Active learning fosters financial behavior: Evidence from rural Uganda

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Abstract

We conduct a randomized field experiment to study the effects of two financial education interventions offered to small-scale retailers in Western Uganda. The treatments contrast "active learning" with "traditional lecturing" within standardized lesson-plans. We find that active learning has a clear positive impact on savings and investment outcomes and weaker effects on financial literacy and debt-related outcomes, in contrast to precisely estimated insignificant impacts of lecturing. The active learning intervention is superior as it works via three mechanisms, i.e. increased financial literacy, self-control, and financial confidence, while lecturing only affects financial confidence.

JEL-Classification: O16 (savings), D14 (personal finance), I21 (analysis of education)
Keywords: financial literacy, financial behavior, RCT, active learning, lecturing, training method

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1 Introduction

Financial education programs are expected to foster the financial knowledge and behavior of individuals to achieve financial inclusion and to promote financial stability. However, the potential impact of these programs is debated in the literature (see Hastings et al., 2013; Lusardi and Mitchell, 2014; van Roij et al., 2014; Brown et al., 2016) because early experiments have shown relatively muted effects on financial outcomes (e.g. Cole et al., 2011). While the more recent evidence tilts clearly towards the intended effectiveness (Kaiser and Menkhoff, 2017), the causal effects of these interventions are economically small on average (cf. Fernandes et al., 2014). Moreover, effects seem to be highly heterogeneous across multiple dimensions (Miller et al., 2015; Kaiser and Menkhoff, 2017). As even large-scale financial education programs appear to have only modest effects on financial behavior (cf. Bruhn et al., 2014, 2016), policymakers and researchers have moved towards evaluating alternatives and complements to the traditionally dominating lecture-based classroom-programs (cf. Drexler et al., 2014; Carpena et al., 2011, 2017; Campos et al., 2017).

Thus, there have been a small number of rigorous randomized evaluations recently that look at impacts of financial education programs with varying design-elements. For example, Drexler et al. (2014) vary the content of the financial education curriculum and show that heuristics-based approaches to financial education may be more effective in impacting financial behavior than traditional curricula. Carpena et al. (2017) show that complementing classroom instruction with personalized elements, such as counselling and goal-setting yields higher impacts. Recently, Berg and Zia (2017) show that financial education interventions that primarily target non-cognitive channels can significantly impact financial behavior. However, overall, the number of rigorous studies examining approaches and design elements of financial

education is quite limited. In particular, to the best of our knowledge there is no study analyzing the effect of varying *how* contents are taught, despite the fact that the advantages of "active learning" over "traditional lecturing" have been demonstrated in other domains such as science instruction (e.g., Freeman et al., 2014). Teaching methods that are engaging and involve participants are expected to yield higher impacts than exposition centered financial education, dominating many current programs. Generally, the importance of how information is conveyed has been demonstrated to be linked to behavioral responses in the context of retirement-savings (cf. Saez, 2009).

Thus, we conduct a large-scale cluster-randomized field experiment to contribute to this recent literature by examining the treatment effects of alternative approaches to financial education. The financial education treatments contrast an *active learning* approach with a *traditional lecturing* approach while teachers, content, and intensity of training are kept constant. The randomized field-experiment is conducted in rural Western Uganda with 1,291 market vendors. Two results emerge:

First, and this is our main finding, the group allocated to the *active learning* program experiences a significant increase in four out of six outcome families, each captured by an index. The strongest impacts occur in the savings and investment domains: total savings increase by 28 percent relative to the control group, and, active learning has a direct effect on investments into the own business and business formalization. Additionally, there are weaker but still marginally significant effects on a measure of financial literacy and an index of debt-related behavior. Thus, this approach generates larger treatment-effects than the precisely estimated insignificant positive treatment effects realized by *traditional lecturing.* Differential treatment effects, however, can only be confirmed for outcomes in the savings domain.

Second, we provide tentative evidence that the relatively strong impact of the active learning approach on savings outcomes is not primarily caused by an increase in financial

literacy but is impacted through the channel of increased self-control. Investment outcomes, in contrast, appear to be impacted through increased financial literacy and changes in financial confidence.

Our research adds to a recently growing literature that advances the understanding of the impact of different delivery channels of financial education interventions. Drexler et al. (2014) study the differential impacts of two different financial education curricula in the Dominican Republic. They provide evidence that a heuristics-based approach, relying on simplification of complex financial concepts ("rule-of-thumb-training"), does generate larger behavioral impacts than the teaching of traditional curricula ("full technical training"), especially for low-skilled individuals and individuals with low baseline financial literacy and motivation. Skimmyhorn et al. (2016) replicate this type of experiment, but do not find evidence to support differential effects regarding the two different types of curricula, probably because of the relatively highly educated sample of respondents. Lusardi et al. (2017) present evidence from online-experiments conducted in the U.S. that interactive tools, narratives and financial education videos may be more effective than written informational content in affecting financial knowledge and self-efficacy.

Targeting entrepreneurs, Campos et al. (2017) show that a psychology-based training program that teaches a pro-active mindset outperforms traditional business training for self-employed individuals in West Africa with strong differential effects on business profits.

Finally, Carpena et al. (2017) show in a multiple-treatment experiment that combining traditional instruction with personalized elements, like counselling and goal-setting, yields higher treatment effects on financial behaviors, such as keeping track of household expenses through a written budget, starting (informal) savings, and avoiding expensive borrowing to cope with unexpected shocks (cf. Carpena et al., 2017, p.2). Much of this evidence suggests that interventions relying on traditional lecturing appear to have small or zero effects. Thus,

alternative approaches to lecture-based education appear to be especially important when targeting low-skilled and low-income individuals. Some have argued that "one-size-fits-all" (Drexler et al., 2014, p. 25; Carpena et al., 2017 p.2) classroom programs are not suitable to foster financial behavior and that these programs have to be complemented with individualized elements. We show that even within a one-size-fits-all classroom program, active learning techniques can provide the missing link between knowledge creation and behavioral change. Exposition centered teaching methods, however, do not impact financial behavior and financial outcomes.

This paper is structured into seven further sections: Section 2 describes the financial education programs, the experimental design, and empirical strategy. Section 3 provides an overview over response rates, descriptive statistics, and a discussion of randomization-balance. Section 4 presents main results. Section 5 provides a discussion of potential causal mechanisms leading to behavioral change. Section 6 presents an investigation of distributional treatment effects and treatment effect heterogeneity. Section 7 summarizes robustness exercises and Section 8 discusses the results and concludes.

2 Treatments and experimental design

In this section we describe the two financial education programs which we evaluate (Section 2.1), the experimental design (Section 2.2) and also the empirical strategy we follow here (Section 2.3).

2.1 Financial education interventions

As one step towards promoting financial inclusion, the central bank of Uganda (*BoU*) has established a national strategy for financial literacy in Uganda.[1] This strategy seeks to foster

[1] See: https://www.bou.or.ug/opencms/bou/bou-downloads/Financial_Inclusion/Strategy-for-Financial-Literacy-in-Uganda_August-2013.pdf; last checked on March 5th, 2017.

the personal financial knowledge and behavior of different target groups, including school students, young adults in urban settings, and adults in rural areas. *BoU* has partnered with the German Development Cooperation (*GiZ)* to design effective financial education interventions. While the specific curricula and dissemination formats vary depending on the target group, they all target financial knowledge and behavior within the five sub-domains of (i) budgeting and personal financial management, (ii) credit, (iii) savings, (iv) business investing, and (v) payments and financial service providers.

Focusing on one of the financial education strategies' major objective of improving rural outreach, *GiZ* has developed educational formats for the target population of the rural self-employed. These programs teach how to create a written household budget and to keep track of household's financial inflows and outflows, encourage household savings, explain the costs and benefits of various forms of credit, explain the trade-off between risk and return regarding productive investments into micro-enterprises, highlight the benefits of diversification among sources of income and investments, and inform about the benefits of using financial services provided by regulated financial institutions. Thus, these trainings promote the use of formal financial services, without generally discouraging semi-formal types of financial products (e.g. saving in village savings and loans associations (VSLAs) or rotating savings and credit associations (ROSCAs)). The trainings do, however, caution against the use of expensive credit provided by informal money-lenders, and the take up of costly loans to finance consumption expenditures in general.

Currently, *GiZ* offers two different educational interventions for the same target group of rural self-employed, creating the opportunity to study the differential impact of alternative delivery approaches to financial education. While these two programs are identical with regards to the content areas covered (they both address the domains (i) to (v)), they differ in their concrete method of instruction, i.e. *how* this content is taught (cf. Table A1 in Appendix A).

Version A of the financial education training uses *active learning* methods and is highly learner-centered (see Ruiz-Primo et al., 2011; Freeman et al., 2014): Its main feature are five distinct stations, designed to provide problem based learning opportunities and encourage discussion among the participants. Exercises and materials are designed to engage participants with the subject matter, e.g. through completing budgeting exercises, sorting different savings- and investment options with regard to their safety risk/return-profiles, differentiating between sensible and non-sensible reasons to take up a loan, and completing an exercise on whether financial services are regulated by the central bank or not. Respondents are encouraged to share their experiences and complete the exercises. The trainer has the role of a learning facilitator.

Version B, on the other hand, is organized as a community lecture, i.e. an "exposition-centered" (Freeman et al., 2014, p. 8410) *traditional lecturing* approach, relying mostly on lecturing with the aid of a flip-chart and some room for participants to ask questions. Here, the lecturer explains important concepts and demonstrates how to create a written budget, and how different financial products and choices could be categorized. The participants are expected to listen to the input of the trainer and are allowed to ask questions. Sharing of experiences is also encouraged but the trainer can be characterized as a lecturer rather than a facilitator. Thus, the experiment is designed to contrast constructivist versus exposition-centered (transmissive) course designs as two stylized ideal-types along the continuum of instructional approaches. Despite these important differences in instructional methods, both versions of the financial education training are standardized to cover the same content and to have the same intensity of about two hours. For the purpose of evaluating the impact of the financial education programs, we employ the same trainers to deliver both financial education versions A and B to the target groups. Thus, the different versions are not confounded with idiosyncratic characteristics of the trainers and implementation is not heterogeneous across sites.

2.2 Experimental design

We organize a cluster-randomized experiment to study the differential impact of the two financial education interventions on financial literacy and financial behavior. The main outcomes of interest are changes in *financial literacy* (see Appendix C for the development of a psychometrically sound measure of financial literacy) and changes in *financial behaviors* within five sub-domains addressed by the training, i.e. (i) budgeting, (ii) usage and handling of credit, (iii) savings, (iv) business investing and formalization, as well as (v) the use of formal payment- and other financial services such as formal insurance. Randomization is done at the cluster-level, and 83 rural marketplaces in seven districts of the Rwenzori Region in Western Uganda form the sample of clusters considered in this study (see Figure 1).

<Figure 1 about here >

To the best of our knowledge, the dataset covers all relevant permanent and regular marketplaces in the region. Because prior information about the marketplaces (such as number of vendors and primary goods traded) is limited at the time of randomization (prior to the individual-level baseline survey), we perform a non-stratified randomization procedure to allocate the 83 clusters in our sample to either receive financial education treatment A (n=27) or B (n=28), or to be allocated to the control group (C) (n=28). The trainings each aim for reaching a target group of about 15 to 16 vendors, which leads to a sample population of 1,245 to 1,328 individuals.

Vendors were mobilized to participate in our survey and the financial education sessions by the local market-chairpersons. The treatment status was unknown to the market-chairperson, so no differential selection (mobilization) should be in place due to the reliance on local market-chairpersons. However, selection biases could arise when market-chairpersons favor specific groups (such as their peers) over others in general. This would indeed impact the *external* validity of our experiment in the sense that treatment effects may causally interact with

unobserved features of our specific context and sample (cf. Muller, 2015). The *internal* validity, however, is not affected because these selection mechanisms will occur in all three groups. After baseline survey, the treatment groups received either financial education treatment A (active learning) or B (traditional lecturing) on the same day, immediately after the interviews. The control group (C) did not receive any treatment. After baseline-survey and subsequent financial education treatments we conducted follow-up surveys approximately 6 months later.

We report power-calculations for minimum detectable effect sizes in order to be able to rule out imprecisely estimated small- or zero-effects of financial education treatment. Intra-cluster-correlation (ICC) ranges from 0.030 (financial literacy score and budgeting index) to 0.000 (savings index) for the outcomes. Thus, our experiment has 80 percent power to precisely detect (at $\alpha = 0.05$) effect sizes as small as 0.15 to 0.18 standard deviation units. Note that type II error rates are higher than 20 percent for coefficients estimated to be smaller than 0.15 (0.18), i.e. we cannot reject positive impacts of financial education treatments smaller than 0.15 standard deviation units. However, these may be regarded as economically insignificant (see Section 4).

2.3 Empirical strategy

To estimate the causal effect of the financial education interventions on financial literacy and behavior, we compare the two treatment groups (A = active learning) or (B = traditional lecturing) to the control group (C= no treatment) at the time of the follow-up survey. Because selection into treatment is done through randomization and the later Section 3.2 indicates balanced groups at baseline, the unbiased *intent-to-treat (ITT)* treatment effect (average effect of being assigned to a treatment-cluster) can be estimated within the following ANCOVA framework (McKenzie, 2012):

$$y_{ic(t)} = \alpha + \delta_1 y_{ic(t-1)} + \beta_1 A_c^T + \beta_2 B_c^T + \theta_d + \varepsilon_{ict} \quad (1)$$

Here, $y_{ic(t)}$ denotes the outcome variable (measure of financial literacy or behavior) for individual i in cluster c at the time of follow-up (t). $y_{ic(t-1)}$ controls for the lagged value of the outcome at baseline and θ_d are district-level fixed-effects. A_{ic}^T and B_{ic}^T are dummy variables equal to one for respondents situated in a cluster being assigned to treatment A or B, respectively. Thus, β_1 captures the (ITT) treatment effect of financial education intervention A, and β_2 captures the (ITT) treatment effect of intervention B. ε_{ict} denotes the error-term. Standard errors are clustered at the market-level to account for the level of randomization.

For binary outcomes, linear probability models are used (see Karlan and Valdivia 2011, Cole et al. 2013 and Drexler et al. 2014 for this approach). However, results are not sensitive to changing the estimations to non-linear (logit or probit) models for binary outcomes (see Appendix B).

Finally, to avoid problems inherent to testing multiple hypotheses (type-I-error inflation), we aggregate multiple related outcomes into index-measures of outcomes families: Following Kling et al. (2007), Karlan and Valdivia (2011), and Drexler et al. (2014), we define $y^*$to be an equally weighted average z-score index of its components y_k^*. Thus, for each component of a given outcome-family, we first rescale each outcome such that positive values indicate desirable treatment effects. Next, we standardize the component to have a mean of zero and standard deviation of one for the control-group: $y_k^* = \frac{y_k - \mu_k}{\sigma_k}$, with μ_k denoting the mean of y_k for the control group (C) and σ_k denoting the standard deviation of y_k for the control group. The aggregate index then takes the following form: $y^* = \frac{\sum_k y_k^*}{k}$. Finally, we standardize the outcome index (y^*) to have a mean of zero and standard deviation of one for the control-group. Thus, coefficients on A_{ic}^T and B_{ic}^T can be interpreted as standardized mean differences (Glass's Δ).

To probe the potential causal mechanisms leading to changes in financial behavior we estimate the following two stage regressions:

$$Z_{ic(t)} = \alpha + \beta_1 A_c^T + \beta_2 B_c^T + \theta_d + \varepsilon_{ict} \quad (2)$$

$$y_{ic(t)} = \alpha + \beta_1 Z_{ic(t)}^* + \theta_d + \varepsilon_{ict} \quad (3)$$

Equation (2) shows the first stage of the regressions. Here, $Z_{ic(t)}$ is a measure of intermediate outcomes in a causal chain that may have an impact on downstream behavior. We test three potential mechanisms: $Z_{ic(t)} = financial\ literacy$, $Z_{ic(t)} = self\ control$, $Z_{ic(t)} = financial\ confidence$ (see Section 6). The first stage estimates the causal effect of financial education treatments A and B on these intermediate outcomes. Thus, in the second stage, $Z_{ic(t)}$is instrumented by the two treatment dummies, and we examine whether predicted levels of the intermediate result ($Z_{ic(t)}^*$) explain variation in financial behavior ($y_{ic(t)}$).

3 Data

After mapping of the markets, piloting the survey tools and interventions, and randomization, we conducted a comprehensive baseline survey between November 1st and December 19th of 2015. This dataset covers all vendors invited and interested to participate in our survey (n=1,291). The questionnaires were translated into three local languages widely spoken in the area, and enumerators conducting the face to face interviews in the local languages were trained extensively prior to the field-activities.

3.1 Response rates

After baseline-survey and subsequent financial education treatments, we conducted follow-up surveys between April 6th and July 19th of 2016. After this first round of tracking efforts, we had followed-up with 1,094 vendors (i.e. the attrition rate was at 15.26 percent). To minimize attrition, we undertook extensive tracking efforts to follow up with another 67 respondents (see timeline Figure A1 in Appendix A). Thus, our final response rate is high, given

the kind of relatively mobile target group: We follow up with roughly 90 percent of the initial sample at endline survey (see Table 1).

< Table 1 about here>

Unfortunately, attrition rates vary by experimental condition: While the control group (C) and treatment group (A) have attrition rates of 8.55 percent and 7.25 percent, respectively, the attrition rate in treatment group (B) is almost twice as high as in group (A) with 14.25 percent. While this may indicate selective attrition, we show in Appendix B that this does not qualitatively change our results: we probe the sensitivity of our results by estimating bounds on the treatment effects with several scenarios imputing missing observations at the endline and applying inverse probability weighting of selection into endline survey to our regressions. Details are provided in Section 7 and Appendix B.

3.2 Baseline descriptive statistics

Table 2 reports summary statistics for the full sample and each experimental condition at baseline.

< Table 2 about here>

Panel A shows variables that measure characteristics at the household level. The average household size is 6.83 people, with an average of 2.17 adults contributing to the household's income, a mean of 4.17 children being supported and a mean of 0.36 adults who do not generate external income, such as elderly (plus 0.13 for missing values). Several currency denominated outcomes had a long right tail, possibly indicating enumeration errors. Therefore, we winsorize all currency denominated outcomes at the 99th percentile (see Blattmann et al., 2015). The mean (winsorized) monthly individual income is around 220,000 UGX (about 60 USD). The mean (winsorized) monthly household consumption value is about 593,000 UGX. Household

consumption is higher than hypothetical added individual incomes because of subsistence farming, being reported by 83 percent of the sample.

Panel B reports variables at the respondent-level. Our sample is predominantly comprised of women (80%) and the average age is 36.2 years. On average, participants have been vending goods on markets for 7.4 years. Only 14 percent report to be selling non-food items (mainly second-hand clothing). The other 86 percent of the sample sell either fresh agricultural products or prepared food. Over two thirds (68 percent) are able to read and write in at least one language and about 25 percent participated in education beyond primary school. About two thirds of the sample report to be married. 70 percent of the respondents state that they are the main contributors of income to the household, while 55 percent report to be the "head of the household". Only 16 percent report to be economically dependent on others. Responding to another survey question, 22 percent of the sample receive aid or assistance from either NGOs or government programs.

We elicit general and domain-specific risk attitudes using common non-incentivized survey items popularized by Dohmen et al. (2010, 2011). These survey-items ask for willingness to take risk on a 0 to 10 scale with highest risk tolerance at 10. On average, respondents are relatively risk-averse. This applies both to the general risk attitudes (mean of 3.61) and to risk attitudes regarding the financial domain (mean of 3.78). The mode and median are at 3 for both the general and the domain-specific case, and thus lower than for a representative sample of the German population with a modal response of 5 (Dohmen et al., 2011). Our survey also includes a measure of numeracy and several psychological variables which are standardized into z-scores to have a mean of zero and a standard-deviation equal to one in the pooled sample. These are mainly used as controls, for the purpose of probing randomization balance in multiple dimensions, and for the investigation of possible causal mechanisms (see Section 5).

Panel C shows descriptive statistics for outcome measures of financial literacy and financial behavior indices (standardized to have a mean of zero and a standard deviation equal to one) at baseline (see descriptive statistics for individual index components in Appendix A).

3.3 Randomization balance

Causal inference within the estimation framework introduced in Section 2.4 rests on the random assignment of cluster to the treatment conditions which achieves balanced observed and unobservable characteristics. Randomization balance is probed by comparing the means between the control group and the treatment groups, as reported in columns (4) and (6) of Table 2. These differences are estimated within a simple regression framework, where standard errors are clustered at the market-level. Due to randomization, only a few small differences exist: In group A, a smaller share of the households seems to own the dwelling they live in. However, this difference is only marginally significant.

Second, the treatment groups are estimated to be slightly younger, on average, than the control group. However, again, this difference is only significant at the 10-percent-level. Thus, the only difference that is estimated to be statistically significant at the 5-percent-level is the average number of years the respondents work as a market-vendor. On average, respondents in the control group have been vending for about 2 years longer than their counterparts in group B. These minor imbalances are what can be expected to occur by chance. *Panel C* shows descriptive statistics for these outcome indices of financial literacy and financial behavior at baseline. Again, no statistically significant differences exist between the three experimental groups (see Table A in Appendix A for descriptive statistics and randomization balance for single index components). Reassuringly, a joint test of orthogonality (where an indicator of being assigned to any treatment is regressed on all observable baseline covariates and the index-measures for outcomes at baseline) results in low explanatory power and a p-value of 0.3. Thus,

orthogonality and balance seem to be met in this sample of 800 respondents (491 respondents have missing values on at least one of the covariates included in this fully specified regression covering all variables in Panels A, B, and C.)

4 Results

This section reports on the main treatment effects of the two financial education interventions (Section 4.1) and detailed analyses of treatment effects on selected index components (Section 4.2).

4.1 Main treatment effects

Table 3 reports the average (intent-to-treat) treatment effects of the financial education trainings (A) and (B) on the financial literacy scores and on five domains of financial behavior. All coefficients are estimated within an ANCOVA framework and include district-fixed-effects to account for district-level unobservable characteristics (see Appendix B for a discussion of district-level events that suggest including these district dummies).

< Table 3 about here >

Financial literacy score. Starting with financial literacy, both treatments show a positive coefficient, indicating that training tends to improve the degree of financial literacy. However, the coefficient sizes are moderate and thus only treatment A, i.e. active learning, has a statistically marginally significant effect relative to the control group. Due to small effect sizes and large standard errors the difference between both treatments is statistically insignificant.

While this result is not very strong in absolute terms, it looks more promising if we consider the circumstances. First, these trainings aim for changing behavior and not primarily for learning about numeracy-related financial knowledge, and second, the training is quite brief, lasting for two hours and covering five domains of behavior, so that the intensity for each

subject matter is rather limited. Additionally, the ICC for outcomes can be relatively high with up to 0.003. Thus, the experiment is powered to detect a minimum effect size of 0.18 standard deviation units with small standard errors. Given these conditions, a marginal significant effect size of 0.134 for treatment A may be seen as promising, since we cannot rule out false negatives due to relatively low power for this outcome.

Indices of financial behavior. The five domains of financial behavior being addressed by these trainings are each measured by an index and are presented in Table 3 in the order of the content areas addressed in the financial education sessions. The indices aggregate the single items as they are presented fully in the Appendix A and selectively discussed in the next Section 4.2. Overall, coefficients are consistently positive, indicating that also financial behavior can be improved into the desired direction.

Among these domains of financial behavior, the overall effects are strongest for the *investment index* with effect sizes being large (more than a quarter of a standard deviation) and statistically significant at the 1-percent-level (column 5). Only treatment A results in a statistically significant change in behavior, here investment behavior. Treatment B, i.e. lecturing, is estimated to have a statistically insignificant effect size of 0.16 standard deviation units. Because the experiment is powered to detect effect sizes as low as 0.15 standard deviation units for outcomes with low ICC we conclude that this is evidence of precisely estimated zero-effects of treatment B.

Similarly, the effect on the *savings behavior index* (column 3) is sizeable for treatment A (about 0.16 standard deviation units) and statistically significant at the 5-percent-level. Again, treatment A has a positive effect, while treatment B is statistically and economically insignificant (effect size of 0.01). The resulting large difference between both trainings generates a statistically significant difference of treatment A over treatment B.

Somewhat surprisingly, relative to low expectations from the literature on the effectiveness of financial education to change *debt behavior* (Miller et al., 2015; Kaiser and Menkhoff, 2017), we find a small but marginally significant effect for treatment A (column 4). As the coefficient sign of treatment B is even negative, the difference between both treatments is marginally significant, as well. Regarding training effects on the *budgeting index* (column 2), the effect sizes are positive but statistically insignificant from zero. The coefficient levels are higher for the *financial services index* (column 6); this is the only case where the coefficient of treatment B is higher than the one of treatment A, but both coefficients are estimated with a large standard error and remain statistically insignificant.

Overall, we see that financial education tends to have desired effects, but effect sizes remain small and statistically insignificant unless the active learning program is implemented. Active learning results in a significant effect on a measure of financial literacy and three out of five financial behaviors addressed by the training. Lecturing, on the other hand has no effect on financial literacy or any of the addressed financial behaviors. However, meaningful differential treatment effects can only be confirmed for outcomes in the savings domain.

4.2 Effects on single index components

This section complements the summary results on indices from above by providing and discussing results about the single index components of interest and considering their economic significance in terms of financial outcomes. In this respect, we focus on those two indices of financial behavior where we found statistically significant results at the 5-percent-level, i.e. savings and investments outcomes, while the other program objectives (financial literacy, budgeting behavior, debt behavior, and financial services behavior) are discussed in more detail in the Appendix A.

Components of the savings index. As shown above, active learning has a positive effect on the savings-index which is both statistically different from zero and from traditional lecturing (treatment B). Now we look at the three detailed results on financial outcomes as they are aggregated in the index, i.e. “any savings”, “total savings” and “net savings”. These results are shown in the first three columns of Table 5.

<Table 5 about here >

Given that 87.9 percent of the control group report any savings, the increase due to financial education has to be modest by definition and is in effect just 3.6 percentage points for treatment A which is statistically significant at the 10-percent-level (column 1). The effect of treatment B is smaller in size at 2.5 percentage points and, thus, statistically insignificant. While this effect size may be economically modest, it is noteworthy that financial education has an effect on savings at the extensive margin similar to other studies in various contexts (e.g. Duflo and Saez, 2003; Drexler et al., 2014; Jamison et al., 2014). Thus, one may speculate whether treatment effects on the extensive margin may be larger in magnitude for samples with lower ex-ante numbers of savers. The main index components driving the overall positive treatment effect of the index, however, are strong increases in financial outcomes in the form of total savings and net savings at the intensive margin.

Respondents in treatment group A report an average increase of 109,000 UGX in total savings which amounts to a treatment effect of approximately 0.18 standard deviation units, or an increase in savings by 21 percent over the mean of the control group (column 2). In contrast, the effect of treatment B is estimated to be economically small (even with a negative sign) and is precisely estimated to be statistically insignificant. Testing for differential impacts, active learning is more successful than traditional lecturing ($p=0.052$). The effect on net-savings appears to be even stronger. This variable captures the moderate reduction in debt volume together with the strong positive treatment effect on total savings so that net savings increase

by even 145,000 UGX (38 percent) relative to the control group (column 3). Thus, this effect appears quite strong and statistically significant at the 5-percent-level, and equality of effects for treatments A and B can clearly be rejected (p=0.025).

Components of the investment index. Turning to total investments into the own business, both treatments yield positive effects. The effect sizes – are estimated to be 94,000 UGX for treatment A and 41,000 UGX for treatment B (column 4 in Table 5). Despite relatively large standard errors, the effect of treatment A is statistically and economically significant: The increase in investment by 94,000 UGX is equivalent to an increase by 29.8 percent relative to the control group. This corresponds to an effect size of 0.17 standard deviation units. The effect of treatment B, again, is estimated to be less than half the size and statistically insignificant. The second component of the investment index looks at business formalization. The survey-data captures whether respondents state to have formally registered the business with any authorities. Again, treatment A results in a statistically and economically significant effect, whereas treatment B yields effect sizes insignificant from zero: Given that only 23.2 percent of the control group state to have formally registered their business with authorities, an increase of 7.7 percentage points is sizeable (an increase in formalization of 33 percent) and significantly different from zero. In contrast, the effect of treatment B, i.e. lecturing, is estimated to be insignificant from zero.

Outcomes on further items within indices. Even though the aggregate impact on the indices regarding the remaining outcome families is only marginally significant and insignificant, respectively (cf. Appendix A for a complete discussion of these results), we note results on single components here, since these confirm the general picture, that active learning consistently outperforms lecturing.

The *financial literacy score* is a combination of five items (see Table A2). Even though the overall effect size is moderate and only marginally statistically significant from zero, the

results on single components reveal that active learning treatment had a significant impact on two out of five items (which were of medium and high difficulty; see Table C1 in Appendix C), whereas lecturing had no effect (see Table A2).

The *budgeting index* consists of five binary items capturing changes in budgeting and record keeping behavior. Here, none of the items is impacted to a positive extent. Thus, the aggregate impact is insignificant from zero on average.

The *borrowing index* consists of six items (see Table A4 in Appendix A). Items aggregated into this index capture the structure and volume of debt along with a proxy for debt-bearing capacity and binary items indicating whether respondents would take up loans without a plan or are able to distinguish between sensible and non-sensible reasons to take up a loan with interest payments. The coefficients on the volume of loans intended for consumption purposes and productive investments are extremely small and, thus, insignificant from zero. There is a significant effect for the active learning treatment, however, on the self-report of respondents to take-up a loan (if offered) when they had no clear plan of how to use the money: approximately 14 percent of the control group report to be willing to take up a loan, even if they had no clear plan on how to use the borrowed amount. The marginal effect of the active learning treatment results in a 5.5 percentage point decrease in the willingness to take up a loan without a clear plan of its utilization. The effect of the lecturing treatment, again, is insignificant from zero.

Finally, the *financial service index* includes two binary items indicating whether respondents were ever covered by a formal insurance product or if they have ever used mobile money (payment) services (cf. Jack and Suri, 2014; Suri and Jack, 2016). 44 percent of the control group report to have ever used mobile money services and active learning has a marginal effect of 6.3 percentage points on mobile money use. Lecturing, however, has a much smaller

effect size (3.5 percentage points) and is statistically insignificant from zero. Both treatments have zero-effects on formal insurance use.

5 Exploring causal pathways

The remarkable difference we have uncovered between the two versions of financial education intervention raises the question of potential causal mechanisms. We motivate our procedure here (Section 5.1), then we introduce and discuss three kinds of potentially intermediating variables (Section 5.2), and finally we show exploratory results on causal pathways to behavior change (Section 5.3).

5.1 Potential mechanisms of financial education impact

The earlier literature on the evaluation of financial education often focused on the general effect size, because the existence of such a positive effect of financial education was heavily debated in the literature (cf. Fernandes et al., 2014). While many recent RCTs have clearly demonstrated that there is a positive effect on both financial literacy and downstream financial behaviors, average effects are generally quite small in size and highly heterogeneous across studies. This raises interest in potential determinants of effective interventions: Meta-analyses find that education intensity matters for its impact and that the timing, participation conditions, and features of the target group can contribute to understand the reported impact heterogeneity (cf. Fernandes et al., 2014; Miller et al., 2015; Kaiser and Menkhoff, 2017). However, little is known about *how and why* differently designed programs lead to heterogeneous impacts on financial outcomes. An important part of this discussion is to better understand the causal mechanisms by which financial education impacts behavior.

Regarding the economic content of such mechanisms, the first candidate is, of course, an improvement in financial literacy which enables individuals to make better financial decisions.

Evidence on this possible causal pathway has been documented in recent studies (cf. Fort et al., 2016; Sayinzoga et al., 2016) and also seems to be supported by a larger sample of experimental work (see Kaiser and Menkhoff 2017, p.617). However, it is a robust insight of (financial) education research that a good transfer of knowledge into behavior is fostered by additional elements (cf. Carpena et al., 2011, 2017). Generally, evidence shows in this respect that better self-control, and in line with it also future-oriented time-preferences, seem to be associated with more savings (cf. Ashraf et al., 2006). Finally, financial confidence and attitudes play an important role in financial behavior which explains why parental education or socialization has clear effects on financial behavior (e.g. Grohmann et al., 2015). Overall, there seem to be three main mechanisms at work which we analyze in the following, i.e. financial literacy, self-control and financial confidence.

Regarding the empirical test of a causal pathway through these variables we follow recent work by Sayinzoga et al. (2016) by applying a two-stage estimation approach (see Section 2.3).

5.2 Intermediating variables

In the following we describe the formation of the three above mentioned variables. While the concept and measurement of "financial literacy" was introduced above already (see Section 4 and Appendix C), "self-control" and "financial confidence" have not been defined yet. We measure the effect on these intermediate outcomes through items in our survey.

"Self-control" is assessed by a survey item asking to respond to the following question on a 1 (often) to 4 (never) rating scale: "If you get money, do you tend to spend it too quickly?" Responses are transformed into a z-score, scaled by the mean and standard deviation for the control group (see descriptive statistics in Table A6 in Appendix A).

"Financial confidence" is assessed by multiple items which are aggregated into an unweighted z-score-index of its components as detailed in Section 2.3. The index covers

responses to binary questions and statements that are answered on a rating-scale. Questions were asked on whether or not respondents felt that a complaint to a financial services provider would not change anything, whether respondents feel confident to inquire about the details of a financial product and to choose the financial product that best meets their needs, and whether respondents consider various products and options before making a financial decision (see descriptive statistics and definitions in Table A6 in Appendix A).

5.3 Results on causal pathways

The results from applying the two-step estimation approach are presented in Table 5. In the two Panels A and B we each analyze one of the two indices of financial behavior were we found significant average treatment effects, as they have been introduced above (see Section 4). Focusing first on the results of the first-stage regressions (which differ across the two panels because of different number of observations on outcomes), we find that the active learning treatment has significant effects on all three possible intermediating variables we consider: financial literacy, self-control, and financial confidence. Treatment B, however, only has an effect on financial confidence but neither on our measure of financial literacy nor on our survey-measure of self-control. Thus, given that only treatment A has an effect on financial behavior and financial outcomes, we can conclude that changes in financial confidence alone (as is the case for treatment B) may not be sufficient to realize behavior change.

< Table 5 about here >

Looking at the second-stage regressions, we see that improved savings behavior is mainly impacted via better self-control, while the investment index is impacted via improved financial literacy and financial confidence. Thus, all three intermediating variables may play a role, however, in different ways: the nexus between self-control and savings confirms other studies, and investment outcomes are mainly affected by literacy and confidence. We note that

treatment A, i.e. the active learning approach, seems to activate all three mechanisms and all of them are helpful to change financial behavior.

6 Treatment effect heterogeneity

This section investigates potentially heterogeneous effects from treatment by an examination of how treatment effects may be conditional on levels of outcomes (Section 6.1) and how treatment effects may interact with observable traits of the target group (Section 6.2).

6.1 Distributional treatment effects

Simultaneous quantile regressions show indeed that the advantageous effects of the financial education training are heterogeneous. Starting with the financial literacy index, the effect of treatment A (active learning) is largely independent from the outcome level of financial literacy (see Table 6, column 1). Graphically speaking, the entire distribution of financial literacy levels appears to be shifted to the right in response to the active learning treatment. By contrast, the effect of treatment B (traditional lecturing) is quite strong only below the median (although not much stronger than that of treatment A) while coefficients are about zero above the median. These differential effects become especially apparent at the top of the financial literacy distribution ($p<0.1$). We conclude, that treatment B may be an alternative approach in this respect if only individuals with very low levels of financial literacy belong to the target group, i.e. for individuals who may never have been confronted with any of the contents of the training. Active learning, however, appears to be beneficial at every stage of the financial literacy distribution.

< Table 6 about here >

Regarding the outcome level of the savings index, treatment A has clearly larger and even significant effects above the median, while effects of treatment B are very small and

insignificant across the whole distribution. Thus, differential treatment effects of active learning within the savings domain are driven by relatively large effects in the 60th and 80th percentile, while effect sizes are small below the median. This would indicate that financial education may increase savings-inequality with respect to our target group.

A similar pattern arises with respect to the investment index, although at a higher level of effect sizes. Treatment A has significant effects from the 40th percentile upwards. Again, effect sizes are largest at the top of the outcome distribution. Also treatment B appears to generate the intended effects at some points of the outcome distribution, being significant at the median and at the 60th percentile, but always with smaller estimated effect sizes and overall insignificant average impact.

Overall, we draw two conclusions: first, active learning seems to be effective in increasing financial literacy across the entire distribution; second, regarding the savings and the investment index, effects are largest for those individuals with higher levels of outcomes and zero for people at the bottom of the outcome distributions for savings and investing. Thus, financial education may not be beneficial for the most constrained individuals in our sample, confirming the intuition that investments in financial literacy may not be rational and beneficial for all individuals (cf. Lusardi et al., 2017).

6.2 Subgroup analyses

Turning to an investigation of treatment effects by subgroups along observable characteristics of the respondents, we examine the possibility of heterogeneous treatment effects for three subgroups which generally are known to have different levels of ex-ante financial literacy and may respond differently to financial education programs (cf. Lusardi and Mitchell, 2014). First, gender differences are treated as a stylized fact in the literature, with men scoring higher on financial literacy tests than women in most surveys. Second, financial literacy

is correlated with general educational attainment. In our sample, nearly 32 percent cannot read or write in any language and only and 25.35 percent have more than primary education. Thus differential impacts conditional on general educational attainment may occur. Third, we examine differential impacts conditional on baseline financial literacy levels.

The three panels of Table 7 show an investigation of heterogeneous treatment effects for the subgroups discussed above. In each panel, binary indicators for each group are interacted with the treatment dummies to estimate the heterogeneous effects. The results are mixed:

Starting with the impact on financial literacy scores (column 1), it can be observed that treatment effects of treatment A appear to be heterogeneous only with respect to gender. First, the positive treatment effect on financial literacy may be driven predominately by a very strong treatment effect on males' financial literacy scores whereas the treatment effect for females appears to be small and insignificant (*Panel A*). Considering the other indicators, no significant interactions appear to exist (*Panels B anc C*).

Turning to outcomes with regard to financial behaviors, however, indicates that treatment effects appear to be less heterogeneous and the average positive treatment effects discussed above are still present when investigating effects for subgroups.

Apart from strong negative interaction effects of the male indicator with the dummy for treatment B (suggesting males respond negatively to this treatment leading to worse outcomes on the debt index but) none of the interactions appear to be statistically significant and meaningful. One exception may be that those who have above average financial literacy scores at baseline appear to respond more positively to the treatments with respect to budgeting behavior, i.e. respondents may only be able to translate the benefits of the training into action (e.g. creating a written household budget) if they are relatively knowledgeable at baseline.

Interestingly, impacts on downstream financial behaviors such as savings and investments are not conditional on skill-levels (education or financial literacy). This is in

contrast to other experiments: Bjorvatn and Tungodden (2010) report that business training in Tanzania is most effective for low-skilled individuals. Drexler et al. (2014) show that benefits of "rules of thumb" training are especially driven by large impacts on low-skilled respondents (cf. Drexler et al., 2014, p.25). Finally, Fort et al. (2016) document that exogenous variation in bank information policies impacts financial literacy and financial behavior, with effects being highest for low-educated elderly households. Benefits of active learning, however, appear to be universal for our sample and generally not contingent on low-ability.

7 Robustness

We demonstrate in various robustness checks that results of the main paper are robust. These checks are briefly reported here, while details are provided in Appendix B, and cover four main areas: (i) As the sample is characterized by some attrition we carefully address this issue showing that there is no reason for concern, among others by a bounds analysis testing results under extreme assumptions and inverse probability weighting for selection into endline survey . (ii) We rerun the analyses on binary outcomes with logit and probit-models. (iii) While the measure of financial literacy used here has better psychometric properties than simple unweighted sum-scores of standard items, we show that also the more common measures lead to qualitatively identical results.

8 Conclusion

This research contributes to revealing the determinants of successful financial education. As a potentially crucial determinant of effective education we test an "active learning" approach versus "traditional lecturing" which appears to be dominant in current financial education programs. Active learning has been shown to be clearly superior in the field of higher science education which provides a strong motivation to test such an approach in financial education.

In doing so, we employ a design similar to Drexler et al. (2014) by comparing two types of financial education treatments. However, instead of comparing two different curricula and thus varying the content, we test the impact of *how* financial education is conducted using the same content, teaching the same target group and relying on the same teachers.

The main result is clear cut: We study five outcome groups of financial behavior, such as savings behavior, plus the outcome on financial literacy, and find that active learning outperforms traditional lecturing. In four out of six cases active learning has the intended effect to a significant degree, while traditional lecturing never has a significant effect. Coefficients of active learning tend to be clearly larger, although the differential treatment effects compared to lecturing are significant only for the savings domain and for a single item in the debt index. Still, in particular given the relatively limited power of our cluster-RCT with 1,162 vendors in the endline survey and a short training of only 120 minutes, the advantage of active learning seems quite strong. Given that some previous studies have tested the effects of financial education programs in developing countries that rely on video screenings or information interventions (e.g. De Mel et al., 2011; Carpena et al., 2017), we argue that one explanation of muted effects observed in the present literature may lie in the form of exposition centered instructional design that falls short of translating financial literacy into financial behavior.

Thus, our experiment adds to the growing body of high-quality evidence that financial education can positively impact financial behavior, especially savings behavior (cf. Skimmyhorn 2016; Calderone et al., 2017)

In a second step we aim for understanding more about the mechanisms intermediating the financial education into changes of financial behavior. For this purpose, we examine the role of financial literacy, self-control and financial confidence each by applying a two-step estimation in line with Sayinzoga et al. (2016), where the intermediating variables are instrumented by the financial education treatments. We find, first, that active learning has a

positive impact on all three intermediating variables, whereas lecturing only has an impact on attitudes. Second, we reveal, that financial outcomes seem to be impacted by changes in specific intermediating variables, suggesting that the transfer from education to change in behavior differs across the kind of behavior. This would call for financial education that is not only specific to target groups (cf. Gibson et al., 2014; Doi et al., 2016) but also to the outcome variables. Obviously, more research in this direction seems necessary in order to fully understand the mechanisms at play.

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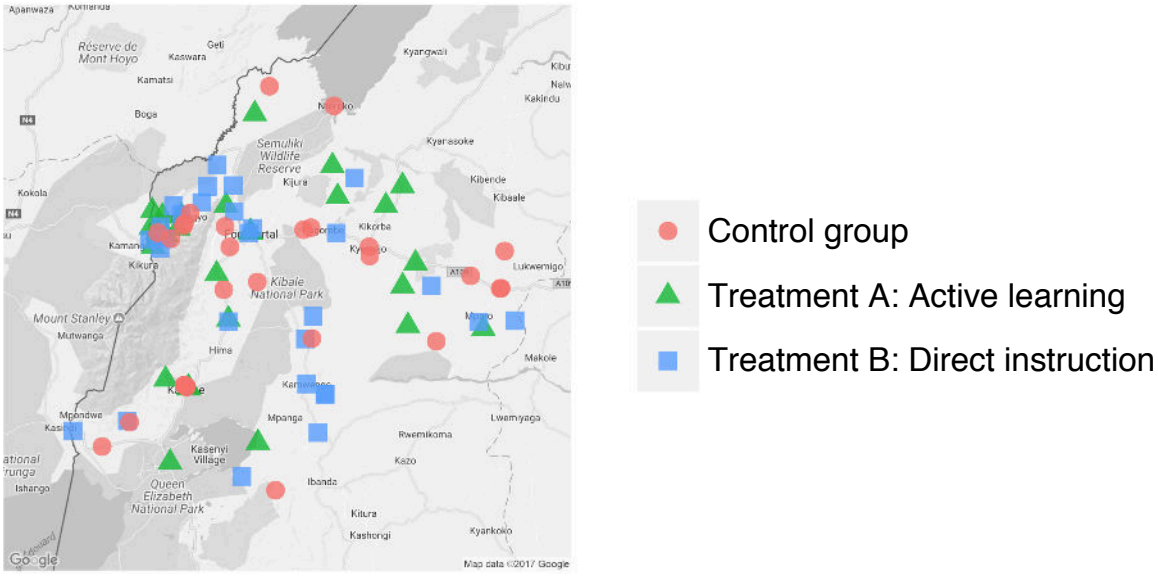

Figure 1: Location and treatment status of 83 clusters

Table 1: Sample overview and response-rates

	Wave	Control group	Treatment groups		Total
			Treatment A	Treatment B	
Clusters (marketplaces) (%)	Baseline	28 (33.73%)	27 (32.54%)	28 (33.73%)	83 (100%)
	Endline	28 (33.73%)	27 (32.54%)	28 (33.73%)	83 (100%)
Individuals (%)	Baseline	456 (35.32 %)	414 (32.07%)	421 (32.61%)	1,291 (100%)
	Endline	417 (35.89%)	384 (33.05%)	361 (31.06%)	1,162 (100%)
	Attrition (individuals)	39 (8.55%)	30 (7.25%)	60 (14.25%)	129 (9.99%)

Notes: Randomization was done in Stata and is reproducible. The cluster-level dataset contained one duplicate cluster (market) which was known by two different names in the local languages. Thus, randomization was done with 84 markets. This was discovered only after randomization and initial field activities. The duplicate (which was allocated to group A) was removed ex post. The baseline survey was conducted between November 2nd and December 19th, 2015. The follow-up survey was conducted between April 6th and July 19th of 2016 with additional tracking efforts and surveys in October 2016 and February 2017 (see Timeline Figure A1 in Appendix A).

Table 2: Summary statistics and randomization-balance at baseline

		Full sample	Control (C)	Treatment (A)		Treatment (B)	
	Obs.	Mean (SD)	Mean (SD)	Mean (SD)	Diff. from C [SE]	Mean (SD)	Diff. from C [SE]
Panel A: Household characteristics at baseline							
Household size	1,259	6.83	6.86	7.00	0.14	6.64	-0.22
		(3.77)	(3.52)	(4.25)	[0.33]	(3.52)	[0.33]
No. of contributors	1,277	2.12	2.12	2.06	0.06	2.19	0.06
		(1.98)	(2.01)	(1.95)	[0.14]	(1.98)	[0.12]
No. of children	1,273	4.17	4.29	4.29	0.00	3.94	-0.35
		(2.95)	(2.88)	(3.22)	[0.27]	(2.75)	[0.24]
No. of rooms	1,284	3.23	3.37	3.14	-0.23	3.16	-0.21
		(1.84)	(1.94)	(1.78)	[0.17]	(1.79)	[0.17]
Owns dwelling	1,291	0.74	0.78	0.70	-0.08*	0.72	-0.56
		(0.44)	(0.41)	(0.46)	[0.05]	(0.45)	[0.05]
Assets (z-score)	1,162	0.00	0.07	-0.06	-0.13	-0.01	-0.08
		(1.00)	(1.07)	(0.99)	[0.13]	(0.93)	[0.11]
Tap water	1,291	0.46	0.40	0.50	0.10	0.47	0.07
		(0.50)	(0.49)	(0.50)	[0.08]	(0.50)	[0.09]
Monthly income^{+}	1,250	219,867	222,400	203,232	-19,168	233,565	11,165
		(327,192)	(337,538)	(283,402)	[32,471]	(355,164)	[33,255]
Monthly consumption^{+}	1,286	592,775	592,219	616,570	24,350	569,925	-22,294
		(408,015)	(402,390)	(448,328)	[49,884]	(370,276)	[39,842]
Panel B: Respondent characteristics at baseline							
Female	1,265	0.80	0.80	0.79	-0.01	0.80	0.00
		(0.40)	(0.40)	(0.41)	[0.05]	(0.40)	[0.06]
Age	1,277	36.23	37.72	35.38	-2.34*	35.46	-2.26*
		(11.89)	(12.36)	(11.53)	[1.18]	(11.59)	[1.19]
Education	1,282	6.83	7.11	6.61	-0.49	6.74	-0.36
		(3.69)	(3.66)	(3.71)	[0.38]	(3.70)	[0.35]
Literate	1,238	0.68	0.70	0.64	-0.06	0.68	-0.02
		(0.47)	(0.46)	(0.48)	[0.05]	(0.47)	[0.04]
Econ. dependent	1,285	0.16	0.15	0.16	0.01	0.16	0.01
		(0.36)	(0.36)	(0.37)	[0.03]	(0.37)	[0.03]
Receives aid	1,277	0.22	0.24	0.21	-0.03	0.21	-0.03
		(0.41)	(0.43)	(0.41)	[0.04]	(0.40)	[0.03]
Married	1,291	0.62	0.59	0.60	0.01	0.66	0.07
		(0.49)	(0.49)	(0.49)	[0.04]	(0.48)	[0.04]
Main contrib.	1,291	0.70	0.70	0.74	0.04	0.67	-0.03
		(0.46)	(0.46)	(0.44)	[0.04]	(.47)	[0.04]
HH head	1,291	0.55	0.55	0.58	0.03	0.51	-0.04
		(0.50)	(0.50)	(0.49)	[0.05]	(0.50)	[0.05]
Years as vendor	1,263	7.42	8.25	7.77	-0.48	6.18	-2.07**
		(7.46)	(8.08)	(7.41)	[0.90]	(6.63)	[0.86]
Sells nonfood items	1,291	0.14	0.14	0.14	0.00	0.14	0.00
		(0.35)	(0.35)	(0.35)	[0.04]	(0.35)	[0.04]
Numeracy (z-score)	1,291	0.05	0.01	0.07	0.06	0.07	0.06
		(0.97)	(0.97)	(0.94)	[0.08]	(0.98)	[0.09]
Self-control (z-score)	1,273	0.00	-0.01	0.07	0.08	-0.05	-0.04
		(1.00)	(0.99)	(1.02)	[0.08]	(0.99)	[0.08]
Patience (z-score)	1,280	0.00	0.02	-0.07	-0.09	0.05	0.04
		(1.00)	(1.01)	(1.04)	[0.09]	(0.95)	[0.07]
Trust (z-score)	1,291	0.00	0.00	-0.04	-0.04	0.04	0.05
		(1.00)	(0.98)	(1.02)	[0.08]	(1.00)	[0.08]

-continued -

Altruism (z-score)	1,267	0.00	-0.04	0.05	0.09	0.00	0.04
		(1.00)	(0.99)	(1.02)	[0.08]	(0.98)	[0.06]
Fatalist worldview	1,253	0.00	0.03	-0.02	-0.05	-0.01	-0.04
(z-score)		(1.00)	(0.99)	(1.03)	[0.08]	(0.98)	[0.10]
General risk attitude	1,262	3.61	3.53	3.67	0.14	3.66	0.13
(0-10)		(2.42)	(2.39)	(2.40)	[0.17]	(2.48)	[0.19]
Specific risk attitude	1,272	3.78	3.72	3.78	0.06	3.85	0.13
(0-10)		(2.52)	(2.53)	(2.46)	[0.16]	(2.57)	[0.19]
Panel C: Outcome measures at baseline							
(1) Fin. literacy score	1,291	0.03	0.00	0.10	0.10	0.00	0.00
		(0.98)	(1.00)	(0.96)	[0.11]	(0.99)	[0.11]
(2) Budgeting index	1,248	-0.02	0.00	-0.10	-0.10	0.04	0.04
		(0.96)	(1.00)	(0.91)	[0.08]	(0.97)	[0.08]
(3) Savings index	1,161	0.04	0.00	0.07	0.07	0.05	0.05
		(1.04)	(1.00)	(1.14)	[0.09]	(1.00)	[0.10]
(4) Borrowing index	1,126	0.06	0.00	0.01	0.01	0.17	0.17
		(1.25)	(1.00)	(1.32)	[0.08]	(1.43)	[0.11]
(5) Investments index	1,142	0.04	0.00	0.00	0.00	0.10	0.10
		(1.07)	(1.00)	(1.08)	[0.11]	(1.14)	[0.11]
(6) Fin. services index	1,241	0.02	0.00	-0.04	-0.04	0.11	0.11
		(1.07)	(1.00)	(0.99)	[0.10]	(1.07)	[0.11]
F-test of joint orthogonality: any treatment (p-value)					0.308		
Observations					862		
Clusters					83		

Notes: + indicates that the currency denominated outcome (in Ugandan Shilling (UGX)) is winsorized at the 99th percentile. Differences between treatment and control groups are estimates from OLS-regressions. Standard errors (clustered at the market-level) are reported in square brackets. Tests are unadjusted for multiple hypothesis testing. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Main experimental results (ANCOVA)

	(1) Fin. literacy (z)	(3) Budget index (z)	(4) Savings index (z)	(5) Debt index (z)	(6) Investment index (z)	(7) Fin. services index (z)
Treatment A	0.134*	0.045	0.162**	0.104*	0.284***	0.109
	(0.079)	(0.087)	(0.071)	(0.057)	(0.097)	(0.077)
Treatment B	0.079	0.009	0.011	-0.038	0.168	0.149
	(0.071)	(0.088)	(0.077)	(0.075)	(0.119)	(0.095)
$A - B = 0$ (p-value)	0.494	0.690	0.079*	0.052*	0.353	0.678
R^2	0.051	0.108	0.151	0.009	0.144	0.130
Mean (SD) of y_t in control group	0.000 (1.000)	0.000 (1.000)	0.000 (1.000)	0.000 (1.000)	0.000 (1.000)	0.000 (1.000)
Observations	1,160	1,114	1,160	1,108	1,007	1,136
Clusters	83	83	83	83	83	83
District FEs	yes	yes	yes	yes	yes	yes
$y_{(t-1)}$ covariate	yes	yes	yes	yes	yes	yes

Notes: Table shows OLS regression results of ANCOVA models. The dependent variables (y_t) are indices of financial literacy and financial behavior and are standardized to have a zero mean and a standard deviation of one for the control group. Thus, coefficients can be interpreted as effect sizes (Glass's Δ). All currency denominated outcomes (in Ugandan Shilling (UGX)) within the indices in columns (4), (5), and (6) are winsorized at the 99th percentile All models include the lagged outcome at baseline and district-level fixed effects. Standard errors (clustered at the market-level) are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Analysis of individual index components saving and investing

	(a) Savings index			(b) Investment index	
	(1) Any savings	(2) Total savings+	(3) Net savings+	(4) Total investments+	(5) Business formally registered
Treatment A	0.036*	109,186*	145,480**	90,173*	0.077**
	(0.020)	(65,132)	(64,784)	(47,461)	(0.034)
Treatment B	0.025	-32,519	-14,226	41,801	0.060
	(0.023)	(68,588)	(66,476)	(63,124)	(0.037)
$A - B = 0$ (p-value)	0.612	0.052*	0.025**	0.487	0.640
R^2	0.023	0.244	0.131	0.184	0.035
Mean (SD) of y_t in control group	0.879 (0.326)	513,629 (937,119)	380,568 (973,769)	301,067 (526,957)	0.232 (0.423)
Observations	1,160	1,162	1,162	1,053	1,110
Clusters	83	83	83	83	83
District FEs	yes	yes	yes	yes	yes
$y_{(t-1)}$ covariate	yes	yes	yes	yes	yes

Notes: Table shows OLS regression results of ANCOVA models. Columns (1) and (5) are linear probability models. All models include the lagged outcome at baseline and district-level fixed effects. + indicates that the currency denominated outcome (in Ugandan Shilling (UGX)) is winsorized at the 99th percentile. Standard errors (clustered at the market-level) are reported in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: 2SLS-Regressions to probe the causal mechanisms from financial education to savings outcomes

	1st stage			2nd stage		
	Panel A: Savings outcomes					
	(1) Fin. literacy	(2) Self-control	(3) Fin. confidence	(4) Savings index(z)	(5) Savings index(z)	(6) Savings index(z)
Treatment A	0.156*	0.152*	0.160**			
	(0.087)	(0.079)	(0.081)			
Treatment B	0.083	-0.058	0.193**			
	(0.076)	(0.092)	(0.083)			
Fin. literacy*				1.248		
				(0.824)		
Self-control*					0.860*	
					(0.518)	
Fin. confidence *						0.515
						(0.476)
Observations	1,161	1,156	1,026	1,161	1,156	1,026
	Panel B: Investment outcomes					
	Fin. literacy	Self-control	Fin. confidence	Investment index (z)	Investment index (z)	Investment index (z)
Treatment A	0.188 **	0.134	0.141*			
	(0.094)	(0.086)	(0.079)			
Treatment B	0.098	-0.073	0.217***			
	(0.083)	(0.093)	(0.074)			
Fin. literacy*				1.683*		
				(0.955)		
Self-control*					0.734	
					(0.660)	
Fin. confidence*						1.321*
						(0.742)
Observations	1,037	1,033	920	1,037	1,033	920
Clusters	83	83	83	83	83	83
District FEs	yes	yes	yes	yes	yes	yes
$y_{(t-1)}$ covariate	no	no	no	no	no	no

Notes: Results show two stage regressions. All models district-level fixed effects. Standard errors (clustered at the market-level) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Simultaneous-quantile regressions for indices of financial knowledge, savings, and investment

		(1) FL score (z)	(2) avings index (z)	(3) Investment index (z)
20th percentile	Treatment A	0.161	0.035	0.027
		(0.119)	(0.030)	(0.017)
	Treatment B	0.281**	0.015	0.015
		(0.120)	(0.031)	(0.020)
	$A - B = 0$ (p-value)	0.372	0.382	0.560
40th percentile	Treatment A	0.180*	0.026	0.095**
		(0.109)	(0.020)	(0.042)
	Treatment B	0.204*	-0.002	0.044
		(0.108)	(0.020)	(0.035)
	$A - B = 0$ (p-value)	0.804	0.217	0.315
Median	Treatment A	0.108	0.037	0.155**
		(0.111)	(0.027)	(0.0649)
	Treatment B	0.101	-0.003	0.108**
		(0.096)	(0.027)	(0.055)
	$A - B = 0$ (p-value)	0.941	0.180	0.527
60th percentile	Treatment A	0.154	0.068**	0.301**
		(0.135)	(0.029)	(0.143)
	Treatment B	-0.022	0.007	0.137*
		(0.115)	(0.029)	(0.076)
	$A - B = 0$ (p-value)	0.154	0.052*	0.263
80th percentile	Treatment A	0.136	0.134*	0.466**
		(0.095)	(0.073)	(0.208)
	Treatment B	-0.037	-0.037	0.187
		(0.096)	(0.057)	(0.206)
	$A - B = 0$ (p-value)	0.091*	0.022**	0.162
Observations		1,162	1,160	1,007
Clusters		83	83	83
District FEs		yes	yes	yes
$y_{(t-1)}$ covariate		yes	yes	yes
Mean (SD) of y_t in control group		0.000 (1.000)	0.000 (1.000)	0.000 (1.000)

Notes: Results present simultaneous-quantile regressions for the impact of the financial education treatments A and B on indices of financial knowledge (1), savings (2), and investments (3). The dependent variables are equally weighted z-score indices of their respective components as discussed in Section 2.4 and are standardized to have a zero mean and a standard deviation of one for the control group. Thus, coefficients can be interpreted as effect sizes (Glass's Δ). The output presents treatment effects for each quartile and the median. All currency denominated outcomes (in Ugandan Shilling (UGX)) within the indices in columns (2) and (3) are winsorized at the 99th percentile. All models include the lagged outcome at baseline and district-level fixed effects. Standard errors in parentheses are bootstrapped with 1,000 replications. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Heterogeneous treatment effects for subgroups

	(1) FK score (z)	(2) Budget index (z)	(3) Savings index (z)	(4) Debt index (z)	(5) Investment index (z)	(6) Fin. services index (z)
Panel A: Male respondents						
Treatment A	0.053	0.029	0.184**	0.141**	0.224**	0.062
	(0.100)	(0.085)	(0.076)	(0.065)	(0.0863)	(0.077)
Treatment B	0.044	0.001	0.0391	0.0470	0.168	0.168*
	(0.086)	(0.089)	(0.072)	(0.088)	(0.106)	(0.091)
Male	-0.101	0.307**	0.306**	0.407***	0.406**	0.362***
	(0.101)	(0.147)	(0.125)	(0.122)	(0.172)	(0.109)
Treatment A × Male	0.392**	0.079	-0.0442	-0.205	0.274	0.157
	(0.170)	(0.214)	(0.204)	(0.185)	(0.348)	(0.195)
Treatment B × Male	0.155	0.059	-0.101	-0.409**	0.0849	-0.0121
	(0.157)	(0.182)	(0.244)	(0.194)	(0.285)	(0.190)
Obs.	1,138	1,091	1,136	1,085	987	1,088
R2	0.053	0.123	0.158	0.018	0.177	0.156
Panel B: Respondents with beyond primary education						
Treatment A	0.121	0.066	0.183**	0.134**	0.237**	0.110
	(0.095)	(0.082)	(0.077)	(0.065)	(0.100)	(0.082)
Treatment B	0.149	0.044	0.061	-0.044	0.128	0.104
	(0.091)	(0.081)	(0.067)	(0.100)	(0.112)	(0.106)
Educated	0.165	0.303***	0.334***	0.184*	0.055	0.228**
	(0.105)	(0.106)	(0.098)	(0.108)	(0.113)	(0.100)
Treatment A × Educated	0.126	-0.053	-0.032	-0.102	0.229	0.032
	(0.147)	(0.147)	(0.164)	(0.139)	(0.215)	(0.168)
Treatment B × Educated	-0.256	-0.107	-0.172	0.037	0.169	0.199
	(0.164)	(0.174)	(0.156)	(0.187)	(0.267)	(0.138)
Obs.	1,162	1,114	1,160	1,108	1,007	1,111
R^2	0.0577	0.120	0.164	0.014	0.150	0.146
Panel C: Respondents with above average financial literacy at baseline						
Treatment A	0.099	-0.140	0.260**	0.162*	0.198*	0.084
	(0.105)	(0.0942)	(0.105)	(0.087)	(0.110)	(0.118)
Treatment B	0.092	-0.121	0.158	-0.068	0.071	0.100
	(0.099)	(0.104)	(0.108)	(0.099)	(0.109)	(0.137)
Fin. literate	0.090	-0.122	0.258**	0.046	0.055	0.118
	(0.132)	(0.079)	(0.111)	(0.095)	(0.091)	(0.093)
Treatment A × Fin. literate	0.079	0.336**	-0.194	-0.106	0.145	0.036
	(0.143)	(0.132)	(0.144)	(0.131)	(0.143)	(0.136)
Treatment B × Fin. literate	-0.019	0.250*	-0.283*	0.060	0.188	0.102
	(0.154)	(0.146)	(0.143)	(0.124)	(0.166)	(0.160)
Obs.	1,162	1,114	1,160	1,108	1,007	1,111
R^2	0.052	0.115	0.157	0.010	0.150	0.136
Clusters	83	83	83	83	83	83
District FEs	yes	yes	yes	yes	yes	yes
$y_{(t-1)}$ controls	yes	yes	yes	yes	yes	yes

Notes: Panel A shows results by gender, with "male" being an indicator variable for male respondents. Panel B shows interactions between treatments and education with "Educated" being an indicator identifying respondents with above primary education at baseline. Panel C reports interactions between the treatments and baseline financial literacy with "Fin. literate" being an indicator for a respondent scoring higher on the baseline financial literacy assessment than the average respondent in the full baseline sample. Standard errors, clustered at the market-level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.