Microcredit, financial literacy and household financial distress

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Abstract

This paper studies the effect of microcredit uptake on household financial distress. Drawing on quasi-experimental survey data collected in urban Uganda, merged with bank administrative data on the same individuals, we find that on average, microcredit uptake increases financial distress. The average impacts, however, conceal fundamental heterogeneity in treatment effects for different subpopulations. The financial distress-response to microcredit uptake is driven by borrowers whose financial literacy skills are low, and we are unable to reject the null of no impact for those with higher levels of financial literacy. Borrowers with low financial literacy levels take on loans (and installments) that are larger relative to their income. These findings are explained by a simple model of stochastic choice that incorporates financial literacy, and they suggest a role for numeracy skills assessment in credit scoring of loan applications.

Keywords: microcredit, financial distress, financial literacy, Uganda

1. Introduction

Whereas the spread of microfinance has relaxed credit constraints in many countries, concerns have increasingly been voiced about potential overborrowing

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by the poor (e.g. Roodman (2012); Angelucci et al. (2015); Fafchamps (2013); Schicks (2013)). The aim of this paper is to analyze impacts of microcredit uptake on financial distress in urban Uganda, and to identify sources of heterogeneity in these impacts. Household financial distress is an outcome not previously included in impact evaluations of microcredit.

The risk of overborrowing by households in developing countries is increasingly being recognized as a problem, fueled by crises in the microfinance sectors of Andhra Pradesh (India) and Bolivia, among others (CSFI, 2012, 2014). Household overborrowing and overindebtedness may also have contributed to the US subprime crisis of 2008 that led to the subsequent global financial crisis. Even in the absence of such crises, overindebtedness of households is a concern for various reasons. On the borrower side, overindebtedness and the associated household financial distress constitutes a welfare loss for households. For example, school dropout as a result of financial distress has long term consequences and so may distress asset sales in the presence of multiple equilibria due to asset-based poverty traps (Barrett & Carter, 2013; Carter & Barrett, 2006). On the lender side, overborrowing increases credit risk. Despite its critical role in understanding impact pathways of microcredit, surprisingly little rigorous research has been conducted on financial distress in the context of microcredit[1], and to the best of our knowledge, no rigorous microcredit impact evaluation has included financial distress in its outcome measures.

We develop a stochastic choice model of households making a borrowing decision. The latent propensity to borrow increases in the expected returns to borrowing, and the household borrows only if it perceives the expected returns to be positive. The model clarifies two points. First, if errors are made in the ex-ante evaluation of the expected returns to borrowing by the household, then microcredit may increase financial distress for those who take it up by fattening the left tail of the outcome distribution. Second, the model highlights how

[1] Several more descriptive studies of the relationship between microcredit and overindebtedness have been conducted, e.g. Guérin et al. (2013, 2015); Afonso et al. (2016).

this effect will vary across households with different financial literacy levels. In subpopulations with lower financial literacy levels, optimization failure is more prevalent, so that a larger share of people borrow for whom the expected returns are negative. Hence, with lower financial literacy, the impact of credit uptake is more adverse, especially in terms of its impact on financial distress.

The main contribution of this paper is empirical: we exploit quasi-experimental panel survey data collected in urban Uganda to estimate the impact of microcredit uptake on household financial distress. Since the exact subsistence consumption level will depend on household characteristics and given measurement error challenges with income data, in the empirical part of this paper we proxy for financial distress by discrete 'distress events'. At baseline, we interview active microcredit borrowers from a Ugandan microfinance deposit taking institution (MDI)[2], as well as a control group consisting of loan applicants who received their first loan after the baseline survey took place. A random subsample of the households is re-interviewed around 12 months later to create a panel dataset. On average, microcredit uptake increases financial distress in our sample. While our quasi-experimental design does not match the rigor of an RCT, various robustness checks, aided by management information system (MIS) data from the MDI, address possible concerns with respect to potential biases of the estimates. Overall, the results are shown to be robust and stable.

In addition, fundamental effect heterogeneity is identified, namely by financial literacy levels. For households with higher financial literacy skills, we are unable to reject the null of no impact, and the effects are strongest for those with low financial literacy levels.

Our findings contribute to two strands of literature. First, our findings contribute to the scant literature on households overborrowing and financial distress in the context of microcredit. An exception is Schicks (2013), who explicitly

[2]In Uganda, an MDI (tier III institution) differs from semi-formal microcredit institution (tier IV institution) in that it is regulated by the Bank of Uganda, and for instance partakes in its credit information sharing system through the Credit Reference Bureau.

tried to infer the debt burden of households. In her study, 531 microborrowers in Ghana were asked about a list of 'sacrifices' they had to make in the last 6 months to meet their microcredit repayment obligation(s) (see Subsection 3.1 for a detailed list). She then asked her sample respondents to classify each of these 'sacrifices' into acceptable and unacceptable sacrifices. A household was deemed overindebted if (a) it experiences a sacrifice that indicates structural problems (an asset seizure, loan recycling or selling/pawning assets) and (b) it makes unacceptable sacrifices repeatedly[3]. While such an approach is subjective in nature, its novelty and strength are that it looks at the problem from the client perspective by trying to directly infer the burden of debt across various dimensions of borrowers' lives in a quantitative manner[4]. There are however two, related, concerns with this approach. First, 'framing' or 'priming' the respondent on their repayment obligations might lead to confirmation bias. Second, Schicks only interviewed microcredit borrowers, so the attribution of the observed distress to microcredit cannot be ascertained (which is implicit in that approach). We address the first of these issues by avoiding to frame the - what we refer to as - 'distress events' as repayment struggles, and address the second issue by including a control group consisting of loan applicants.

Second, our findings fit into the literature on microcredit impacts. A series of Randomized Controlled Trials (RCT) have investigated the impacts of microcredit supply expansion in various settings. A meta-analysis of seven randomized trials found that the general impact of microcredit access on investment in self-employed activities is likely positive but small (Meager, 2015). A summary of results of six RCTs by Banerjee, noted there is no clear evidence of strong effects on higher-level outcomes such as household income, consumption, educa-

[3]Either >3 unacceptable sacrifices, or ≥ 1 unacceptable sacrifice made >3 times.

[4]Supply side measures, such as repayment delays and delinquency, may reflect debt-induced hardship to some extent, but do not provide the whole picture: delinquent borrowers may include those facing a short-term liquidity shortfall without necessarily facing a structurally unsustainable debt burden. Moreover, such figures also capture willful delinquency and default (which is potentially more important for non-collateralized loans and in weak legal systems

tion, or health (Banerjee et al., 2015b). One plausible explanation for the lack of large effects is that the small businesses that the households invest in have low marginal product of capital (Crépon et al., 2015). However, several studies do find an impact on profits for pre-existing businesses or for businesses at the top end of the distribution of profits (Angelucci et al., 2015; Banerjee et al., 2015a; De Mel et al., 2008). An alternative or complementary explanation for the lack of average effects is that microcredit may have opposing effects (in- or decreasing the expected solvency position and welfare of a household) under different initial conditions, and that cancellation of positive and negative effects within a pooled sample population may result in a lack of treatment effects[5]. It seems plausible that the expected solvency position of a household may respond positively or negatively to microcredit expansion, in a context of low and varying financial literacy levels. And even if for all of those who take up microcredit, the effect would be to improve the *expected* solvency position, the actual solvency status may not, given income shocks and investment risk. Moreover, noisy outcome measures, such as income, profits or expenditure, might compound power challenges in randomized trials of microcredit arising from small differences in take-up rates between treated and control(s) (areas). Discrete events-based outcomes such as the one in this study may have more power to detect gainers and losers from microcredit. The effect heterogeneity identified in this study at least suggests the possibility that individual treatment effects of opposite sign may (partly) explain the preponderance of null findings for the *average* treatment effects on income and consumption that characterize many RCT evaluations of microcredit expansion. The average impact of microcredit depends as much on lenders screening and selecting creditworthy borrowers as it depends on the average impact for those who take up a loan. Hence, the findings of this study suggest a role for numeracy skills assessment in the credit scoring process of loan applications.

[5] In other words, the assumption of *monotone treatment response* seems implausible for the case of microcredit.

The rest of this paper is organized as follows. The next section develops a stochastic choice model of microcredit impact and its heterogeneity, with a focus on financial distress. Section 3 describes the data and methods used to test the model's predictions. Section 4 reports the estimation results on the mean impacts of microcredit uptake in urban Uganda, with Section 5 describing a range of sensitivity analyses conducted to check for the robustness of those estimates. Section 6 reports on results regarding treatment effect heterogeneity, and Section 7 concludes.

2. A model of financial literacy and microcredit effect heterogeneity

Suppose an agent borrows only if she perceives the returns to borrowing to be positive, and she invests the full loan amount into a project with expected returns $\bar{r}$. The net interest rate i and the loan size L are assumed to be equal across individuals. Assume that for those who take up a loan, the expected returns to investment $\bar{r}_i$ are normally distributed in the population, with homoskedastic variance $\sigma^2_{\bar{r}_i}$. Assume furthermore that the interest rate offered is equal across individuals, as is the loan size. In the ex-ante evaluation, the individual makes errors in the computation of her expected returns to borrowing, so that the latent propensity to borrow is given by:

$$D_i * = (\bar{r}_i - i)L + V_i \tag{1}$$

where $V_i \sim N(0, \sigma^2_{V_i})$ is an idiosyncratic error due to errors in the individual's computation of her expected returns to investment, with $\rho(\bar{r}_{i},_{i}) = 0$. Assume a threshold-crossing model: the agent borrows only if she perceives the expected returns to be positive.[6] There are four cases:

$$D_i = \begin{cases} 1 \quad , D_i^* > 0 & \begin{array}{l} \nearrow \text{borrow with } (\bar{r}_i - i)L > 0 \text{ (case 1)} \\ \searrow \text{borrow with } (\bar{r}_i - i)L \leq 0 \text{ (case 2)} \end{array} \\ \\ 0 \quad , D_i^* \leq 0 & \begin{array}{l} \nearrow \text{does not borrow with } (\bar{r}_i - i)L > 0 \text{ (case 3)} \\ \searrow \text{does not borrow with } (\bar{r}_i - i)L \leq 0 \text{ (case 4)} \end{array} \end{cases} \tag{2}$$

Note that for the Average Treatment effect on the Treated (ATT), only case 1 and case 2 are relevant. In our empirical section, we sample only individuals who applied for credit (and were accepted later on by the lender) or those who already borrow, so we do not observe individuals who do not borrow. Suppose post-borrowing income is determined by the outcome equation[7]

$$Y_i = \beta(L, \bar{r}_i - i)D_i + y + \epsilon_i \tag{3}$$

with $\beta(.)$ a functional coefficient given by $\beta(L, \bar{r} - i) = (\bar{r} - i)L$. Since the loan size is equal across individuals, we can normalize it to 1, and have $\beta(L, \bar{r} - i) = \bar{r} - i$. Income in the absence of borrowing is composed of deterministic income y and stochastic income $\epsilon_i \sim N(0, \sigma_\epsilon)$. We further assume that $\rho(\bar{r}_i, \epsilon_i) = 0$. We assume the 'ignorability of treatment', i.e., the determinants of loan take-up are independent of unobservables ϵ_i

$$(\bar{r}_i, V_i) \perp \epsilon_i \tag{4}$$

This is also the identifying assumption in our empirical part, where we interview both already-borrowers and loan applicants who have not yet received

[6] Arguably, the expected returns to borrowing being positive would be a necessary (if the agent is risk neutral or risk-averse), but not sufficient condition to take up a loan. However, the results presented here hold without loss of generality, assuming that risk and time preferences are statistically independent from $\bar{r}_i, V_i$.

[7] Adding exogenous covariates X_i would not alter the crux of the model.

their loan, and assume the timing of credit take-up to be orthogonal to unobservable characteristics. Note that the model (3) implies *essential heterogeneity*: selection on unobserved gains from treatment (determined by $\bar{r}$).[8] The potential outcomes are

$$Y^0 = y + \epsilon_i \tag{5}$$

$$Y^1 = (\bar{r_i} - i) + y + \epsilon_i \tag{6}$$

where Y^1 and Y^0 are the potential outcomes for treated and untreated states. The potential outcome distributions are not easily derived[9], but Figure 1 plots an example of the potential outcome distributions when $y = 1$, $i = 0.7$, $r_i \sim N(0.7, 1.5)$, $V_i \sim N(0, 2)$, $\epsilon_i \sim N(0, 1)$,

For simplicity, define financial distress as a state wherein income Y_i is below subsistence level y_{min}:

$$\textit{Financial distress}_i = \mathbb{1}[Y_i < y_{min}] \tag{7}$$

where $\mathbb{1}[.]$ is the indicator function taking the value 1 if its argument is true and the value 0 otherwise. As long as y_{min} lies weakly to the left of the intersection point of the two cumulative distribution functions, microcredit uptake increases financial distress. In the numerical example given, this means that $y_{min} \leq -0.1 = Y^0 - 1.1 * \sigma_{Y^0}$.

Proposition 1. *Assume that equation* (1), (3) *and* (4) *hold, and that* y_{min} *lies to the left of the intersection point of the cumulative distribution functions of the potential outcome distributions. Then the ATT of the uptake of microcredit*

[8] Hence, in a (randomized) encouragement design of credit expansion, only the local average treatment effect (LATE) is identified (Heckman et al., 2006), inducing the statistical power problems when encouragement-induced loan take-up is low (partly due to the availability of substitutes), as discussed in the previous section.

[9] The distribution of Y^0 is simply $N(0, \sigma^2_{\epsilon_i})$, but the distribution of Y^1 is less tractable analytically, especially if $Var(r_i) \neq Var(V_i)$ or if $r_i \neq i$.

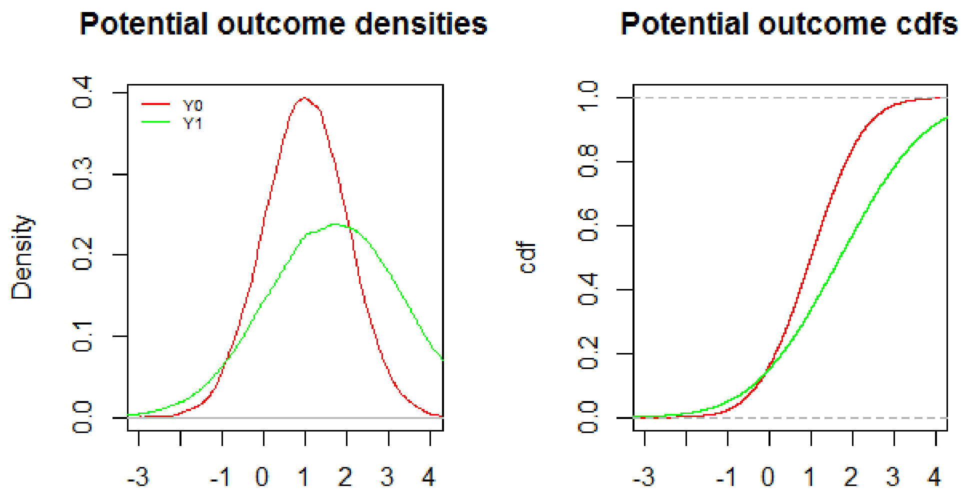

Figure 1: Marginal and cumulative probability distributions of the potential outcomes for treated (Y^1) and untreated (Y^0).

($L > 0$ rather than $L = 0$) on financial distress is positive, i.e., treatment uptake increases the incidence of financial distress.

In the empirical part of this paper, we construct a financial distress index that gives more weight to more severe distress events, akin to the Foster-Greer Thorbecke poverty measure with $\alpha > 1$. With such a financial distress measure as outcome, we would have that under the assumptions made ((1), (3) and (4)), the condition that y_{min} lies weakly to the left of the intersection of the potential outcome cumulative distribution functions (CDF), would be still be sufficient, but no longer be necessary for the ATT to be positive (i.e., for microcredit to be distress increasing in expectation). The reason is that more weight is given to observations (and counterfactual relocations) more in the extreme of the left tails of the counterfactual distributions.

Simulations for different values of $Var(V_i), Var(\epsilon_i), Var(\bar{r}_i), E[\bar{r}_i]$ reveal the following:

Proposition 2. *Assume that equation* (1), (3) *and* (4) *hold. Then the effect of the uptake of microcredit is more adverse in terms of its effect on fiancial distress if:*

- *the difference* $(E[r]-i)$ *is smaller (returns to capital in the population are lower and/or the interest rate is higher);*
- $Var(\bar{r}_i)$ *is larger (more heterogeneity in the returns to capital);*
- $Var(V_i)$ *is larger (more noise in the decision-making stage);*

2.1. The role of financial literacy

For the moment, reverse back to income being the outcome Y. Lower financial literacy levels are likely to lead to more errors (more noise) in an agent's computation of her returns to borrowing, i.e.:

$$\text{Finlit} \downarrow \Rightarrow \ Var(V_i) \uparrow \Rightarrow \ P(D_i = 1 \cap (\bar{r} - i) < 0) \uparrow \tag{8}$$

Hence, the probability (share of) case 2 individuals increases with lower financial literacy levels. The probability of Case 3 individuals also increases, but these individuals are not relevant for the ATT. The average treatment effect on the treated (with income as outcome variable) is given by

$$
\begin{aligned}
ATT &= E[Y_{1i} - Y_{0i}|D = 1] && (9)\\
&= P(D = 1 \cap (\bar{r}_i - i) > 0)Y_{1i} + P(D = 1 \cap (\bar{r}_i - i) \leq 0)Y_{1i} && (10)\\
&+ P(D = 1 \cap (\bar{r}_i - i) > 0)Y_{0i} + P(D = 1 \cap (\bar{r}_i - i) \leq 0)Y_{0i} && (11)\\
&= P(D = 1 \cap (\bar{r}_i - i) > 0)E[\beta(L, \bar{r}) + y|D^* > 0] + P(D = 1 \cap (\bar{r} - i) \leq 0)E[\beta(L, \bar{r}) + y|D^* \leq 0] && (12)\\
&= \underbrace{P(D = 1 \cap (\bar{r}_i - i) > 0)E[(\bar{r}_i - i)L + y|D^* > 0]}_{\text{Case 1}} + \underbrace{P(D = 1 \cap (\bar{r}_i - i) \leq 0)E[(\bar{r}_i - i)L + y|D^* \leq 0]}_{\text{Case 2}} && (13)
\end{aligned}
$$

Note that the sign of the ATT is ambiguous, depending on the share of Case 1 and Case 2 individuals, as well as the distribution of the expected returns to capital, determined by $E[r_i], Var(\bar{r}_i)$. For a given $E[r_i], Var(\bar{r}_i)$, the lower the level of financial literacy in the population, the more 'noise' in the decision stage, the higher the share of Case 2 individuals, the lower the ATT and the more likely the ATT is negative:

$$\text{Finlit} \downarrow \Rightarrow \; Var(V_i) \uparrow \Rightarrow \; P(D_i = 1 \cap (\bar{r} - i) < 0) \uparrow \tag{14}$$

By the same reasoning, the lower the financial literacy skills in a subpopulation, the larger the share of Case 2 individuals, and thus the lower the ATT. Thus, with financial literacy as conditioning variable, the conditional average treatment effect on the treated (CATT) increases monotonically in financial literacy levels.

The aforementioned discussion is about a 'neutral outcome' that affects each location in the outcome distribution equally. Increases in financial literacy lead to higher 'downside' errors (case 2) and 'upside' errors (case 3). However, case 3 is not relevant when considering the ATT. Therefore, for a given distribution of returns to capital $x, Var(\bar{r}_i)$, the more movements of observations to lower positions in the counterfactual (treated) outcome distribution there will be. Hence, the lower the level of financial literacy, the more adverse the effect of microcredit on household financial distress:

Proposition 3. *For a given $x, Var(\bar{r}_i)$, the lower the financial literacy skills levels, the greater the more adverse the ATT of microcredit uptake on financial distress.*

A positive correlation between financial literacy levels and the returns to capital/investment,

$$\rho(\text{finlit}_i, \bar{r}_i) > 0 \tag{15}$$

will lead to even stronger treatment effect heterogeneity along the financial literacy dimension, as $E[(\bar{r}_i - i)L|(\bar{r}_i - i) \leq 0]$ in equation (3) will be lower. All else equal, 'ability' - of which numeracy and financial literacy are one dimension or proxy, is likely to be correlated with the returns to capital[10].

3. Data and methods

3.1. Data structure

For data collection, we collaborated with a Ugandan Microfinance Deposit-Taking Institution ('the MDI' hereinafter) in order to have enough formal microcredit clients in our sample. Loan officers directed us to their borrowing clients. Which clients were visited depended on the schedule of the loan officer(s) within a month; little systematic bias is expected here since loan officers visit every group according to a pre-determined repayment frequency. Some bias may have arisen from only interviewing those present at the group repayment meeting, though efforts were made to track down and interview those group members not showing up at the group meeting. Loan officers also pointed us to the places of residence of first-time applicants for loans. Households were sampled in all 5 divisions of Kampala, as well as in the Wakiso, Luweero and Mukono districts surrounding Kampala. The final baseline sample consists of 714 respondents (and their households) interviewed between September 2013 and March 2014. Of the 714 respondent households, 116 were applicants at the MDI and the remaining 598 microcredit borrowers.[11]

A follow-up survey was conducted between October 2014 and March 2015 on a random subsample of respondents who were either formal borrowers or applicants for formal credit at baseline. Of the 304 households in the second

[10] Ceteris paribus, higher ability entrepreneurs are more credit constrained. Indeed, De Mel et al. (2008) and McKenzie & Woodruff (2008) found evidence for higher marginal returns to capital among microenterpreneurs with higher ability in Sri Lanka and Mexico, respectively.

[11] We excluded the 145-116=29 baseline applicants whom we could not identify in the MIS data, and whose loan applications were therefore, likely rejected.

wave, 209 already had a microcredit at baseline and 95 were applicant at baseline and had taken up their loan by the time of the follow-up survey. The main objective of our study is not to obtain unbiased estimates of the incidence of financial distress in a larger reference population, but rather to infer the average treatment effect on the treated of microcredit uptake on financial distress. We therefore follow (Solon et al., 2015) and do not use sampling weights[12].

3.2. Outcome measures

As touched upon in the Introduction section, our outcome measures focus on financial distress, which captures the symptomatic events of household dropping below subsistence level. It is thus a broader concept than overindebtedness, which refers to distress that is debt-induced or for which a high debt burden prevents a transition to a state of less distress.

Inspired by the approach of Schicks (2013), the following questions were asked to elicit respondents' level of financial distress[13]

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[12]That is, we do not use sampling weights to adjust for the sampling scheme. We will, later on, include weights to achieve covariate balance between treated and control units, using entropy balancing.

[13]In Schicks (2013), interviewers asked each respondent about the following list of sacrifices in relation to their loan obligations (% finding sacrifice unacceptable out of the borrowers who made each respective sacrifice):

- Reduce food quantity/quality (cut down eating) (73 %)
- Reduce education (e.g. taking children out of school) (80 %)
- Work more than usual (e.g. take additional paid labour or work longer hours) (32 %)
- Postpone important expenses (e.g. for health, housing, business assets etc.) (33 %)
- Deplete your financial savings (e.g. money in the house or in a savings account) (38 %)
- Borrow anew to repay (take an additional loan) (85 %)
- Sell or pawn assets (e.g. jewellery, cattle, productive or household assets) (90 %)
- Seizure of assets (MFI takes property by force to make up for missed payments) (100 %)
- Use family/friends' support to repay (e.g. monetary contribution or other help) (72 %)
- Suffer from shame or insults (also gossip about you/exclusion from a contract) (100 %)

(1) *assetless*: During the last 6 months, did your household have to sell any of its assets (i.e. land, motocycle, etc.) to meet other payment obligations?

(2) *eatless*: During the last month, did you eat less or of less quality than usual because of lack of money?

(3) *schoolless*: During the last 6 months, did you have to take out your children from school because of lacking funds?

(4) *healthless*: During the last month, was your household unable to pay for medicine/visiting the doctor because of lack of money?

(5) *run-out-of-money*: How often during the last 6 months, did your household run out of money from previous revenues before the next revenues arrived (e.g. wages)? Choose one response: every month, every other month, twice, once, never, don't know.

(6) *any-open-bill*: Do you (or any other member of your household) currently have any unpaid bills - open balance - and where? Do you have any open bill outstanding?

In contrast to (Schicks, 2013), none of the questions makes a direct link to debt or borrowing, to avoid priming effects (i.e. confirmation bias). In question 1, we mention 'payment obligations' to try to capture those asset sales that were induced by financial distress of the household (as opposed to sales due to reduced need for the asset or because a better substitute asset has just been acquired). We restricted ourselves to these six distress events (out of the 12 used by Schicks) because it is possible to ask them in a relatively objective way without having to 'frame' the question in terms of debt servicing. Seizure and sales of assets were viewed as some of the least acceptable by respondents in

- Feel threatened/harassed by peers/family/loan officer (100 %)
- Suffer psychological stress in your marriage (80 %)

(Schicks, 2013)', as was reducing education (deemed unacceptable by 80% of her sample respondents who made this sacrifice) and reducing food consumption (deemed unacceptable by 73% of respondents who made this sacrifice). The other three distress events used by us were not used by (Schicks, 2013).

The responses to some of the questions on distress events are subjective, in particular distress event (1)-(4) above. For the *eatless* event, different people may interpret 'less than usual' in a different way, possibly depending on the volatility over time of their disposable income. Moreover, money is fungible and the 'inability to pay' for an expense might also reflect a changing opportunity cost of the expense itself and of time. Borrowing-fuelled business expansion may for instance increase the opportunity cost of education and be a stronger driver of the 'schoolless' event than a lack of funds for some households. The extent to which this is the case depends on how forward-looking and risk-averse individuals are, i.e. to what extent uncertain payoffs from education far into the future are discounted. Arguably, the more severe the distress event and the longer lasting its potential adverse effects, the more a household will try to prevent it at all cost and thus the more it captures genuine distress. In this light, it is worth noting that the *schoolless* event of school dropout is mostly a unidirectional event: of all follow-up respondents, 37.8% dropped their child out of school, while only 3.66% answered affirmatively to the 'opposite' question: 'During the last 6 months, have you been able to put a child, who was out of school for at least one term, back into school?'. For the medical dimension of financial distress (*healthless*), there is some misclassification, as only 285 out of 855 respondents got sick or injured the last month; for those households with no member falling ill or getting injured, this indicator takes on the value of 0. Hence, for some households the distress index is artificially too low, but results are not strongly affected by this choice as we chow in the robustness checks. For the fifth question, the *run-out-of-money* indicator variable was coded as 1 if the respondent indicated to have run out of money at every other month (and 0 otherwise).

As a measure of financial distress, we combine the 6 indicators into a single index. To give distress events that are more serious for respondents more weight, we construct a continuously distributed financial distress index from the binary indicators using polychoric PCA (Kolenikov & Angeles, 2009).

3.3. Conditioning variable: financial literacy

For explaining financial distress risk, not only the mean of household income is important, but also the degree of volatility of income flows: for two households with identical characteristics including average income, the household whose income is more volatile has a higher likelihood of experiencing (more severe) financial distress. We therefore asked respondents to what extent they considered their household income stable (answer options: very stable, stable, unstable, very unstable). Financial literacy has been identified in the literature as a correlate of overindebtedness (measured as amount of credit card debt, arrears on consumer credit and self-reported excessive financial burdens of debt) in the UK (Gathergood, 2012) and in the US (Lusardi & Tufano, 2009). However, financial literacy was unrelated with Schicks (2013)' measure of overindebtedness in her sample from urban Ghana. More generally, randomized trials of financial literacy programs have been conducted in various countries to evaluate their impacts. Cole et al. (2009) offered randomly selected unbanked households in India and Indonesia financial literacy education, and found modest increases in the likelihood of opening a bank account for uneducated and financially illiterate households. The likelihood of opening a savings account was more affected by offering a small monetary incentive than by the financial literacy training. Our proxy for financial literacy levels was constructed as the sum of correct answers to a set of basic numeracy questions, and a set of five questions involving percentages and interest, slightly adapted from Bandiera et al. (2010). The questions whose answers make up the financial literacy score are listed in Appendix A.

3.4. Econometric approach and identification strategy

To identify the impact of the take-up of microcredit, it helps to consider first an idealized experiment, in which one would randomly deny credit to some applicants and not to others. The problem with this approach is twofold: (a) very few lenders would want to turn down creditworthy borrowers and (b) rejected clients may look for substitutes, contaminating the clean counterfactual. Actual randomized trials of microcredit have therefore mainly focused on estimating impact of changes in access to, rather than uptake of microcredit. The approach closest to our design is that of randomizing the credit applications of marginally rejected (or accepted) loan applicants into loans. Karlan & Zinman (2009), the first ones to our knowledge to implement this approach, did so in the Phillipines.

The approach we take is to estimate the impacts of take-up of microcredit by comparing applicants for microcredit to those who already received it. A key underlying assumption is that the client recruitment and self-selection mechansisms are constant over time. We will scrutinize and evaluate this assumption in our robustness checks. Potential advantages of this approach compared to randomizing credit access to marginally uncreditworthy applicants, are (i) impacts are estimated not just for marginal but for all borrowers (Wydick, 2016), and (ii) statistical power challenges due to rejected applicants taking up substitutes, are alleviated (Banerjee et al., 2015b).

A potential limitation of our approach over the individual randomization approach is the potentially less clean counterfactual due to the lack of pretreatment data for those who are already borrowing at baseline. For this reason, various robustness checks in Section 5 address concerns regarding the identification of the causal estimands.

To examine the effect of microcredit uptake on financial distress, then, we first use the baseline data to estimate regression models of the form

$$y_i = \beta \times microcredit_i + X_{it}\gamma + \epsilon_i \tag{16}$$

where y_i is the continuously distributed financial distress for household i, $formal_i$ takes on 1 if the household contains a formal borrower and 0 otherwise, and X_i is a vector of controls. The key assumption underlying this approach is that the selection mechanism into microcredit uptake is time-invariant over the time span considered[14]. To relax this assumption, a second type of analysis is conducted on the longitudinal data:

$$y_{it} = \beta \times microcredit_{it} + X_{it}\gamma + \alpha_i + \epsilon_{it} \tag{17}$$

where α_i are household fixed effects. The fixed effects filter out any time-invariant differences between treated (those having microcredit at baseline) and controls (those applying for credit at baseline). Such characteristics that could plausibly be considered time-invariant (over the course of a few years) include the applicant's enterpreneurial ability, risk preferences, and the personality of loan officers. The identifying assumption for the fixed effects estimation is that selection into microcredit in terms of individual-specific trends in financial distress is time-invariant. More on this in the Robustness checks section.

3.5. Control variables

In line with the literature, we included standard demographics, such as the sex, age, religion and education level of the respondent.

A household's initial (i.e., pre-borrowing) socio-economic status is an important potential confounder, as it would influence both borrowing as well as financial distress. However, measures of household income often contain a lot of ill-behaved measurement error, with potential biased estimates as a result (Azzarri et al., 2010; Millimet, 2011). Furthermore, post-treatment bias would be a concern, as one of the likely mechanisms through which borrowing potentially affects financial distress is through changes in income, and we do not have pre-treatment income data for the subsample of borrowers. Household asset counts are likely to be more stable over time then income and subject

[14]For those having (had) microcredit at baseline, the variable 'years since becoming customer' has a mean of 3.6, a median of 1, an interquartile range of [0,4], and a maximum of 40. In Section 5, we show that estimates are relatively stable when excluding observations at lower or upper parts of the support of this variable.

to less (ill-behaved) measurement error. It also allows us to increase statistical power by being able to use more observations (some households were not able to come up with reasonably accurate income numbers). The wealth index was constructed based on asset counts on 25 assets, including non-productive assets and housing characteristics (the assets are listed in Appendix A). Using polychoric PCA, a wealth index was constructed as the linear combination of those asset indicators that maximize the proportion of explained variance in this first component. This proxy for socio-economic status is not perfect either as it is potentially endogenous: through the *assetless* indicator in the distress index, assets feature in both the regressand as well as in a regressor. However, omitting the wealth index as a control variable in regressions of distress on borrowing would leave out a potentially important confounder. Excluding the wealth index from the regressions hardly changes our results[15].

Income volatility is a potential confounder, as ceteris paribus, a household whose income is more volatile, is more likely to experience financial distress. We therefore include a self-reported measure of income volatility, with indicator variables for respondents considering their income flows to be 'very stable', 'stable', 'unstable', or 'very unstable'. Moreover, an indicator variable (referred to as 'Shock took place') is included in the regressions taking on the value of 1 if at least one of the following 4 events occurred: (1) a member of the extended family fell sick and the respondent had to pay for larger medical or hospital expenses during the last month, (2) a household member lost his or her job during the last month, (3) a member of the extended family got married for which the respondent had to pay a substantial amount of money during the last month, (4) a large and special household expenditure was made (related to weddings and funerals of members of the household) during the last 30 days. Informal insurance may aid in smoothing consumption when income is stochastic. The

[15]Across the 5 estimation methods used in the mean effect estimates in this paper, the coefficient on microcredit uptake always retains its statistical significance, and the change in percentage points is at most 1.0%

endogenous timing of the payout in Rotating Saving and Credit Associations (ROSCAs) has been shown to have such an insurance function Klonner (2003); Fang et al. (2015). We therefore also include as control an indicator variable for ROSCA membership. Note that we do not control for other (informal) loans the household may have outstanding at the time of the interview in order to prevent the introduction of post-treatment bias.

The timing of the baseline survey may matter due to seasonality and common shocks. Therefore, other variables included (not reported in the regression tables) are (i) a count variable taking on the value of 1 if the baseline survey took place in September 2013, the value of 2 if the baseline survey took place in October 2013, and so on up to March 2014; and (b) this variable squared.[16].

For all applicants, and a large share of those borrowing at baseline (79% of those borrowing at baseline[17]), we are able to link the survey respondents to the clients in the MIS data. This allows the inclusion of loan officer fixed effects. The loan officer fixed effects serve a dual role: first, they control for geographical effects as each loan officer serves an area unit that does not overlap with that of other loan officers; and second, since loan officers give out one type of loan product, it also controls for loan type.

4. Results

4.1. Descriptive statistics

Table 1 reports summary statistics. The average number of distress events experienced in our sample during the six months preceding the interview is 1.72

[16]Another approach, adding a dummy control variable for each calendar month in which the baseline survey took place, produced very similar results. Because the latter approach resulted in a lower goodness-of-fit, we opt for the approach described in the main text instead.

[17]We suspect that for at least some of the individuals interviewed at baseline whom we were not able to link to the MIS data, borrower status may have been misreported by the respondent, or miscoded by an enumerator. In the estimations that include loan officer fixed effects, the treated observations that we were unable to link to the MIS data are dropped, so those estimations also constitute a robustness check against such possible miscoding.

(st. dev. 1.25). With 56% of the baseline sample having at least once been unable to pay for medicine when falling ill or getting injured one month prior to the interview, the level of financial distress in this sample appears high. Having to cut back on food consumption and frequently running out of money before the next income stream arrives[18], are also fairly commonly experienced distress events; distress asset sales and distress school dropouts are less common. The age of the respondent and household size are higher for treated than for control households, which is what one would expect if the average age of loan take-up were not to change over time. This may also possibly explain why the treated are more often heads of the household even though the gender of the respondent is not statistically significant, and why the wealth index is higher among treated households (treated individuals have run their business for longer). In Section 5, we report also estimates wherein for instance we restrict the treated subsample to those having been client for at most 5 years. In such a restricted sample, the difference in mean for the wealth index between treated and controls is much smaller and not statistically significant anymore. The numeracy score variable is somewhat higher at baseline among applicants than among borrowers, but in the panel regression the household fixed effects will control for those differences.

To give a sense of the institutional context, the average loan size in the baseline sample amounted to UGX 852,000[19], and the baseline sample average of the average annual interest rate (which is either simple or on a declining balance), is 58.1% (st. dev. 12.28%). Virtually all loans are group loans, some of them with joint liability substituting collateral requirements. Of the baseline sample, 63% of borrowers repay their loans on a biweekly basis, and 37% on a monthly basis.

[18] This might not (exclusively) be reflective of financial distress, but part of running a business for a low-income household.

[19] $\approx$ USD 908, using the IMF PPP conversion rate of 31 December 2013. The variable loan size has a standard deviation of UGX 110,000, a minimum of UGX 50,000 and a maximum of UGX 20,000,000.

Table 1: Summary statistics for the baseline data.

	Obser-vations	Mean (St. dev.)	Min.	Max.	Mean treated	Mean controls	difference p-value[(1)]
Outcome variables							
Financial distress index[(2)]	714	-0.008 (0.984)	-1.324	2.849	-.006	-.180	0.049**
healthless	714	0.199	0	1	0.217	0.103	0.053*
healthless (those ill/injured)[(3)]	255	0.557	0	1	0.580	0.387	0.005***
schoolless	714	0.140	0	1	0.151	0.086	0.079
assetless	714	0.101	0	1	0.112	0.043	0.027
eatless	714	0.462	0	1	0.460	0.474	0.839
run-out-of-money	714	0.465	0	1	0.448	0.552	0.042
any-open-bill	714	0.584	0	1	0.610	0.448	0.001
# of distress events experienced	714	1.951 (1.420)	0	6	1.998	1.707	0.0430*
Treatment variable							
Microcredit	714	0.838	0	1			
Conditioning variable							
Financial literacy score	714	6.071 (2.360)	0	10	6.000	6.440	0.066*
Control variables							
Female	714	0.783	0	1	0.786	0.767	0.712
Head of household	714	0.584	0	1	0.609	0.457	0.003***
Age in years	713	38.335 (10.891)	19	82	38.716	33.661	0.000***
Household size	714	5.706 (2.815)	1	37	5.723	4.922	0.004***
Muslim	714	0.202	0	1	0.202	0.198	1.000
Primary education completed	714	0.363	0	1	0.370	0.328	0.401
Secondary educ. completed	714	0.305	0	1	0.313	0.267	0.379
Tertiary educ. completed	714	0.071	0	1	0.074	0.060	0.698
Wealth index[(4)]	712	6.091 (3.086)	0.733	20.027	6.238	5.333	0.004***
Member of a ROSCA	714	0.714	0	1	0.734	0.612	0.000***
Shock took place	714	0.580	0	1	0.590	0.526	0.284
Income reported 'very stable'	714	0.025	0	1	0.028	0.009	0.213
Income reported 'stable'	714	0.382	0	1	0.393	0.328	0.064
Income reported 'unstable'	714	0.522	0	1	0.510	0.586	0.064
Income reported 'very unstable'	714	0.069	0	1	0.067	0.078	0.064

(1) Reported is $P(|T| > |t|)$ for count or continuously distributed variables, and p-values from two-sided Fisher's exact test for binary variables.

(2) The construction of the financial distress index is explained in the main text.

(3) The subsample of respondents who got ill or injured in the month prior to their interview.

(4) The assets for the wealth index are shown in Appendix A, they have empirical weights based on PCA.

4.2. The mean impact of microcredit uptake on financial distress

Results on the average treatment effects of microcredit uptake on financial distress from both the cross-sectional and the panel data analyses are reported in Table 2. The cross-sectional analysis on the baseline data has the advantage of a sample with a larger number of observations, whereas the panel data analysis has the advantage of filtering out any time-invariant unobservables.

The results from both types of analyses coincide qualitatively, but the effect size estimate from the panel fixed effects estimation is larger (in absolute value) than the corresponding OLS regression partial effect estimate. In addition to OLS and linear panel regressions, we also run these estimations after entropy balancing (Hainmueller, 2011). Entropy balancing relies on a maximum entropy reweighing scheme that calibrates unit weights so that the reweighted treatment and control group satisfy balance conditions for the first, second and third sample moments of the covariates as well as all their pairwise interactions. Hainmueller (2011) show that after such reweighting, the treatment effect estimate based on observational data comes very close to an experimental benchmark. The procedure has been shown by Zhao & Percival (2016) to be doubly robust: if either the (logit) propensity score model or the outcome regression model is correctly specified, the mean causal effect estimator is consistent. Whereas the estimation approach works very well if either the outcome or propensity score model is specified correctly, entropy balancing is biased when neither is specified correctly, but the inverse probability weighing (IPW) estimator is unbiased in this case (Hirano et al., 2003; Zhao & Percival, 2016)[20]. Therefore, we also estimate the ATT nonparametrically by means of augmented inverse probability weighing (AIPW) (Rubin & van der Laan, 2008). In the tails of the propensity score distribution, overlap may be limited, which affects estimates negatively in terms of precision and bias. In the estimates of columns (2) and (3), as advised by Crump et al. (2009), we therefore also trim observation lying outside of the

[20]On the other hand, if either model is correctly specified, IPW can be off the mark, and performs worse than entropy balancing (Zhao & Percival, 2016).

interval $[\alpha, 1-\alpha]$ to increase precision and robustness of the estimates. Their data-driven method gives $\alpha = 0.08$, dropping 32% of observations.

Reassuringly, all estimates come to qualitatively the same conclusion: microcredit uptake increases financial distress on average.

Table 2: Average treatment effects on the treated of microcredit uptake on the financial distress index.

	(1) OLS	(2) OLS w/loan officer FE	(3) Entropy-bal OLS	(4) Augmented IPW	(5) Panel hh FE
Microcredit	0.265***	0.305***	0.305**	0.383***	0.265**
	(0.078)	(0.070)	(0.121)	(0.082)	(0.110)
Female	0.231***	0.208**	0.017		
	(0.087)	(0.098)	(0.122)		
Head of household	0.186**	0.185**	0.054		
	(0.074)	(0.071)	(0.113)		
Age in years	0.004	0.003	0.034***		
	(0.003)	(0.005)	(0.009)		
Household size	0.036***	0.036***	0.026		0.028
	(0.012)	(0.009)	(0.027)		(0.026)
Muslim	0.097	0.102	-0.096		
	(0.081)	(0.095)	(0.093)		
Completed primary education	-0.026	-0.004	-0.076		
	(0.090)	(0.063)	(0.116)		
Secondary education	0.059	0.076	0.072		
	(0.097)	(0.139)	(0.139)		
Tertiary education	-0.104	-0.114	-0.177		
	(0.138)	(0.152)	(0.228)		
Financial literacy score	-0.016	-0.019	0.000		
	(0.014)	(0.013)	(0.024)		
Wealth index	-0.039***	-0.044***	-0.048**		-0.034
	(0.011)	(0.011)	(0.021)		(0.028)
Member of ROSCA	-0.272***	-0.253***	-0.230*		-0.239*
	(0.075)	(0.092)	(0.134)		(0.133)
Household faced a shock	0.351***	0.281***	0.301***		0.248***
	(0.065)	(0.060)	(0.086)		(0.095)
Income reported stable[(4)]	0.358***	0.361**	0.211		-0.040
	(0.129)	(0.141)	(0.245)		(0.235)
Income reported unstable	0.999***	1.012***	0.876***		0.954***
	(0.133)	(0.129)	(0.251)		(0.250)
Income reported very unstable	1.449***	1.499***	1.063***		1.664***
	(0.177)	(0.146)	(0.321)		(0.303)
Loan officer FE		✓	✓		
Household FE					✓
Obs.	711	711	459	471	1309
R^2	0.32	0.30	0.50		0.34
BIC	1850.35	1794.28			1273.77

(1) * p<0.1, ** p<0.05, *** p<0.001
(2) Standard errors in parentheses; robust standard errors in columns (1),(2),(5).
(3) In columns (3) and (4), the sample is trimmed to the [0.08,0.92]-propensity score interval to increase precision and robustness, following Crump et al. (2009).
(4) Income reported 'very stable' is the left-out category.

5. Robustness of the mean effect estimates

The inclusion of loan applicants as a control group for microcredit borrowers addresses an important source of potential selection bias: the non-random self-selection by individuals into microcredit. Still, given the observational, quasi-experimental nature of the data, various concerns may linger regarding the strength of identification and remaining sources of bias in the estimates of interest. We will discuss and address these one by one. The coupling of the survey data with the MIS data allows for assessing some of the identifying assumptions.

5.1. Reverse causation

What comes first: borrowing or distress? Since our outcome variable reflects distress in the 6 months preceding an interview, the clearest counterfactuals are obtained when restricting the sample of those borrowing at baseline to those who received their first loan at least 6 months ago, as the possibility of reverse causation can then be excluded. The panel analysis is such that in survey wave 1, the treated group consists of the 'borrowers', while the applicants constitute the control group. By the time wave 2 arrives, both groups are in a treated state. Some of the applicants may have applied (partly) because they were in distress prior to this 6-month window preceding loan take-up, but this holds too for those who are already borrow before wave 1: some of them may have applied for their first loan because they were in financial distress in the months preceding their first loan take-up.

To obtain estimates free of reverse causation bias, we exclude baseline borrowers who took up their loan less than 6 months prior to the baseline survey. The estimates, reported in Table 3, reassuringly, are similar to those reported in the previous section (Table 2). There could still be selection on *trends* in the dependent variable, e.g. systematic pre-treatment trends, that would bias estimates. We get to this issue in Section 5.4

5.2. Survivorship bias

One potential source of sample selection bias in the first survey wave is that, whereas we (tried to) interview all loan applicants available, for the subsample of formal borrowers we only sample those who are still active borrowers. We did try to locate ex-clients, but the success rate in finding (and interviewing) them, is lower than for borrowers who are still active clients (i.e., who still have a loan outstanding at the time of the baseline survey). The reason is that loan officers change branch, ex-clients change phone numbers and move to a different part of the city or outside of the city, etc. Whenever this process is non-random, and the 'positive' and 'negative' loan drop-outs do not happen to cancel out, then survivorship bias would bias estimates comparing applicants and borrowers. Clients may stop borrowing (earlier compared to other clients) because of 'positive' reasons, for instance, find a cheaper substitute source of capital; or because they they do not 'need' a loan anymore due to re-investing increased business profits obtained through loan-induced investments that were successful. Alternatively, they could have stopped borrowing because of 'negative' reasons: they did not have a good repayment record on their loan, thereby diminishing or eliminating the dynamic incentives (of repeat-borrowing with greater loan sizes upon good repayment record); they were able to stay within terms but only by making severe 'sacrifices' (possibly due to being hit by an adverse shock); they did not (perceive to) reap side-benefits from being a member of a borrowing group (e.g., increases in social capital (Karlan & Zinman, 2011)); or even because they defaulted during a loan cycle. Especially if, on average, individuals stop borrowing because of distress, this would be unfortunate given our special emphasis on this outcome variable. Since we found that borrowers experience more distress than applicants, such distress-induced reduced likelihood of survivorship would have could lead to a downward bias in the effect estimate.

A client dropout study conducted by the MDI found that the reasons clients give for departing tend to relate more to the lending methodology than problems with loan repayment. Most drop-outs complain about inadequate loan amounts

Table 3: Sensitivity analysis with respect to time passed since becoming customer for baseline borrowers. The rownames refer to the restriction on the subsample of those having microcredit at baseline.

	OLS w/ loan officer FE	Entropy-bal. OLS	AIPW	Panel hh FE
>6 months	0.322*** (0.08) [n=599]	0.300** (0.13) [n=375]	0.252*** (0.08) [n=506]	0.432*** (0.135) [n=446]
<5 years	0.313*** (0.08) [n=561]	0.316** (0.12) [n=428]	0.254*** (0.09) [n=350]	0.492*** (0.14) [n=428]
>6 months & <=5 years	0.347*** (0.09) [n=450]	0.316** (0.13) [n=344]	0.255*** (0.08) [n=491]	0.464*** (0.14) [n=364]
<=3 years	0.301*** (0.08) [n=509]	0.330*** (0.13) [n=396]	0.230*** (0.08) [n=403]	0.497*** (0.14)
>6 months & <=3 years	0.333*** (0.11) [n=398]	0.333** (0.14) [n=312]	0.234*** (0.09) [n=316]	0.467*** (0.14) [n=346]
>1 year	0.327*** (0.10) [n=487]	0.319** (0.14) [n=291]	0.271*** (0.08) [n=421]	0.407*** (0.14) [n=372]

(1) * p<0.1, ** p<0.05, *** p<0.001

Standard errors in parentheses; robust standard errors in columns (1) and (4).

The models correspond to those of columns (2)-(4) of Table 2, and the set of control variables is the same.

and terms, mandatory weekly meetings, and having to pay for group members who default.

The longer the (average) time lapsed since taking up the loan for the subsample of formal borrowers, the more severe the survivorship bias problem would plausibly be, as a relatively larger share of initial borrowers will have stopped borrowing. Therefore, as a robustness check, we gauge the sensitivity of the effect estimates to restrictions on the subsample of borrowers with regards to the number of months or years that passed since they became client, derived from the MIS data[21]. The estimates, reported in Table 3, show that the coefficient on microcredit remains rather stable for different subsampling definitions. For a given estimation method, i.e., within each column of Table 3, none of the coefficients on microcredit uptake statistically significantly differ from each other. This suggests that survivorship bias is not much of an issue.

As another way to examine potential survivorship bias, we merge the baseline survey data with MIS data of 31 October 2013 as well as MIS data of 31 March 2014. The dependent variable takes on the value of 1 if the individual is still present in the data on 31 March 2014 (which is the case if and only if he or she is still borrowing), and 0 otherwise. We regress this attrition indicator on the financial distress index and a set of controls, including the time passed since becoming client until the date of the survey, and the time passed since the date of the survey and the 31st of March 2014. The results, presented in Table 6 in Appendix B, are mixed. The overall picture that arises is that those who are most likely to remain borrower are male, have tertiary education, have relatively stable income flows, and have loans with joint rather than individual liability. Experiencing a shock is associated with a higher likelihood of dropout. Conditional on the covariates, the coefficient on the financial distress index is close to zero however, and not statistically significant, indicating survivorship bias may not be much of a concern.

[21] The date the individual became client in the system is the date they registered their first loan application at the MDI.

5.3. Non-randomness of the timing of microcredit take-up

The quasi-experiment we consider is that two individuals apply for credit at two different points in time, a few years apart, and that their difference in timing of applications is (quasi-)random. A concern however, may be that individuals with more promising investment projects (higher expected returns $\bar{r}_i$ as in our theoretical model) would apply for microcredit 'earlier' after a branch opens. Furthermore, loan officers may first recruit the most promising and least risky clients, and later on, when local credit markets are relatively more saturated and the least risky borrowers already have a microcredit, are forced to recruit more risky potential clients or potential clients with lower returns to capital. On the other hand, those who apply later may be less risky as some of them are 'recommended' to a recruiting loan officer by existing clients as is not uncommon at the MDI. If social networks are formed by assortative matching on aspects that are correlated with delinquency risk, then loan officers may choose to take recommendations from long-term, successful clients who recommend them other low-risk potential clients.

Such non-random differential temporal selection into microcredit would be a form of unobserved confounding, potentially generating omitted variable bias. To the extent that (potential) borrowers' unobserved attributes such as time, risk and ambiguity preferences, are time-invariant, they would be filtered out by the household fixed effects in the panel data analysis. The same holds true if the factors affecting the expected return to investment are time-invariant over the course of a few years (think of educational attainment, entrepreneurial skills, etc.). But for the baseline data analysis, it would still be a concern.

Fortunately, we have MIS data from the MDI to shed light on the direction, if any, of the evolution of the selection mechanism. From October 2010 until January 2014, we consider the branches we sampled from for the loan products that are part of our research. The number of borrowing clients has declined slowly but steadily (perhaps due to the opening of new branches and 'transfer' of some clients to those new branches), from 7725 in October 2010 to 6803 in January 2014. But over the same period, the delinquency rate (> 0 days late on a loan) has increased monotonically over time, from 7.96% in October 2010 to 14.01% in January 2014. Hence delinquency risk has increased, perhaps due to increasing credit market saturation in Kampala and the saturation of lower risk potential clients that can be recruited by loan officers. Indeed, the urban Ugandan credit market has been perceived by MDI CEOs as competitive (McIntosh et al., 2005). If over time, more risky borrowers are selected into credit uptake, then the cross-sectional estimates reported here, if anything, underestimate the true effect of microcredit uptake on financial distress.

To test for changes over time since a branch starts lending, for instance learning effects, we add a variable for months passed since the branch of the MDI opened as a covariate to the financial distress regression[22]. But this variable's coefficient does not reach statistical significance and is close to zero. We also conduct sensitivity analyses with respect to selection on unobservables that we will now report.

Another possible concern is that not all individuals may apply at random times during their (productive) lives. Some individuals may apply for credit when their socioeconomic status or business is in a dip (á la Ashenfelter dip) (Ashenfelter, 1978; Heckman & Smith, 1999). This would invalidate the assumption of no systematic selection based on individual-specific trends in financial distress that underlies our panel data analysis. Note however, that in the subsample of the MDI covering the branches considered in this study, the median number of days passed since becoming client to receiving a loan is 28 days (interquartile range: [13,182]). This suggests that the formal microloans considered in this study are unlikely to be useful in making up for liquidity shortfalls. Second, the financial distress index reflects distress experienced not in the last weeks but over a longer period of the last 6 months, thereby diluting any Ashenfelter dip type selection. If, in spite of the aforementioned facts, some of individuals do select into credit due to them experiencing financial distress, then this would

[22]The branches under consideration existed for an average of 5.25 years (63.6 months, minimum 22 months, maximum 100 months) at the time of the baseline survey.

bias the estimated coefficients downwards, and the coefficients could be interpreted as lower bounds on the true effect. Such pre-treatment dip would likely show up in running out of money or having open bills. However, when excluding the 73 applicants from the analysis who ran out of money at least twice, the estimated effect if anything, is stronger (coef. 0.656; st. error 0.119, $p<0.01$). Similarly, when excluding the 58 applicants who have any open bill, the effects get stronger (coef. 0.525; st. error .057, $p<0.01$)[23]. This reiterates that the estimates of the impact of microcredit uptake on household financial distress in this paper are, if anything, conservative.

We conduct a placebo test by interacting the treatment indicator with a variable measuring time lapsed (in days) between the survey and the date the new client receives her loan. In the absence of a systematic pre-treatment trend, one would expect this interaction term to not be statistically significantly different from zero. Reassuringly, this is the case, see Table 7 in Appendix C.

5.4. Additional robustness checks

We test the stability of the effect estimates with regard to the covariate set, by conducting the Sala-i Martin (1997)'s extension of Leamer (1985)'s Extreme Bounds Analysis. The results are reassuring: in 98% of the covariates subset permutations, the coefficient on microcredit in the estimations on the baseline data, is of the same sign. See also the density of the coefficient estimates in Figure 2. As a second sensitivity analysis, we apply a method developed by Oster (2014), which analyzes coefficient stability and movements in R^2 when control variables are added to the regression. The assumption is that selection on unobservables is proportional (governed by a coefficient of porportionality, δ) to selection on observables. A value of $\delta = 1$ would imply equal selection on observables and unobservables. More generally, assuming proportional selection on observables and unobservables, a positive δ implies that the coefficient estimate is biased away from zero by selection on unobservables, whereas a negative

[23]Both of these estimations are linear regressions on the baseline data that include loan officer fixed effects.

δ implies that the coefficient estimate is biased towards zero. After specifying a maximum R^2 that would be obtained if all confounders were included in the regression equation (an $R^2 = 1$ is not realistic given measurement error), the δ can be estimated. For $R^2_{max} = 0.7$ we obtain a negative value of $\hat{\delta} = -28.6$, and for $R^2_{max} = 0.9$, we obtain $\hat{\delta} = -19.6$. Assuming proportional selection on observables and unobservables, we can thus conclude the following with regard to the coefficient on formal borrowing in the financial distress regressions: the positive and significant estimate in the sample of formal borrowers and applicants is a lower bound on the true effect. The panel fixed effect regressions can in a way (namely, when proportional selection on observables and unobservables is assumed) also be seen as a sensitivity analysis with respect to bias from selection on unobservables, as the household fixed effects filter out all time-invariant household-level unobservable differences between formal borrowers and applicants.

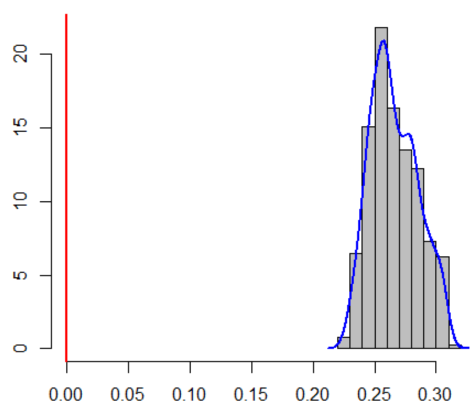

Figure 2: Sala-i-Martin's extended extreme bounds analysis: distribution of the coefficient on microcredit in regressions under all possible covariate subsets.

A final robustness check pertains to two of the distress events making up the financial distress index: the *healthless* and *any-open-bill* events. As discussed in Section 3.2, the *healthless* is miscoded for some respondents who did not get ill or injured in the last month; and the *any-open-bill* event may capture purchasing business inputs on credit and having open bills out of convenience.

We construct three additional financial distress indices using polychoric PCA: one that omits the *healthless* event, one that omits the *any-open-bill* event, and one that omits both. The results, presented in Table 8 in Appendix D, show that the coefficient shrinks in size, but retains its sign and statistical significance across estimations.

6. Effect heterogeneity

The prediction of Proposition 3 in Section 2 regarding effect heterogeneity was that the microcredit uptake-distress response is more adverse for households with low financial literacy skills. We now test this prediction against the data.

The financial literacy score is a count variable running from 0 to 10, but can be treated as a continuous one by adding small white noise, which we do[24]. Abrevaya et al. (2015) proposed a consistent and asymptotically normal semi-parametric estimator for the conditional average treatment effect on the treated (CATT) where the conditioning is on a continuously distributed covariate. The top panel of Figure 3 displays the CATT with financial literacy skills as conditioning variable, with different bandwidths. Again we trim the observations with propensity score estimates outside the interval [0.08, 0.92] to increase precision and robustness of the estimates. The bottom panel of Figure 3 shows the CATT estimates with a bandwidth that seems to work reasonably well, with 95% confidence bands. The CATT estimates are in line with Proposition 3 in Section 2: the lower the financial literacy skill levels of the respondent, the stronger the household's financial distress response to microcredit.

Table 4 displays regression estimation results from specifications that include interaction terms between microcredit uptake and the financial literacy score. The interaction term of microcredit and financial literacy is negative and statistically significant across estimations. This provides more evidence that the microcredit-distress response is more adverse for borrowers with low financial

[24]As suggested in the code by Abrevaya et al. (2015), a uniformly distributed random number in the interval (0,1) is added to the variable, and then 0.5 substracted.

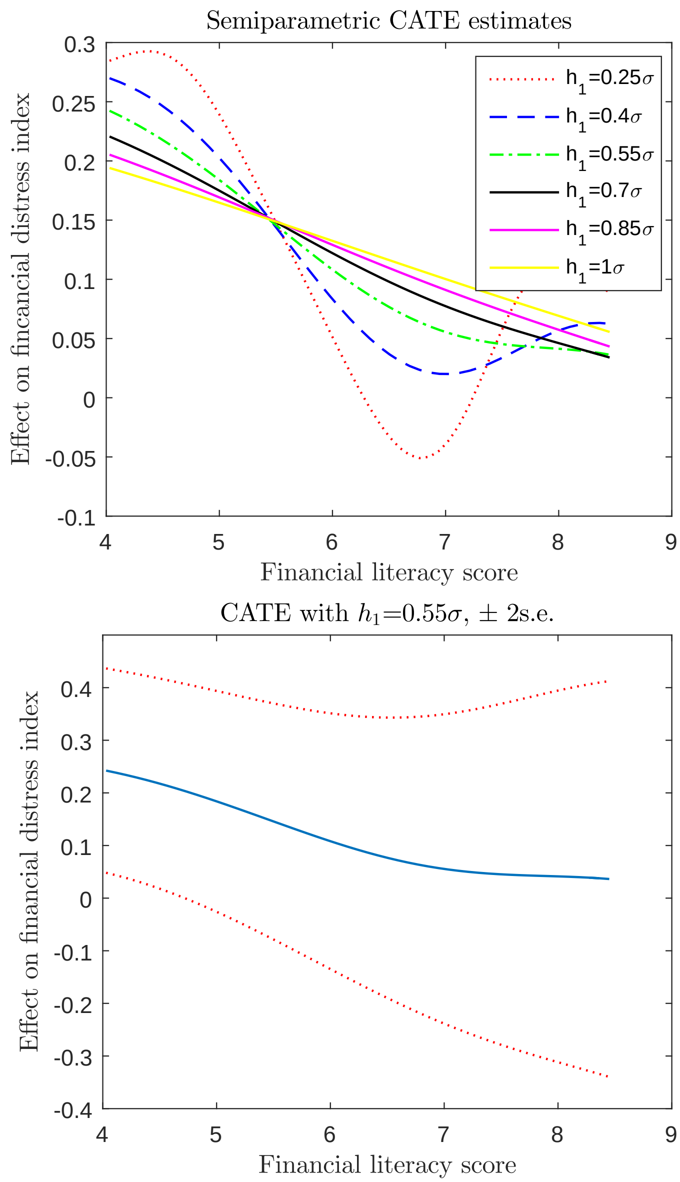

Figure 3: CATT estimates of microcredit uptake on the financial distress index, with the financial literacy score as conditioning variable. The top panel displays estimates with different bandwidths h_1; the bottom panel displays the CATT estimate with 95% confidence interval when $h_1 = 0.55$.

Table 4: Heterogeneity in the effect of microcredit take-up.

	(1) OLS	(2) OLS w/loan officer FE	(3) Entropy-bal. OLS	(4) Panel hh FE-EB m4
Microcredit	0.5983***	0.6120***	0.7778***	1.1799***
	(0.18)	(0.21)	(0.25)	(0.26)
Financial literacy score	0.0285	0.0209	0.0521	-0.4472***
	(0.03)	(0.03)	(0.03)	(0.07)
Microcredit × Financial literacy	-0.0567**	-0.0479*	-0.0696**	-0.1267***
	(0.03)	(0.03)	(0.03)	(0.04)
Controls	✓	✓	✓	✓
Loan officer FE		✓	✓	
Household FE				✓
Obs.	866	711	459	1278
Wald	0.31	0.36	0.51	0.39
BIC	2222.81	1805.52	.	1091.99

* p<0.1, ** p<0.05, *** p<0.01

literacy skills.

We examine treatment effect heterogeneity also in another way: subgroup analysis. For the financial literacy score, the choice of a cutoff is somewhat arbitrary, so we apply multiple cutoffs, see Table 7 in Appendix E. Treatment effect parameter movements are consistent with treatment effect of credit uptake to be decreasing monotonically in financial literacy skill levels[25]. This finding is consistent across estimators and is robust with respect to the choice of cutoff for the financial literacy score level in creating the subgroups.

Why is the financial distress response to microcredit most prominent among the subsample with low levels of financial literacy? One explanation may be that those who lack numeracy and financial literacy skills may be more likely to overborrow. To examine this, we regress the size of the loan and the size of the monthly installment on the same set of predictors as in the financial distress regressions, using the sample of loan applicants. We either control for monthly household income, or scale the outcome by monthly household income, see Table 5. The results, reported in Table 5, show that financial literacy skills

[25]In other words, the effect of microcredit uptake on financial distress being less positive and less statistically significant for higher financial literacy levels.

Table 5: Financial literacy skills predict debt-burdens posed by the uptake of a first micro-credit.

	(1) Loan amount	(2) Monthly installment	(3) Loan size-to-income ratio	(4) Installm.-to-income ratio	(5) Installm.-to-income ratio>0.5
Financial literacy score	-0.044***	-0.053***	-0.393**	-0.032	-0.042***
	(0.15)	(0.01)	(0.20)	(0.02)	(0.02)
Monthly household income	-0.09***	-0.067**			
	(0.04)	(0.03)			
Controls	✓	✓	✓	✓	✓
Loan officer FE	✓	✓	✓	✓	✓
Obs.	104	104	106	106	108
BIC	486.03	474.26	635.58	250.46	64.13

(1) Marginal effect estimates predicting various indicators of debt burden, on the subsample of applicants at baseline. Column (1) and (2) report $exp(\beta)$ from Poisson Pseudo-ML estimates; columns (3)-(5) estimates from linear FE models. The outcome for the 5^{th} column is binary, but fixed effects logit estimation does not converge.
(2) * p<0.1, ** p<0.05, *** p<0.01
(3) Robust standard errors in parentheses.

are consistently associated with taking up loans and installments that are larger relative to household income. The fact that the financial distress-response to microcredit uptake is stronger among borrowers with low financial literacy, can thus be explained - at least in part - by these individuals taking on larger debt burdens.

Although being consistent with a plausible mechanism, the estimations in this subsection are explorative in nature and the findings tentative, given that financial literacy is potentially endogenous to the loan size as well as the installment size.

7. Conclusion

To gain an understanding of the links between household borrowing behavior and financial distress risk, we developed a simple theoretical model. One, perhaps intuitive, prediction that is derived is that borrowing increases financial distress risk. The predictions regarding the heterogeneity of the credit-distress response is that financial distress more likely increases after taking up microcredit if income flows are more volatile, when financial literacy skills are low, and when it does not have access to informal insurance.

These predictions were tested against quasi-experimental survey data collected in urban Uganda, merged with MIS data on the same households. Indeed, microcredit uptake is on average associated with an increased risk of financial distress. Given the observational, quasi-experimental nature of the data, these mean impact results are scrutinized. Effects found are similar both when the variation in borrowing status is cross-sectional (baseline survey), as well as when the exploited variation is over time (panel estimations with household fixed effects). Moreover, they withstand various types of robustness checks - some of which are possible due to the MIS data. If anything, the estimated effects are lower bounds on the true effects.

Second, substantial effect heterogeneity is uncovered. The finding that the microcredit-distress response becomes much smaller and loses statistical significance for higher financial literacy levels and is much stronger and statistically significant for lower financial literacy levels, is to our knowledge novel. We explain this finding in a model of an agent making a stochastic borrowing decision, with less numerate individuals making more often (than more numerate individuals) errors that lead them to borrow despite a negative expected returns to it.

The finding on effect heterogeneity along the financial literacy dimension highlights the fact that the average impact of microcredit depends as much on lenders screening and selecting creditworthy borrowers as it depends on the average impact on a random member of the population. On the policy side, testing basic numeracy skills and using the test results in credit scoring may reduce financial distress in the microcredit borrowing population. Similarly, there have been attempts to incorporate psychometrics into the credit screening process (Klinger et al., 2013), to gauge entrepreneurial capacity. We also found suggestive evidence that less financially literate individuals take on loans that present a larger debt load, with larger repayments relative to their income. In a second-best world where returns to capital of individual microenterprises cannot be known and income levels and volatility can at best only partially be observed, this may suggest approving smaller loan sizes when all else equal, loan applicants' financial literacy skills are found to be low.

On the methodological front, eliciting respondents' recent experiences of discrete events (which could also include salutary events, such as asset purchases, housing improvements and enrolling a child back into school) may help boost statistical power to detect relocations up and down the counterfactual distribution of socio-economic outcomes. It is therefore advised that future randomized trials that aim to evaluate population-level impacts of financial services and financial market interventions include such indicators in the range of outcome measures to more completely and effectively capture their complex and heterogenous impacts.

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Appendix A: The construction of the financial literacy score and the wealth index.

A1: The Financial literacy score

Four questions were asked to elicit basic numeracy skills:

- What is 25+17?
- What is 49-23?
- What is 12*4?
- What is 56:7?

The following five questions were posed to elicit financial literacy levels, slightly adapted from Bandiera et al. (2010):

- What is 20% out of 3000 UgSh?
- If you could save UGX5,000 per month, how many months would you need to save to get UGX30,000?
- If you needed UGX180,000, how much would you need to save per month (in UgSh) to have the money within one year (12 months)?
- Assume that you saw a radio of the same model on sale in two different shops. The initial retail price was UGX 20,000. One shop offers a discount of UGX 1,500, while the other one offers a 10% discount. Which one is a better bargain?
  - Discount of 10% on 20,000
  - Discount of UgSh 1,500
  - They are equally good
  - Don't know
  - Suppose you have deposited UGX 100,000 in the bank for an interest of UGX 10,000 per year. If you withdraw all the money after 3 years, how much will you get?

The financial literacy score was constructed as the sum of correct answers to the above nine questions plus 1 if the number of commercial banks and microcredit institutions known to the respondent ('Please mention as many names of banks in Uganda as possible') was higher than the average number for all respondents, which was 6.33 financial institutions.

A2: The wealth index

Counts of the following assets were used to construct the asset index: (1) rooms, (2) chairs, (3) tables, (4) beds, (5) sofas, (6) mirrors, (7) watches, (8) kerosene stoves, (9) gas stoves, (10) televisions, (11) radios, (12) mobile phones, (13) generators, (14) solar panels, (15) light bulbs, (16) bicycles, (17) motorcycles, (18) cars, (19) refridgerators, (20) chicken.

Appendix B: Testing for survivorship bias

Table 6: Marginal effect estimates from regressions predicting dropping out of borrowing as of 31/03/2014.

	(1) Probit	(2) LPM[(2)] $w/loan$ officer FE
Fin. distress score at baseline survey	-0.002 (0.005)	-0.001 (0.001)
Nr. of weeks passed since becoming client till interview	-0.002*** (0.000)	-0.001** (0.000)
Nr. of weeks passed since interview till 31/03/2016	0.002*** (0.001)	0.001** (0.000)
Female borrower	-0.028** (0.012)	-0.028** (0.011)
Age in years	-0.002*** (0.001)	-0.001* (0.001)
Household size	-0.002 (0.001)	-0.000 (0.000)
Completed primary education	-0.046*** (0.013)	-0.020 (0.012)
Compl. secondary educ.	-0.061*** (0.020)	-0.022 (0.018)
Compl. tertiary educ.	-0.053*** (0.021)	0.005 (0.014)
Financial literacy score	0.011*** (0.003)	0.004** (0.002)
Shock took place	-0.001 (0.001)	0.002 (0.009)
Income volatility	0.022*** (0.007)	0.011* (0.006)
Saving group[(3)]	. .	0.006 (0.007)
Loan officer FE	No	Yes
N	596	595
$Pseudo - R^2$	0.576	.
R^2	.	0.075
Wald	28.31	.
F-stat.	7.88	4.32

(1) The MIS data of 31 October 2013 are merged with the survey data and used to predict dropout from borrowing as of 31 March 2014. The dependent variable takes on the value of 1 if the borrower is still present in the MIS data (i.e., has not dropped out of borrowing from the MDI), and 0 otherwise.
(2) Linear probability model.
(3) In the probit estimation, inclusion of the ROSCA is not feasible due to multicollinearity; inclusion of the wealth index was not feasible in either of the two models.
(4) Robust standard errors in parentheses
(5) * p<0.1, ** p<0.05, *** p<0.01

Appendix C: Testing for pre-treatment dip

Table 7: Marginal effect estimates from a linear regression model predicting the financial distress index.

	(1)	(2)
Loan applicant	-0.2045**	-0.1998
	(0.10)	(0.15)
Applicant*days to loan disbursement	0.0023	0.0049
	(0.00)	(0.00)
Female	0.2091**	-0.0085
	(0.09)	(0.12)
Head of household	0.1686**	0.0237
	(0.07)	(0.11)
Age in years	0.0042	0.0347***
	(0.00)	(0.01)
Household size	0.0364***	0.0238
	(0.01)	(0.03)
Muslim	0.0704	-0.1173
	(0.08)	(0.09)
Completed primary education	-0.0110	-0.0660
	(0.09)	(0.12)
Secondary school	0.0434	0.0382
	(0.10)	(0.14)
Tertiary education	-0.0966	-0.1837
	(0.14)	(0.24)
Financial literacy score	-0.0167	-0.0036
	(0.01)	(0.02)
Wealth index	-0.0385***	-0.0445**
	(0.01)	(0.02)
Member of ROSCA	-0.2512***	-0.2126
	(0.07)	(0.13)
Household faced a shock	0.3488***	0.3033***
	(0.07)	(0.09)
Income reported stable	0.3496***	0.2241
	(0.13)	(0.25)
Income reported unstable	1.0036***	0.8960***
	(0.13)	(0.26)
Income reported very unstable	1.4662***	1.0815***
	(0.18)	(0.33)
Loan officer FE	No	Yes
N	698	450
Wald	0.32	0.52

(1) * $p<0.1$, ** $p<0.05$, *** $p<0.01$
(2) Robust standard errors in parentheses.

Appendix D: Robustness check: leaving out *healthless* or *any-open-bill* from the financial distress index

Table 8: Replications of estimates of the effect of microcredit of Table 2 without *healthless* and/or without *any-open-bill*.

	(1) OLS	(2) OLS w/loan officer FE	(3) Entropy-bal OLS	(4) OLS	(5) OLS w/loan officer FE
Without *healthless*	0.208**	0.245***	0.309***	0.190**	-0.467***
	(0.075)	(0.089)	(0.093)	(0.074)	(0.143)
	[n=711]	[n=711]	[n=483]	[n=483]	[n=550]
Without *any-open-bill*	0.195***	-0.233***	0.348***	0.323***	0.352**
	(0.074)	(0.090)	(0.094)	(0.115)	(0.139)
	[n=711]	[n=711]	[n=483]	[n=483]	[n=530]
Without *healthless* & *any-open-bill*	0.127*	0.161*	0.245***	0.245**	0.352**
	(0.071)	(0.086)	(0.093)	(0.115)	(0.139)
	[n=711]	[n=711]	[n=483]	[n=483]	[n=530]

(1) Robust standard errors in parentheses; * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
(2) The full set of controls is used in all estimations.

Appendix F: Effect heterogeneity: subgroup analyses

		(i)	(ii)	(iii)	(iv)	(v)
A. OLS with loan officer FE						
	Financial literacy score	>3	>4	>5	>6	>7
High fin. lit.		0.243**	0.214*	0.126	0.169	0.128
		(0.103)	(0.112)	(0.126)	(0.142)	(0.166)
		[n=608]	[n=520]	[n=420]	[n=298]	[n=210]
	Financial literacy score	≤ 3	≤ 4	≤ 5	≤ 6	≤ 7
Low fin. lit.		0.631**	0.494***	0.583***	0.438***	0.400***
		(0.300)	(0.182)	(0.151)	(0.136)	(0.121)
		[n=103]	[n=191]	[n=291]	[n=413]	[n=501]
B. Augmented IPW						
	Financial literacy score	>3	>4	>5	>6	>7
High fin. lit.		0.185**	0.198**	0.163	0.137	0.077
		(0.086)	(0.094)	(0.103)	(0.108)	(0.106)
	Obs.	[n=520]	[n=450]	[n=372]	[n=269]	[n=199]
	Financial literacy score	≤ 3	≤ 4	≤ 5	≤ 6	≤ 7
Low fin. lit.		0.570*	0.372**	0.334***	0.319***	0.306***
		(0.294)	(0.164)	(0.117)	(0.100)	(0.098)
	Obs.	[n=66]	[n=129]	[n=202]	[n=322]	[n=392]

(1) * p<0.1, ** p<0.05, *** p<0.01

(2) Robust standard errors in parentheses.

(3) Full set of controls included in each estimation.