0.191)	0.120 (0.182)	0.122 (0.180)
Tertiary Education	-0.433 (-0.379)	-0.007 (0.196)	-0.077 (0.190)	-0.096 (0.302)	-0.064 (0.222)	-0.051 (0.200)
Bank Branches per 1000 km ²	0.1407** (0.064)	0.138*** (0.044)	0.139*** (0.048)	0.0862 (0.053)	0.086* (0.051)	0.086* (0.0508)
Strength of Legal Rights Index	-2.146** (0.915)	-1.647** (0.670)	-1.741** (0.677)	-2.244*** (0.758)	-2.20*** (0.698)	-2.185*** (0.678)
Ease of Doing Business Index	-0.176 (0.110)	-0.238*** (0.074)	-0.227*** (0.078)	-0.189*** (0.094)	-0.194*** (0.080)	-0.197** (0.082)
Constant	1.263 (32.98)	30.531 (20.712)	25.011 (20.806)	-6.92 (28.7)	-4.449 (22.489)	-3.425 (22.266)
Observations	95	95	95	95	95	95
Hansen J-Statistic	0	8.8	8.18	0	4.29	4.53
Hansen J-Statistic p-value	0	0.066	0.147	0	0.368	0.476
F-Statistics of First Stage	10.58	10.65	12.92	10.58	10.65	12.92

Note: The table reports the results of Lewbel model regression, of financial literacy on access to finance. Columns (1) and (4) show results for standard IV regressions. Columns (2) and (5) show results of regressions using generated instruments only, columns (3) and (6) show results regressions using a combination of generated and external instruments.

Table C.2: Financial literacy and Use of Financial Services: IV Results using Lewbel (2012)

	Saved at Formal Fin. Institution			Used Debit Card in Last Year		
	Standard IV (1)	Lewbel Generated Instruments (2)	Combined Model (3)	Standard IV (4)	Lewbel Generated Instruments (5)	Combined Model (6)
Financial Literacy	1.329*** (0.473)	1.518*** (0.255)	1.489*** (0.202)	1.714*** (0.528)	2.095*** (0.365)	1.945*** (0.279)
Secondary Education	-0.096 (0.142)	-0.109 (0.140)	-0.107 (0.141)	0.089 (0.150)	0.065 (0.173)	0.075 (0.162)
Tertiary Education	-0.022 (0.222)	-0.095 (0.167)	-0.084 (0.155)	0.160 (0.231)	0.012 (0.228)	0.071 (0.184)
Bank Branches per 1000 km ²	0.098** (0.040)	0.098** (0.042)	0.098** (0.041)	0.051 (0.053)	0.052 (0.058)	0.052 (0.056)
Strength of Legal Rights Index	-0.863 (0.663)	-0.962 (0.527)	-0.947 (0.590)	-1.390** (0.692)	-1.590** (0.725)	-1.511** (0.675)
Ease of Doing Business Index	-0.059 (0.062)	-0.047 (0.063)	-0.048 (0.061)	-0.124 (0.085)	-0.099 (.084)	-0.109 (0.082)
Constant	-14.012 (18.632)	-19.800 (18.543)	-18.900 (16.820)	-26.26 (25.927)	-37.97 (22.900)	-33.333 (22.111)
Observations	95	95	95	95	95	95
Hansen J-Statistic	0	7.58	8.53	0	1.55	2.68
Hansen J-Statistic p-value	0	0.108	0.046	0	0.817	0.883
F-Statistics of First Stage	10.58	10.65	12.98	10.58	10.65	12.98

Note: The table reports the results of Lewbel model regression, of financial literacy on use of financial services. Columns (1) and (4) show results for standard IV regressions. Columns (2) and (5) show results of regressions using generated instruments only, columns (3) and (6) show results regressions using a combination of generated and external instruments.

Table C.3: Financial Literacy, GDP, and Their Interaction

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.520*** (0.165)	0.428** (0.171)	0.449*** (0.084)	0.546*** (0.161)
Log GDP p.c. (PPP)	0.684*** (0.165)	0.777*** (0.178)	0.190* (0.100)	0.634*** (0.169)
Financial Literacy × Log GDP p.c.	−0.015** (0.006)	−0.003 (0.007)	0.019*** (0.005)	0.013* (0.006)
Population Share between 15 and 64	0.010 (0.357)	−0.134 (0.277)	−0.251 (0.209)	−0.281 (0.292)
Secondary Education	0.145 (0.107)	0.170* (0.097)	0.031 (0.072)	0.107 (0.083)
Tertiary Education	−0.189 (0.144)	−0.134 (0.181)	−0.106 (0.110)	0.014 (0.169)
Private Credit to GDP	0.149*** (0.032)	0.100** (0.042)	0.107*** (0.038)	0.042 (0.037)
Bank Branches per 1000 km ²	0.057*** (0.018)	0.029 (0.023)	0.030** (0.013)	0.023 (0.025)
Strength of Legal Rights Index	0.003 (0.546)	−0.368 (0.480)	−0.283 (0.380)	−0.129 (0.421)
Ease of Doing Business Index	−0.158*** (0.050)	−0.168*** (0.051)	−0.066* (0.035)	−0.135** (0.054)
Constant	55.114** (24.191)	51.277** (20.057)	35.807** (15.263)	47.168** (23.407)
R-squared	0.790	0.815	0.773	0.799
Observations	119	119	119	119

Note: The table shows the effect of financial literacy, log GDP per capita and their interaction on different measures of financial inclusion, including access to and use of financial services. Robust standard errors in parentheses. The interacted variables were centered at their means which correspond to about 6041,35 PPP USD for GDP per capita (re-converted to real values) and 36.4% for financial literacy. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.4: Financial Literacy, Bank Branch Penetration, and Their Interaction

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.506*** (0.141)	0.518*** (0.157)	0.534*** (0.086)	0.686*** (0.158)
Bank Branches per 1000 km ²	0.079** (0.033)	0.033 (0.029)	0.011 (0.027)	0.033 (0.034)
Financial Literacy× Bank Branches per 1000 km ²	−0.003 (0.003)	0.000 (0.004)	0.003 (0.003)	−0.000 (0.005)
Log GDP p.c. (PPP)	13.392*** (2.834)	13.936*** (2.570)	6.061*** (1.479)	12.236*** (2.428)
Population Share between 15 and 64	−0.274 (0.342)	−0.480 (0.303)	−0.578** (0.222)	−0.725** (0.302)
Secondary Education	0.017 (0.106)	0.028 (0.103)	−0.052 (0.067)	−0.031 (0.095)
Tertiary Education	−0.129 (0.140)	0.030 (0.147)	0.004 (0.114)	0.245* (0.139)
Private Credit to GDP	0.127*** (0.032)	0.094** (0.043)	0.117*** (0.044)	0.046 (0.041)
Strength of Legal Rights Index	0.279 (0.548)	−0.000 (0.512)	0.177 (0.408)	0.327 (0.502)
Ease of Doing Business Index	−0.098* (0.050)	−0.105* (0.053)	−0.034 (0.040)	−0.074 (0.058)
Constant	−51.204** (25.194)	−57.948** (25.265)	0.236 (18.639)	−40.174 (29.623)
R-squared	0.804	0.816	0.739	0.779
Observations	119	119	119	119

Note: The table shows the effect of financial literacy, bank branch penetration and their interaction on different measures of financial inclusion, including access to and use of financial services. Robust standard errors in parentheses. The interacted variables were centered at their means which correspond to 22.44 for bank branches per 1000 km² and 36.4% for financial literacy. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.5: Financial Literacy and Financial Inclusion for Different Income Groups

	Account Ownership		Debit Card Ownership	Saved at Formal Fin. Institution	Used Debit Card in the Last Year				
	(1)	(2)			(3)	(4)	(5)	(6)	(7)
FL Bottom 40%	0.426*** (0.148)		0.500*** (0.139)	0.360** (0.145)			0.649*** (0.136)		
FL Top 60%		0.530*** (0.134)		0.508*** (0.129)		0.442*** (0.145)		0.689*** (0.136)	
Log GDP p.c. (PPP)	14.073*** (2.965)	12.864*** (2.670)	13.587*** (2.784)	14.340*** (2.581)	2.203 (2.917)	3.291 (2.894)	10.725*** (2.732)	13.360*** (2.709)	
Population Share between 15 and 64	-0.537 (0.347)	-0.066 (0.316)	-0.760** (0.326)	-0.314 (0.306)	-0.849** (0.342)	-0.769** (0.343)	-0.885*** (0.320)	-0.628* (0.321)	
Secondary Education	0.019 (0.117)	0.018 (0.103)	0.036 (0.110)	0.024 (0.100)	0.002 (0.116)	-0.045 (0.112)	0.005 (0.108)	-0.053 (0.105)	
Tertiary Education	-0.035 (0.164)	-0.223 (0.143)	0.161 (0.154)	-0.055 (0.139)	0.156 (0.162)	-0.005 (0.155)	0.359** (0.151)	0.165 (0.146)	
Private Credit to GDP	0.158*** (0.043)	0.111*** (0.038)	0.096** (0.040)	0.091** (0.037)	0.127*** (0.042)	0.108** (0.041)	0.046 (0.040)	0.046 (0.039)	
Bank Branches per 1000 km ²	0.059* (0.033)	0.058** (0.029)	0.030 (0.031)	0.037 (0.028)	0.014 (0.032)	0.025 (0.031)	0.018 (0.030)	0.037 (0.029)	
Strength of Legal Rights Index	0.247 (0.605)	0.375 (0.534)	-0.035 (0.568)	0.038 (0.516)	0.737 (0.596)	0.643 (0.578)	0.394 (0.558)	0.298 (0.542)	
Ease of Doing Business Index	-0.137** (0.057)	-0.079 (0.051)	-0.122** (0.054)	-0.094* (0.049)	0.033 (0.056)	0.033 (0.055)	-0.081 (0.053)	-0.071 (0.052)	
Constant	-63.114** (31.726)	-77.057*** (28.073)	-61.545** (29.782)	-87.831*** (27.147)	49.882 (31.207)	47.887 (30.428)	-46.158 (29.226)	-78.765*** (28.488)	
Test Bottom 40%= Top 60% (p-values)	0.789 119	0.3085 119	0.782 119	0.9258 119	0.312 119	0.352 119	0.734 119	0.6039 119	
R-squared									
Observations									

Note: The table shows OLS results with standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.6: Financial Literacy and Financial Inclusion - OLS, Excluding Countries with More than 50% Muslims

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.510*** (0.175)	0.538*** (0.185)	0.549*** (0.096)	0.684*** (0.185)
Log GDP p.c. (PPP)	12.497*** (3.192)	12.999*** (2.840)	6.549*** (1.686)	12.536*** (2.927)
Population Share between 15 and 64	-0.174 (0.416)	-0.428 (0.350)	-0.717** (0.272)	-0.627 (0.388)
Secondary Education	0.066 (0.123)	0.092 (0.131)	-0.080 (0.074)	0.050 (0.129)
Tertiary Education	-0.117 (0.145)	-0.009 (0.169)	0.031 (0.128)	0.251 (0.172)
Private Credit to GDP	0.121*** (0.030)	0.086* (0.045)	0.106** (0.046)	0.039 (0.046)
Bank Branches per 1000 km ²	0.043*** (0.015)	0.021 (0.016)	0.023 (0.017)	0.009 (0.019)
Strength of Legal Rights Index	-0.712 (0.575)	-1.016* (0.524)	-0.523 (0.461)	-0.480 (0.567)
Ease of Doing Business Index	-0.094 (0.058)	-0.136** (0.068)	-0.047 (0.050)	-0.081 (0.080)
Constant	-63.942** (31.905)	-65.433* (33.170)	-8.209 (25.617)	-71.900* (42.273)
R-squared	0.800	0.825	0.751	0.787
Observations	89	89	89	89

Note: The table reports OLS regression results with robust standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.7: Financial literacy and Financial Inclusion - IV, Excluding Countries with More than 50% Muslims

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	1.533** (0.584)	1.708*** (0.479)	0.974*** (0.360)	1.763*** (0.446)
Log GDP p.c. (PPP)	3.936 (8.571)	2.919 (7.164)	2.488 (4.862)	3.772 (6.170)
Population Share between 15 and 64	0.931 (0.944)	1.237 (0.842)	-0.223 (0.641)	1.002 (0.861)
Secondary Education	-0.063 (0.173)	-0.003 (0.201)	-0.089 (0.132)	0.041 (0.207)
Tertiary Education	-0.349** (0.166)	-0.302 (0.200)	-0.036 (0.156)	0.019 (0.221)
Private Credit to GDP	0.130*** (0.041)	0.081* (0.046)	0.103** (0.042)	0.027 (0.043)
Bank Branches per 1000 km ²	0.074 (0.053)	0.036 (0.060)	0.069* (0.039)	0.000 (0.060)
Strength of Legal Rights Index	-1.856** (0.910)	-2.282*** (0.801)	-1.063 (0.719)	-1.631* (0.838)
Ease of Doing Business Index	-0.080 (0.100)	-0.112 (0.116)	-0.044 (0.072)	-0.045 (0.131)
Constant	-80.884 (49.805)	-108.683* (55.257)	-15.487 (34.891)	-127.806** (61.354)
R-squared	0.747	0.735	0.739	0.711
Observations	72	72	72	72

Note: The table reports IV regression results with robust standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.8: Financial Literacy and Borrowing Decisions - OLS and IV Results

	Borrowed from a formal fin. institution		Borrowed from an Informal Institution		Used Credit Card in the Last Year	
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Financial Literacy	0.092 (0.064)	-0.085 (0.141)	-0.094** (0.045)	-0.597** (0.234)	0.264** (0.111)	0.856*** (0.331)
Log GDP p.c. (PPP)	0.360 (0.972)	1.872 (1.830)	0.650 (1.093)	5.654** (2.764)	7.655*** (1.688)	3.102 (3.981)
Population Share between 15 and 64	0.179 (0.131)	-0.177 (0.234)	0.008 (0.100)	-0.666** (0.318)	-0.593*** (0.177)	0.056 (0.480)
Secondary Education	0.032 (0.047)	0.069 (0.048)	-0.051 (0.044)	-0.023 (0.078)	-0.031 (0.056)	-0.023 (0.091)
Tertiary Education	0.022 (0.050)	0.086 (0.059)	0.011 (0.038)	0.071 (0.065)	0.240** (0.100)	0.123 (0.141)
Private Credit to GDP	0.003 (0.016)	-0.003 (0.017)	-0.007 (0.016)	-0.001 (0.024)	0.059* (0.032)	0.039 (0.034)
Bank Branches per 1000 km ²	-0.009 (0.006)	-0.001 (0.020)	-0.011* (0.006)	-0.027 (0.019)	0.037* (0.019)	0.076 (0.049)
Strength of Legal Rights Index	0.273 (0.270)	0.195 (0.279)	-0.047 (0.185)	0.301 (0.319)	0.405 (0.323)	-0.168 (0.577)
Ease of Doing Business Index	-0.039** (0.019)	-0.052** (0.023)	0.014 (0.017)	-0.011 (0.031)	-0.017 (0.033)	0.008 (0.043)
Constant	-5.791 (10.457)	8.101 (11.924)	3.033 (9.766)	15.769 (15.829)	-35.636* (18.617)	-53.646** (24.342)
R-squared	0.436	0.377	0.182	-0.942	0.708	0.605
Observations	119	93	119	93	119	93

Note: The table reports OLS regression in columns (1), (3) and (5), and IV regression results in columns (2), (4) and (6) with robust standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. Borrowed at formal financial institution is the proportion of people that currently borrow at a formal financial institution, borrowed at informal financial institution described those that borrowed from an informal financial institution, credit card used during the last year is the proportion of people that used their credit card during the last year.

Table C.9: Financial Literacy and High Frequency of Use - OLS and IV Results

	High Frequency of Account Use	
	OLS	IV
	(1)	(2)
Financial Literacy	0.588*** (0.115)	1.471*** (0.445)
Log GDP p.c. (PPP)	8.835*** (1.805)	2.071 (4.641)
Population Share between 15 and 64	-1.119*** (0.252)	-0.240 (0.643)
Secondary Education	-0.058 (0.073)	-0.023 (0.123)
Tertiary Education	0.314*** (0.113)	0.151 (0.157)
Private Credit to GDP	0.105*** (0.032)	0.086** (0.036)
Bank Branches per 1000 km ²	0.021 (0.015)	0.038 (0.050)
Strength of Legal Rights Index	0.285 (0.463)	-0.703 (0.738)
Ease of Doing Business Index	-0.079* (0.043)	-0.052 (0.069)
Constant	-15.432 (22.296)	-37.482 (33.796)
R-squared	0.816	0.732
Observations	119	93

Note: The table reports OLS regression in column (1), and IV regression results in column (2) with robust standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively. High frequency of account use denotes the percentage of the population (older than 15 years) that have taken money out of a formal bank account at a bank or other formal financial institution at least three times in a typical month, including cash withdrawals, electronic payments or purchases, checks, or any other type of payment debit, either by account owner or other parties.

Table C.10: Financial Literacy and Account Ownership - Additional Control Variables

	Account Ownership			
	(1)	(2)	(3)	(4)
Financial Literacy	0.567*** (0.172)	0.592*** (0.175)	0.530*** (0.189)	0.389 (0.239)
Log GDP p.c. (PPP)	11.705*** (3.708)	12.112*** (3.789)	18.758*** (3.474)	24.048*** (3.726)
Population Share between 15 and 64	-0.106 (0.377)	-0.090 (0.384)	-0.135 (0.437)	-0.195 (0.542)
Secondary Education	0.017 (0.139)	0.016 (0.138)	-0.036 (0.159)	-0.008 (0.181)
Tertiary Education	-0.131 (0.169)	-0.149 (0.172)	-0.230 (0.189)	-0.302 (0.295)
Private Credit to GDP	0.124*** (0.032)	0.123*** (0.032)	0.111*** (0.031)	0.146** (0.055)
Strength of Legal Rights Index	0.289 (0.682)	0.493 (0.703)	0.675 (0.711)	0.805 (0.990)
Ease of Doing Business Index	-0.064 (0.065)	-0.048 (0.067)	-0.017 (0.069)	0.072 (0.090)
Bank Branches per 1000 km ²	0.050*** (0.019)	0.024 (0.021)	0.021 (0.023)	0.025 (0.025)
Political Risk	0.229 (0.270)	0.213 (0.268)	0.050 (0.279)	0.117 (0.378)
ATMs per 1000 km ²		0.015** (0.006)	0.018*** (0.006)	0.018** (0.007)
Cost Checking Account (Imputed)			0.943* (0.479)	
Cost Checking Account (original)				1.371** (0.563)
Constant	-85.412*** (32.134)	-92.331*** (32.846)	-139.172*** (38.231)	-196.886*** (49.643)
R-squared	0.789	0.787	0.776	0.783
Observations	103	101	88	57

Note: The table reports OLS regression results with robust standard errors in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.11: Financial Literacy and Financial Services - OLS Results
Including Country Group Dummies

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.411*** (0.145)	0.349** (0.153)	0.523*** (0.100)	0.472*** (0.147)
Log GDP p.c. (PPP)	6.846 (4.758)	7.264** (3.176)	7.840*** (2.904)	6.197** (2.644)
Population Share between 15 and 64	-0.055 (0.317)	-0.196 (0.271)	-0.618*** (0.220)	-0.370 (0.253)
Secondary Education	0.021 (0.112)	0.049 (0.098)	-0.062 (0.077)	-0.006 (0.081)
Tertiary Education	-0.130 (0.149)	0.013 (0.148)	-0.022 (0.115)	0.169 (0.137)
Private Credit to GDP	0.122*** (0.031)	0.079 (0.050)	0.112** (0.044)	0.027 (0.046)
Strength of Legal Rights Index	0.412 (0.538)	0.100 (0.456)	0.113 (0.413)	0.414 (0.399)
Ease of Doing Business Index	-0.093** (0.042)	-0.096** (0.045)	-0.038 (0.041)	-0.072* (0.042)
Bank Branches per 1000 km ²	0.054*** (0.019)	0.026 (0.016)	0.032* (0.018)	0.018 (0.017)
Low Income Country	-20.595 (13.810)	-21.964** (9.184)	3.387 (9.160)	-21.268** (9.681)
Lower Middle Income Country	-19.580** (8.677)	-23.710*** (6.075)	3.949 (6.238)	-24.098*** (6.671)
Upper Middle Income Country	-11.375** (4.857)	-16.777*** (4.611)	-0.811 (4.075)	-20.828*** (5.014)
Constant	-10.981 (48.494)	-14.810 (30.251)	-32.491 (30.116)	-8.987 (30.689)
R-squared	0.818	0.845	0.742	0.826
Observations	119	119	119	119

Note: The table reports OLS regression results. Robust standard errors are shown in parentheses. The omitted country group variable is high income country. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.12: Financial Literacy and Access to Financial Services - OLS Results - Without Population And/Or Education Variables

	Account Ownership		Debit Card Ownership	
	(1)	(2)	(3)	(4)
Financial Literacy	0.535*** (0.108)	0.464*** (0.135)	0.604*** (0.138)	0.495*** (0.153)
Log GDP p.c. (PPP)	11.533*** (2.285)	13.295*** (2.788)	12.499*** (2.192)	14.845*** (2.674)
Population Share between 15 and 64		-0.141 (0.347)		-0.362 (0.311)
Secondary Education	0.045 (0.099)		0.027 (0.103)	
Private Credit to GDP	0.129*** (0.030)	0.128*** (0.031)	0.092** (0.044)	0.092** (0.044)
Strength of Legal Rights Index	0.334 (0.543)	0.289 (0.538)	0.055 (0.532)	-0.015 (0.513)
Ease of Doing Business Index	-0.079 (0.051)	-0.078 (0.052)	-0.079 (0.052)	-0.089* (0.051)
Bank Branches per 1000 km ²	0.057*** (0.017)	0.052*** (0.017)	0.035** (0.017)	0.030* (0.018)
Constant	-77.990*** (24.572)	-79.807*** (27.092)	-99.678*** (24.437)	-91.438*** (26.288)
R-squared	0.800	0.784	0.811	0.798
Observations	120	120	120	120

Note: The table reports OLS regression results. Robust standard errors are shown in parentheses. Columns (1) and (2) show results with the proportion of the population that have a bank account as the outcome variable. Columns (3) and (4) show results with the proportion that has a debit card as the outcome variable. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.13: Financial Literacy and Use of Financial Services
- OLS Results - Without Population And/Or Education
Variables

	Saved at formal fin. institution		Debit Card Ownership	
	(1)	(2)	(3)	(4)
Financial Literacy	0.652*** (0.080)	0.522*** (0.084)	0.844*** (0.150)	0.689*** (0.157)
Log GDP p.c. (PPP)	4.278*** (1.368)	6.157*** (1.516)	11.183*** (2.302)	13.585*** (2.531)
Population Share between 15 and 64		-0.575*** (0.213)		-0.572* (0.320)
Secondary Education	-0.053 (0.066)		-0.066 (0.102)	
Private Credit to GDP	0.113** (0.045)	0.118*** (0.045)	0.045 (0.040)	0.052 (0.040)
Strength of Legal Rights Index	0.183 (0.430)	0.137 (0.415)	0.404 (0.543)	0.320 (0.521)
Ease of Doing Business Index	0.005 (0.037)	-0.022 (0.036)	-0.048 (0.054)	-0.065 (0.055)
Bank Branches per 1000 km ²	0.033* (0.019)	0.032* (0.019)	0.033* (0.019)	0.028 (0.020)
Constant	-47.266*** (16.310)	-23.646 (18.314)	-104.878*** (26.050)	-86.662*** (30.726)
R-squared	0.720	0.731	0.764	0.755
Observations	120	120	120	120

Note: The table reports OLS regression results. Robust standard errors are shown in parentheses. The outcome variables are the proportion of people that saved at a formal financial institution and the proportion of people that used their debit card during the last year. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.14: Fin. Literacy and Fin. Inclusion Incl. Religiosity - OLS Results

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.364** (0.159)	0.443*** (0.138)	0.357*** (0.083)	0.627*** (0.112)
Log GDP p.c. (PPP)	14.211*** (2.482)	14.326*** (2.212)	5.009*** (1.547)	9.896*** (2.074)
Population Share between 15 and 64	-0.378 (0.342)	-0.511* (0.278)	-0.523** (0.219)	-0.585** (0.257)
Secondary Education	0.106 (0.102)	0.089 (0.104)	0.087 (0.068)	0.002 (0.101)
Tertiary Education	-0.014 (0.126)	0.174 (0.137)	0.245*** (0.083)	0.393*** (0.125)
Private Credit to GDP	0.105*** (0.033)	0.062 (0.041)	0.087*** (0.030)	0.059 (0.036)
Strength of Legal Rights Index	-0.222 (0.557)	-0.361 (0.519)	0.027 (0.395)	0.080 (0.546)
Ease of Doing Business Index	-0.042 (0.048)	-0.059 (0.050)	-0.012 (0.031)	-0.032 (0.056)
Bank Branches per 1000 km ²	0.058*** (0.017)	0.044*** (0.015)	0.030* (0.016)	0.030** (0.015)
Christianity (prot.)	8.591 (10.252)	10.697 (8.488)	16.591*** (5.744)	7.553 (9.124)
Christianity (other)	4.608 (7.269)	-1.242 (6.123)	-14.656*** (3.958)	-12.242* (6.667)
Judaism	6.551 (7.135)	-59.908*** (5.759)	9.820* (5.259)	-69.786*** (5.025)
Islam (Sunni)	-4.810 (5.051)	-3.045 (4.569)	-2.666 (3.232)	-7.097 (4.372)
Islam (Shi'a)	-34.167*** (8.163)	-27.957*** (9.093)	-12.842** (5.188)	-21.271** (9.210)
Islam (Other)	-11.189 (35.004)	-28.893 (39.566)	-3.205 (49.855)	-2.523 (44.700)
Buddhism	13.780 (11.516)	1.445 (8.419)	12.148** (5.566)	-14.145** (6.395)
Hinduism	26.042*** (7.234)	11.507* (6.167)	15.349*** (4.911)	5.002 (5.308)
Non-Religious	29.992** (13.362)	30.184** (15.208)	15.506 (9.702)	29.684* (15.399)
Other Religions	-1.979 (11.043)	5.264 (7.667)	8.600 (7.950)	-19.735*** (5.873)
Constant	-77.603*** (26.730)	-84.495*** (24.908)	-18.750 (17.365)	-55.657** (26.314)
R-squared	0.845	0.868	0.837	0.862
Observations	117	117	117	117

Note: The table reports OLS regression results. Robust standard errors are shown in parentheses. ***, ** and * denote significance at the 1%, 5% and 10% levels. Religion variables depict percentage shares of the population adhering to the respective faith. Catholic Christianity is the left-out category due to concerns of multicollinearity.

Table C.15: Financial Literacy and Financial Inclusion Incl. Hofstede's Cultural Dimensions – OLS results

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.375 (0.285)	0.376 (0.266)	0.358 (0.218)	0.392 (0.357)
Log GDP p.c. (PPP)	17.286** (7.485)	18.404** (6.828)	8.488* (4.551)	15.841** (7.627)
Population Share between 15 and 64	0.670 (0.603)	-0.305 (0.635)	-0.535 (0.609)	-0.213 (0.716)
Secondary Education	0.114 (0.217)	0.188 (0.186)	0.072 (0.141)	0.296 (0.219)
Tertiary Education	-0.097 (0.266)	-0.238 (0.259)	0.149 (0.179)	0.148 (0.277)
Private Credit to GDP	0.172*** (0.040)	0.083* (0.048)	0.110** (0.044)	-0.031 (0.065)
Strength of Legal Rights Index	-0.720 (0.667)	-0.242 (0.689)	-0.298 (0.694)	0.000 (0.844)
Ease of Doing Business Index	0.006 (0.104)	-0.007 (0.116)	-0.021 (0.061)	0.039 (0.143)
Bank Branches per 1000 km ²	0.015 (0.021)	0.012 (0.021)	0.033* (0.016)	0.012 (0.025)
Power Distance	0.117 (0.117)	-0.017 (0.097)	-0.069 (0.099)	-0.105 (0.144)
Individualism	0.225 (0.144)	0.207 (0.132)	-0.105 (0.097)	0.315** (0.153)
Masculinity	-0.091 (0.081)	-0.182** (0.080)	0.079 (0.058)	-0.265** (0.099)
Uncertainty Avoidance	0.043 (0.091)	-0.021 (0.099)	-0.294*** (0.083)	-0.101 (0.135)
Long Term Orientation	0.067 (0.110)	0.240* (0.124)	0.171* (0.093)	0.178 (0.171)
Indulgence	-0.149 (0.106)	0.051 (0.124)	0.104 (0.070)	0.083 (0.147)
Constant	-186.289** (73.181)	-142.398* (72.088)	-33.998 (34.062)	-133.964* (74.996)
R-squared	0.869	0.879	0.874	0.856
Observations	52	52	52	52

Note: The table reports OLS regression results. Robust standard errors are shown in parentheses. The five dimensions rank countries from 0-100 with 100 fulfilling the specific dimension exactly and 0 displaying the respective counterpart. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.16: Financial Literacy and Financial Inclusion Incl. Legal Origin – OLS Results

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.407*** (0.147)	0.397** (0.159)	0.415*** (0.093)	0.581*** (0.163)
Log GDP p.c. (PPP)	13.474*** (2.709)	13.821*** (2.461)	6.356*** (1.563)	11.992*** (2.329)
Population Share between 15 and 64	-0.284 (0.314)	-0.498* (0.292)	-0.577*** (0.189)	-0.684** (0.318)
Secondary Education	0.017 (0.107)	0.035 (0.096)	-0.051 (0.066)	-0.023 (0.090)
Tertiary Education	-0.139 (0.151)	0.091 (0.149)	0.037 (0.102)	0.300** (0.139)
Private Credit to GDP	0.114*** (0.033)	0.089** (0.044)	0.092** (0.046)	0.043 (0.043)
Strength of Legal Rights Index	-0.095 (0.486)	-0.174 (0.471)	-0.104 (0.374)	0.326 (0.499)
Ease of Doing Business Index	-0.089* (0.051)	-0.070 (0.052)	-0.023 (0.037)	-0.043 (0.057)
Bank Branches per 1000 km ²	0.076*** (0.018)	0.044*** (0.017)	0.051** (0.023)	0.036** (0.018)
Scandinavian Legal Origin	-5.831 (5.798)	13.743* (7.115)	8.914* (5.063)	22.670*** (8.325)
French Legal Origin	-11.741*** (3.334)	-2.332 (2.987)	-8.222*** (2.060)	1.957 (2.878)
German Legal Origin	2.216 (4.239)	11.528** (4.453)	-1.417 (3.394)	10.155* (5.169)
Constant	-59.477** (25.634)	-74.951*** (25.039)	-12.301 (18.404)	-69.413** (30.602)
R-squared	0.836	0.837	0.775	0.797
Observations	118	118	118	118

Note: The table reports OLS regression results. Robust standard errors in parentheses. British legal origin is the reference category for the different legal origin dummy variables. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.17: Fractional Response Probit Regressions - Marginal Effects

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.537*** (0.130)	0.379*** (0.138)	0.441*** (0.073)	0.418*** (0.120)
Log GDP p.c. (PPP)	0.124*** (0.023)	0.146*** (0.023)	0.068*** (0.017)	0.139*** (0.024)
Population Share between 15 and 64	-0.529* (0.290)	-0.307 (0.262)	-0.398** (0.202)	-0.340 (0.242)
Secondary Education	0.032 (0.089)	0.076 (0.090)	-0.044 (0.073)	0.048 (0.085)
Tertiary Education	-0.063 (0.125)	-0.055 (0.126)	-0.078 (0.096)	0.047 (0.110)
Private Credit to GDP	0.165*** (0.035)	0.074** (0.037)	0.086*** (0.032)	0.021 (0.026)
Bank Branches per 1000 km ²	0.001** (0.000)	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)
Strength of Legal Rights Index	0.005 (0.005)	0.001 (0.005)	0.001 (0.004)	0.002 (0.004)
Ease of Doing Business Index	-0.001 (0.000)	-0.001* (0.000)	-0.000 (0.000)	-0.001 (0.001)
Observations	119	119	119	119

Note: The table reports fractional probit regression results. Robust standard errors are shown in parentheses. The outcome variables are the proportion of people over the age of 15 that have a bank account or own a debit card, proportion of people that saved at a formal financial institution and the proportion of people that used their debit card during the last year. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.18: Financial Literacy and Financial Inclusion - Quantile Regressions

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
25th Quantile				
Financial Literacy	0.396** (0.197)	0.592** (0.250)	0.386** (0.157)	0.540** (0.221)
Constant	-66.842 (41.387)	-100.718*** (37.853)	-3.422 (28.231)	-61.575* (36.143)
Control Variables	yes	yes	yes	yes
50th Quantile				
Financial Literacy	0.456*** (0.152)	0.529*** (0.192)	0.507*** (0.127)	0.784*** (0.171)
Constant	-54.805 (37.330)	-88.692*** (32.082)	-28.256 (27.131)	-19.421 (35.303)
Control Variables	yes	yes	yes	yes
75th Quantile				
Financial Literacy	0.272 (0.179)	0.649*** (0.178)	0.599*** (0.152)	0.670*** (0.193)
Constant	-66.284* (38.425)	-57.808 (37.506)	-27.485 (27.993)	-44.462 (41.268)
Control Variables	yes	yes	yes	yes
Observations	119	119	119	119
Model Deg. of Freedom	30	30	30	30
Degrees of Freedom	109	109	109	109
No. of Replications	500	500	500	500
q1=0.25	0.25	0.25	0.25	0.25
Raw Sum of Deviations (q1)	1137.25	994.79	528.35	763.57
Min Sum of Deviations(q1)	476.05	466.23	343.53	454.20
q2=0.5	0.50	0.50	0.50	0.50
Raw Sum of Deviations (q2)	1577.54	1501.39	836.84	1297.54
Min Sum of Deviations(q2)	586.93	573.86	441.79	573.61
q3=0.75				
Raw Sum of Deviations (q3)	1176.84	1261.57	812.80	1263.94
Min Sum of Deviations(q3)	472.81	445.88	345.09	447.01

Table C.19: Financial Literacy, Financial Depth and Their Interaction (IV)

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.605*** (0.166)	0.623*** (0.211)	0.455*** (0.127)	0.780*** (0.214)
Private Credit to GDP	0.180*** (0.045)	0.118** (0.049)	0.085** (0.040)	0.053 (0.045)
Financial Literacy × Private Credit to GDP	−0.008* (0.005)	−0.006 (0.005)	0.005 (0.004)	−0.002 (0.006)
Log GDP p.c. (PPP)	16.911*** (2.947)	17.253*** (3.015)	6.712*** (1.929)	14.216*** (2.870)
Population Share between 15 and 64	−0.982** (0.419)	−0.939** (0.424)	−0.664** (0.327)	−0.846** (0.398)
Secondary Education	0.013 (0.135)	0.061 (0.137)	−0.061 (0.112)	0.023 (0.131)
Tertiary Education	−0.101 (0.151)	−0.040 (0.191)	−0.054 (0.132)	0.157 (0.193)
Strength of Legal Rights Index	0.460 (0.633)	0.001 (0.662)	−0.148 (0.458)	0.242 (0.634)
Ease of Doing Business Index	−0.074 (0.061)	−0.087 (0.064)	−0.045 (0.042)	−0.063 (0.069)
Bank Branches per 1000 km ²	0.038 (0.041)	0.011 (0.044)	0.082** (0.036)	0.007 (0.054)
Constant	−34.784 (29.625)	−55.873* (28.752)	7.249 (19.436)	−50.379 (32.951)
R-squared	0.828	0.792	0.757	0.760
Observations	93	93	93	93

Note: The table shows the effect of financial literacy, private credit to GDP and their interaction on different measures of financial inclusion, including access to and use of financial services. Numeracy levels among primary school children act as instrument for financial literacy. Robust standard errors in parentheses. The interacted variables were centered at their means which correspond to 57.31% of GDP for financial depth and 36.4% for financial literacy. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.20: Financial Literacy, Bank Branch Penetration and Their Interaction (IV)

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.475*** (0.147)	0.535*** (0.174)	0.533*** (0.105)	0.747*** (0.173)
Bank Branches per 1000 km ²	0.114 (0.096)	0.018 (0.077)	0.004 (0.083)	0.012 (0.090)
Financial Literacy × Bank Branches per 1000 km ²	−0.008 (0.009)	0.001 (0.008)	0.009 (0.011)	−0.000 (0.011)
Log GDP p.c. (PPP)	16.494*** (2.977)	16.919*** (2.908)	6.926*** (1.941)	14.094*** (2.764)
Population Share between 15 and 64	−0.793** (0.394)	−0.746** (0.380)	−0.733** (0.311)	−0.778** (0.365)
Secondary Education	−0.030 (0.137)	0.021 (0.132)	−0.043 (0.107)	0.009 (0.123)
Tertiary Education	−0.127 (0.188)	−0.112 (0.200)	−0.077 (0.166)	0.134 (0.204)
Private Credit to GDP	0.128*** (0.034)	0.093** (0.044)	0.123*** (0.045)	0.043 (0.047)
Strength of Legal Rights Index	−0.066 (0.581)	−0.166 (0.639)	0.302 (0.635)	0.165 (0.710)
Ease of Doing Business Index	−0.088 (0.054)	−0.103* (0.059)	−0.041 (0.052)	−0.068 (0.066)
Constant	−44.095 (27.741)	−66.029** (27.960)	1.506 (22.021)	−54.379* (32.995)
R-squared	0.824	0.810	0.735	0.771
Observations	93	93	93	93

Note: The table shows the effect of financial literacy, bank branch penetration and their interaction on different measures of financial inclusion, including access to and use of financial services. Numeracy levels among primary school children act as instrument for financial literacy. Robust standard errors in parentheses. The interacted variables were centered at their means which correspond to 22.44 branches per 1000 km² and 36.4% for financial literacy. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

Table C.21: Financial Literacy, GDP, and Their Interaction (IV)

	(1) Account Ownership	(2) Debit Card Ownership	(3) Saved at Formal Fin. Institution	(4) Used Debit Card in the Last Year
Financial Literacy	0.622*** (0.168)	0.557*** (0.204)	0.438*** (0.111)	0.618*** (0.204)
Log GDP p.c. (PPP)	0.710*** (0.210)	0.818*** (0.169)	0.096 (0.119)	0.527*** (0.155)
Financial Literacy × Log GDP p.c. (PPP)	−0.020 (0.014)	−0.009 (0.013)	0.025** (0.010)	0.020 (0.014)
Population Share between 15 and 64	−0.192 (0.419)	−0.198 (0.366)	−0.388 (0.273)	−0.124 (0.370)
Secondary Education	0.087 (0.142)	0.149 (0.135)	0.048 (0.108)	0.125 (0.119)
Tertiary Education	−0.134 (0.171)	−0.157 (0.226)	−0.105 (0.130)	−0.038 (0.221)
Private Credit to GDP	0.161*** (0.031)	0.107** (0.044)	0.114*** (0.037)	0.051 (0.038)
Strength of Legal Rights Index	−0.355 (0.616)	−0.668 (0.585)	−0.480 (0.440)	−0.446 (0.508)
Ease of Doing Business Index	−0.156** (0.061)	−0.169*** (0.061)	−0.075* (0.042)	−0.140** (0.063)
Bank Branches per 1000 km ²	0.046 (0.045)	−0.005 (0.056)	0.051 (0.034)	−0.027 (0.058)
Constant	71.212*** (26.285)	59.194** (23.990)	43.504** (17.621)	38.299 (28.092)
R-squared	0.805	0.799	0.774	0.779
Observations	93	93	93	93

Note: The table shows the effect of instrumented financial literacy, log GDP per capita and their interaction on different measures of financial inclusion, including access to and use of financial services. Numeracy levels among primary school children act as instrument for financial literacy. Robust standard errors in parentheses. The interacted variables were centered at their means which correspond to about 6041.35 PPP USD for GDP per capita (re-converted to real values) and 36.4% for financial literacy. ***, ** and * denote significance at the 1%, 5% and 10% levels, respectively.

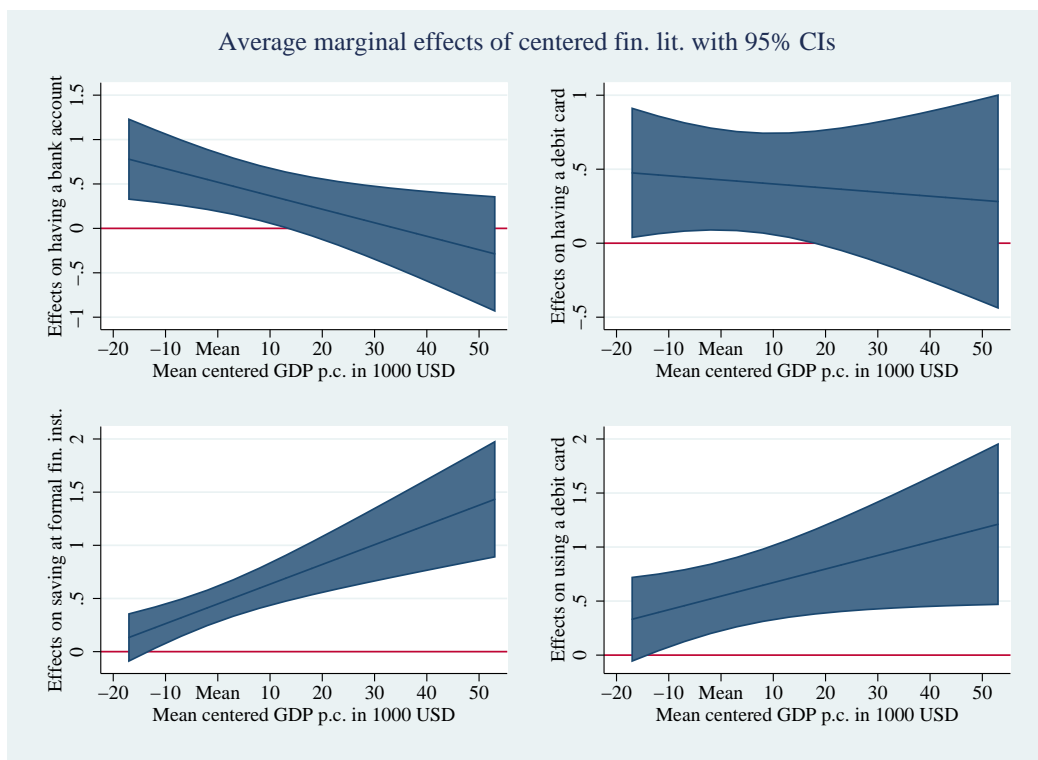


Figure C.1: Average Marginal Effects of Financial Literacy on Four Measures of Financial Inclusion at Different Levels of GDP per capita.

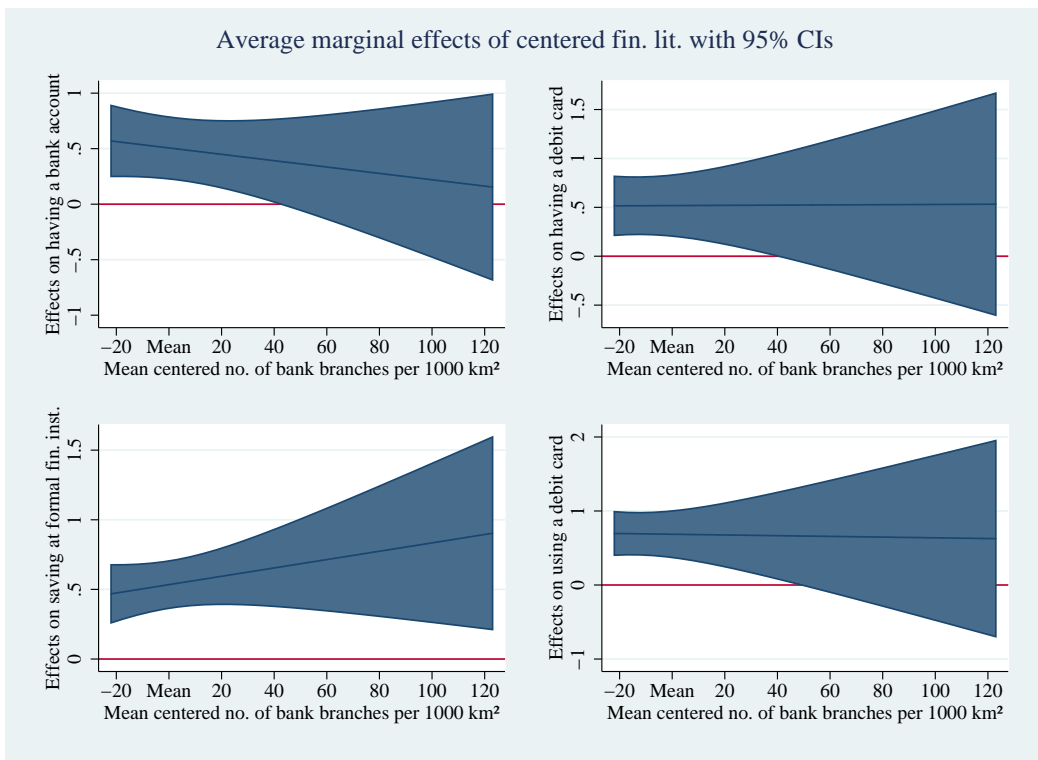


Figure C.2: Average Marginal Effects of Financial Literacy on Four Measures of Financial Inclusion at Different Levels of Bank Branches per 1000 km².

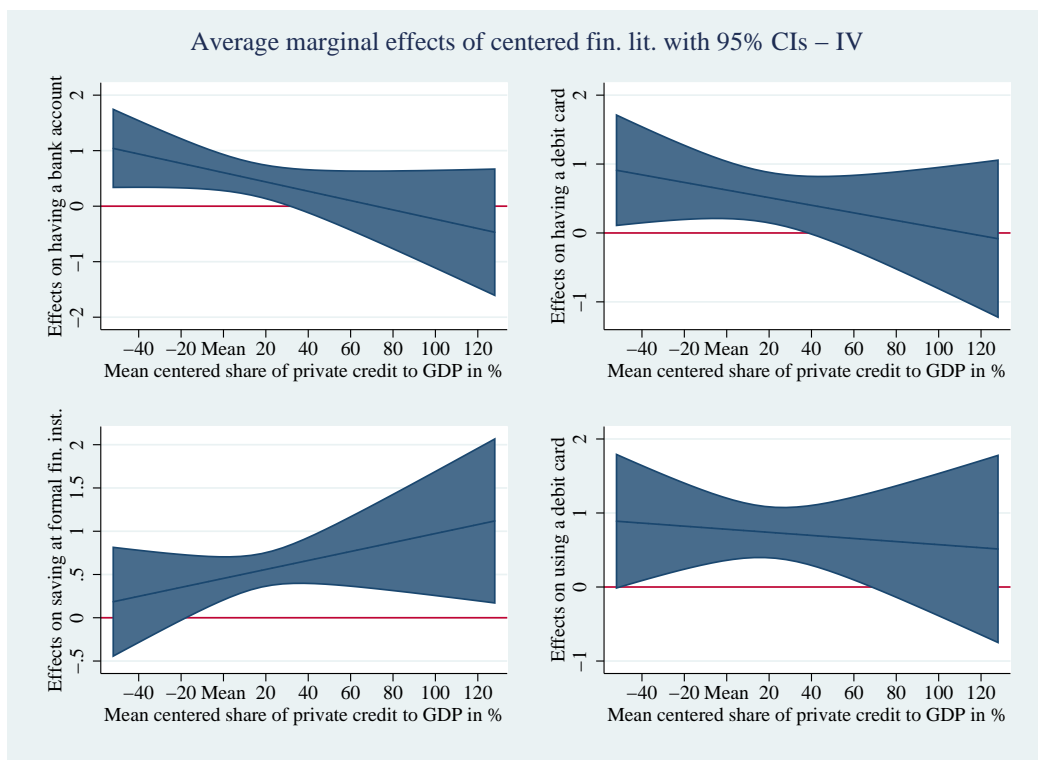


Figure C.3: Average Marginal Effects of Financial Literacy on Four Measures of Financial Inclusion at Different Levels of Private Credit to GDP (IV).

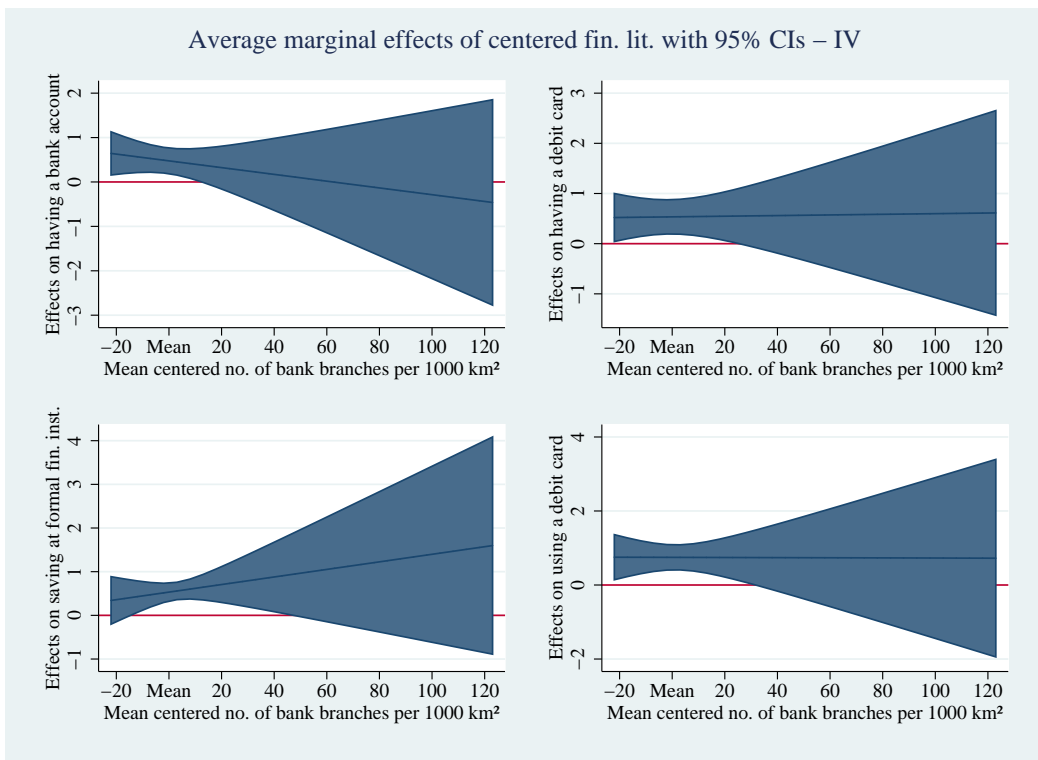


Figure C.4: Average Marginal Effects of Financial Literacy on Four Measures of Financial Inclusion at Different Levels of Bank Branches per 1000 km² (IV).

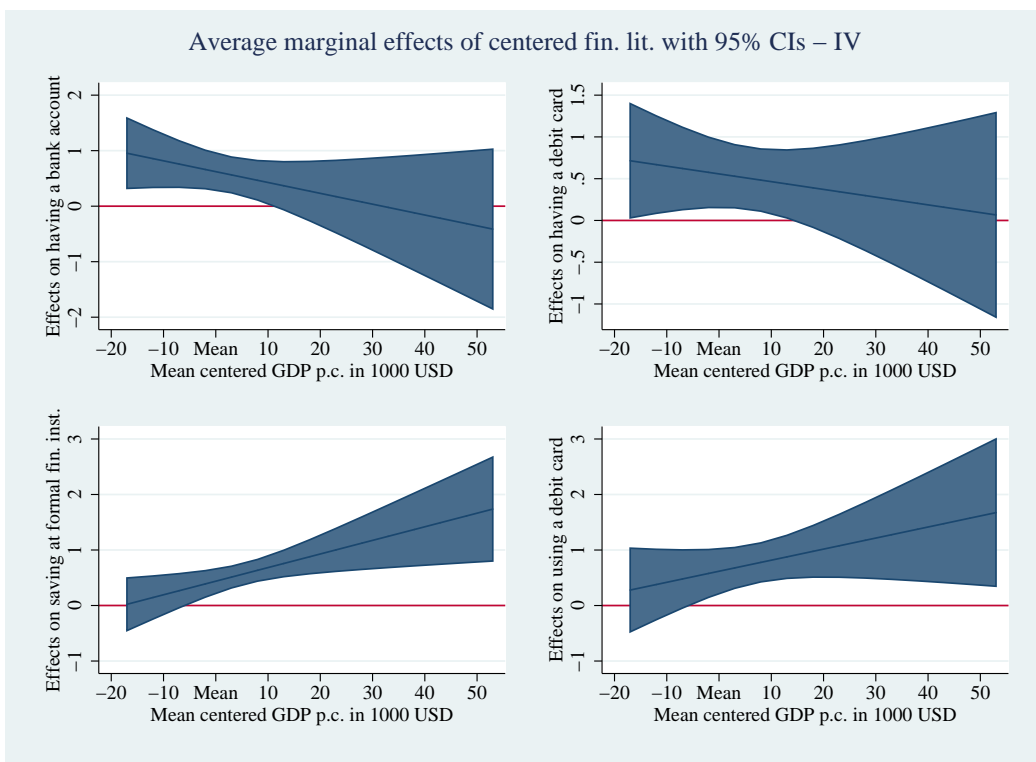


Figure C.5: Average Marginal Effects of Financial Literacy on Four Measures of Financial Inclusion at Different Levels of GDP per capita (IV).

Appendix D - Appendix for Chapter 4

Survey Appendix

Table D.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*** (0.120)	0.395*** (0.122)	0.232*** (0.066)	0.172** (0.068)	0.204*** (0.071)	0.182** (0.074)	0.053 (0.037)	0.072* (0.037)
Constant	-0.122** (0.048)	-1.218* (0.706)	0.179*** (0.022)	-0.269 (0.321)	0.278*** (0.029)	-0.836** (0.357)	0.135*** (0.019)	0.028 (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 D.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 (0.104)	0.173 (0.113)	0.134 (0.091)	0.181* (0.102)	0.025 (0.060)	0.049 (0.063)	0.108 (0.119)	0.151 (0.121)
Constant	-0.012 (0.055)	-1.168* (0.701)	-0.034 (0.048)	-1.942*** (0.562)	1.366*** (0.035)	1.147*** (0.436)	-0.097 (0.070)	-0.293 (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 D.3: Subsample: Income Expectation Bias Dummy - Over-Indebt. Indicators

	Over-indebteness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Bias Dummy	0.241** (0.101)	0.187* (0.109)	0.161*** (0.042)	0.123*** (0.042)	-0.002 (0.032)	0.000 (0.036)
Constant	-0.074 (0.055)	-1.155* (0.673)	0.134*** (0.019)	-0.100 (0.266)	0.141*** (0.020)	-0.286 (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 D.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*** (0.123)		0.186*** (0.064)		0.180** (0.083)		0.083** (0.035)	
Wider Bias (30%)		0.357*** (0.094)		0.136*** (0.050)		0.214*** (0.078)		0.070** (0.028)
Constant	-1.018* (0.570)	-1.101* (0.593)	-0.120 (0.266)	-0.137 (0.279)	-0.296 (0.382)	-0.397 (0.353)	-0.067 (0.225)	-0.085 (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 D.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 (0.103)		0.176* (0.092)		0.055 (0.061)		0.029 (0.111)	
Wider Bias (30%)		0.054 (0.082)		0.095 (0.074)		0.022 (0.047)		-0.020 (0.100)
Constant	-1.363** (0.562)	-1.326** (0.564)	-2.018*** (0.475)	-1.998*** (0.472)	1.023*** (0.349)	1.037*** (0.350)	-0.494 (0.668)	-0.453 (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 D.6: Wider and Narrower Bias Measures - Over-Indebtedness Indicators

	Over-indebtedness Index		DSR > 0.4 (=1)		Holds > 4 Loans (=1)	
	(1)	(2)	(3)	(4)	(5)	(6)
Narrower Bias (20%)	0.218** (0.101)		0.136*** (0.041)		0.006 (0.032)	
Wider Bias (30%)		0.158** (0.080)		0.108*** (0.033)		-0.004 (0.027)
Constant	-0.943* (0.537)	-0.961* (0.549)	0.070 (0.245)	0.048 (0.253)	-0.312* (0.176)	-0.304* (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 D.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*** (0.147)	0.401*** (0.147)
Fin. Forecast Error		0.027 (0.020)
Constant	-0.037 (0.241)	0.070 (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 D.8: All Biases - Interaction with Conscientiousness

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

Table D.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

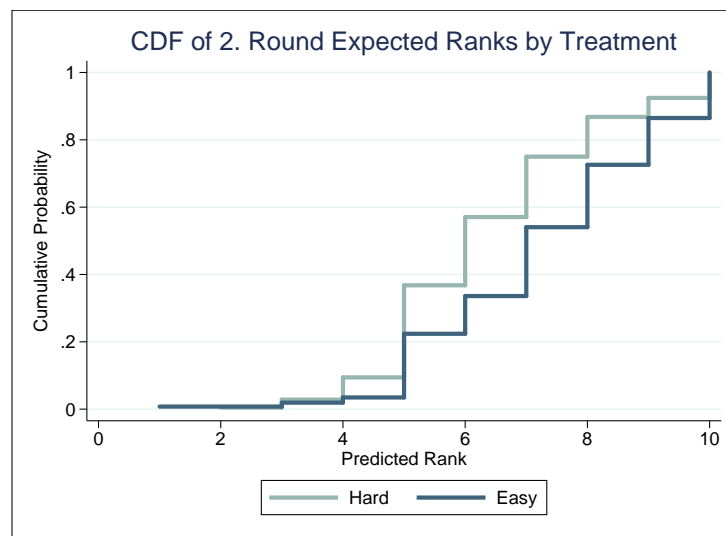


Figure D.1: CDF for the Expected Rank by Treatment, After the Main Quiz

Table D.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 D.9.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table D.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 D.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 “truly” 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 D.12).

Table D.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 (see Table D.13) (for non-rationals the difference is in the opposite direction, but not significant). Thus, these participants might have taken 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.

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 D.14). All other effects are almost unchanged.

Table D.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

Table D.14: Consumption Decision including Rationals

	Exp. Rank	No. Goods		
	(1)	(2)	(3)	(4)
Treatment	0.373** (0.168)	-0.234 (0.199)		-0.254 (0.199)
Exp. Rank			0.048 (0.052)	0.054 (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.

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 polychoric 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.
<hr/>	
Controls	
Age	Age of respondent in years.
Age Squared	Squared term of age.
BMI	Respondent’s 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	Respondent’s 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	Respondent’s 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 Conscientiousness 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.