

Kaiser, Tim; Menkhoff, Lukas

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# Active learning improves financial education: Experimental evidence from Uganda

Tim Kaiser and Lukas Menkhoff

## Abstract

We conduct a randomized field experiment to study the effects of two financial education interventions offered to small-scale retailers in rural western Uganda. The treatments contrast “active learning” with traditional “lecturing” within standardized lesson-plans. After six months, active learning has a positive effect on savings and investment outcomes, in contrast to small or zero effects for lecturing. After four years, estimates come with substantial uncertainty but are generally larger for the active learning group, such as a 60 percent increase in investments. As an adverse outcome, reported late payment on loans increases by about 30 percent for both treatments. The findings suggest that teaching methods can play an important role in affecting how financial education programs impact financial behavior and outcomes.

JEL-Classification: O16 (savings), I21 (analysis of education), G53 (financial literacy)

Keywords: financial behavior, financial literacy, active learning, lecturing, training method, field experiment

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Tim Kaiser, University of Koblenz-Landau, 76829 Landau, Germany; [kaiser@uni-landau.de](mailto:kaiser@uni-landau.de)

Lukas Menkhoff, Humboldt-Universität zu Berlin, German Institute for Economic Research (DIW Berlin), 10108 Berlin, and Kiel Institute for the World Economy, Germany; [lmenkhoff@diw.de](mailto:lmenkhoff@diw.de)

# **Active learning improves financial education: Experimental evidence from Uganda**

## **1 Introduction**

Financial education is a high priority for policymakers globally. Many developing economies have implemented national financial education strategies that aim to promote financial inclusion and to contribute to financial stability (OECD, 2015). As enormous resources are being invested into financial education, the effectiveness of financial education programs is an exigent issue.

Accordingly, the issue of effectiveness is intensively debated (see Hastings et al., 2013; Lusardi and Mitchell, 2014), albeit with a change in focus. Some years ago, studies raised concerns about the general effectiveness of financial education (e.g., Cole et al., 2011; Fernandes et al., 2014), but with a rapidly increasing number of field experiments, new meta-analyses show that effects are positive, on average (Miller et al., 2015; Kaiser and Menkhoff, 2017; Kaiser et al., 2020). Thus, a more recent debate focuses on improving effectiveness of interventions, including that of large-scale financial education programs (e.g., Bruhn et al., 2014, 2016; Brown et al., 2016; Cole et al., 2016; Berry et al., 2018). Therefore, researchers are evaluating alternatives to typical lecture-based classroom-programs. Proposals include tailoring interventions to narrow target groups (Doi et al., 2014; Drexler et al., 2014), introducing personalized elements like counseling (Carpena et al., 2019), and using mass media (Berg and Zia, 2017), among others. In contrast, we study an approach that aims for improving effectiveness within still pervasive classroom-based programs, i.e., by applying the concept of active learning to the field of financial education.

“Active learning” is an established and well evaluated umbrella term for interactive teaching methods that engage and involve participants. Specifically, in our setting, participants

not only speak more, discuss with each other, solve mini cases, and engage with prepared teaching material, but also show higher levels of physical activity than attendees of lecture programs. The advantage of an “active learning” approach over traditional “lecturing” is documented in large empirical literatures from other domains, including science, technology, engineering, and mathematics instruction (e.g., Deslauriers et al., 2011, 2019; Ruiz-Primo et al., 2011; Freeman et al., 2014; Wieman, 2014; Sheridan and Smith, 2020). We build on this literature from other domains and apply the active learning approach to financial education.

We conduct a cluster-randomized field experiment contrasting an “active learning” program with a traditional “lecturing” program, while keeping content, teachers, and intensity of training constant. The field-experiment was conducted in rural western Uganda with 1,291 small scale retailers in November and December 2015. The midline survey took place about six months later, while the endline was conducted in September 2019, i.e., almost four years after the baseline, as a phone survey. The latter comes with a restricted number of survey items and an attrition of 52 percent relative to baseline.

A clear result emerges: The group randomly allocated to the “active learning” program experiences an improvement in process outcomes (self-reported self-control and financial confidence) and, ultimately, some dimensions of financial behavior. By contrast, the group allocated to “lecturing” only reports increased levels of financial confidence but does not appear to change financial behaviors. Specifically, the active learning program causes an increase in two out of five outcome-domains at midline, each captured by an index. Among others, total savings increase by about 20 percent relative to the control group and total investments into the own business by about 35 percent. After almost four years these increases relative to the control group tend to persist (possibly at an even higher level), albeit estimates come with substantial uncertainty. Yet, our results suggest that at least part of the “active learning” group has persistently changed their financial behavior.

However, a rather worrying effect is that both treated groups report a significantly higher degree of late payments on loans than the control group. As we do not observe (significant) treatment effects on the frequency of borrowing, loan volume or loan repayment, and as savings and investments at both the midline and endline survey are uncorrelated with the likelihood of late payments, the reasons for increased late payments remain speculative within the setting of this study. This late repayment result is generally consistent with the evaluation of a large program in Brazil (Bruhn et al., 2016), which also finds adverse effects on debt-taking behavior among students.

Thus, the overall welfare effects of the financial education treatments examined are ambiguous until one understands if the late repayment results are real and, if so, how big these effects are relative to the benefits of the treatments. The opposing effects may be heterogeneously distributed, i.e., some individuals are better off and others worse. Alternatively, the effects may also occur for the same individuals, e.g., that those saving and / or investing more do less often service their loans on time because of additional liquidity constraints. Our experiment does not allow a definitive answer to this issue.

Our research not only adds to the growing literature that advances the understanding of the differing impact of delivery channels of financial education, but it also contributes to the debate on how to design effective and scalable training programs. Drexler et al. (2014) shows that a heuristics-based approach, relying on the simplification of complex financial concepts (“rule-of-thumb-training”), generates larger behavioral impacts than the teaching of traditional curricula (“full technical training”), especially among low-skilled individuals and individuals with low baseline financial literacy and motivation. Skimmyhorn et al. (2016) reruns this type of experiment but does not find evidence to support differential effects regarding the two types of curricula, probably because their sample of respondents has high levels of *ex-ante* human capital, confirming the result of Drexler et al. (2014) that the benefits of the “rule-of-thumb”

approach may be driven by strong effects on low-skilled individuals. Lusardi et al. (2017a) presents evidence from online experiments that interactive tools, narratives, and financial education videos may be more effective than written informational content in affecting financial knowledge and self-efficacy.

Moreover, Berg and Zia (2017) show that financial education interventions that primarily target non-cognitive channels through mass media can significantly impact financial behavior. Campos et al. (2017) demonstrate that a psychology-based training program for entrepreneurs, teaching a pro-active mindset, outperforms traditional business training for self-employed individuals in West Africa with strong differential effects on business profits. Iterbeke et al. (2020) shows that ability matching and differentiated instruction may especially benefit low-ability students. Finally, Carpena et al. (2019) shows that complementing classroom instruction with personalized elements, like individualized counseling and goal setting, yields higher treatment effects on financial behaviors.

Much of this evidence suggests that interventions relying on lecturing within classroom-settings appear to have relatively small effects. Thus, alternative approaches to lecture-based education seem to be important. Some argue that “one-size-fits-all” (Drexler et al., 2014; Carpena et al., 2019) classroom programs are not suitable for improving financial behavior and programs must be complemented with individualized elements. In contrast, we show that even within a one-size-fits-all classroom program, active learning techniques may affect how financial education programs impact financial behavior.

## **2 Treatments and experimental design**

### **2.1 Context and financial education interventions**

As a step toward promoting financial inclusion, the Bank of Uganda (*BoU*), the country's central bank, established a national strategy for financial literacy in Uganda.<sup>1</sup> This strategy seeks to foster the personal financial knowledge and behavior of different target groups, including school students, young adults in urban settings, and adults in rural areas. The *BoU* 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) savings, (iii) credit, (iv) business investing, and (v) payments and financial service providers.

Focusing on the financial education strategy's major objective to improve rural outreach, *GIZ* developed educational formats for the target population of the rural self-employed. These programs teach how to create a written household budget as well as how to keep track of household's financial inflows and outflows. It also encourages household savings, explains the costs and benefits of various forms of credit, explains the trade-off between risk and return regarding productive investments into micro-enterprises, highlights the benefits of diversification among sources of income and investments, as well as informs about the costs and benefits of using financial services provided by regulated financial institutions. The trainings emphasize the importance of direct and opportunity costs associated with each type of financial product, which sometimes also means considering how financial resources may be protected against the demands of peers. Thus, this training promotes 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 both the use of expensive credit

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<sup>1</sup> See: [https://www.bou.or.ug/opencms/bou/bou-downloads/Financial\\_Inclusion/Strategy-for-Financial-Literacy-in-Uganda\\_August-2013.pdf](https://www.bou.or.ug/opencms/bou/bou-downloads/Financial_Inclusion/Strategy-for-Financial-Literacy-in-Uganda_August-2013.pdf) - last accessed February 9, 2018.

provided by informal moneylenders and the take up of costly loans to finance consumption expenditures in general.

At the time of our study, a *GIZ*-project in Western Uganda developed a new delivery method for the standard educational intervention. This created the opportunity to study the differential impact of alternative delivery approaches to financial education for the same target group. Thus, *GIZ* cooperated with external researchers to test these two approaches via an RCT. While these two programs are identical with regards to the content areas covered and their intensity (two hours), they differ in their concrete method of instruction, i.e., *how* this content is taught (cf. [Table A1](#) in Appendix A).

The financial education training using “active learning” methods is highly learner-centered (see Ruiz-Primo et al., 2011; Freeman et al., 2014; Sheridan and Smith, 2020). Its main feature are five distinct stations, each 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., by completing budgeting exercises or sorting savings options regarding their safety risk/return-profiles. Respondents are encouraged to share their experiences. The trainer has the role of a learning facilitator.

The traditional “lecturing” program, on the other hand, is organized as a community lecture, i.e., an “exposition-centered” (Freeman et al., 2014, p.8410) approach, relying mostly on lecturing with the aid of a flipchart and some room for participants to ask questions or to share experiences. Here, the lecturer explains important concepts and demonstrates, e.g., how to create a written budget and how different financial products could be categorized. The trainer can be characterized as a lecturer rather than a facilitator.

Thus, there are four characteristics that distinguish active learning from lecturing treatments: (i) Participants speak more. Based on our own classroom-observations and statements of trainers, participants speak about 30 to 60 percent of the time (i.e., 40 to 70 percent



for the trainer) while this share is less than 20 percent in the lecturing treatment. (ii) Discussion between participants is common while it is rare in lecturing treatments. (iii) Active learning always uses prepared teaching materials, often a poster that needs to be completed by sticking cards on it. Posters may show intuitive structures and mini cases, such as a “money tree” in the case of financial management. (iv) Participants are physically activated. They get up to stand in a half-circle in front of the poster, may move toward the poster, move to the next station, and may change position while discussing; by contrast, during the lecturing treatment, participants typically stay seated.

To evaluate the impact of the financial education programs, we employ the same group of newly recruited teachers to deliver both financial education treatments to the target groups. Thus, the different versions are not confounded with idiosyncratic characteristics of the teachers and implementation is not heterogeneous across experimental sites. The teachers are all enrolled in a program on banking and microfinance at a local university and have participated in a training of trainers offered by *BoU* and *GIZ*.

## **2.2 Experimental design**

We organize a cluster-randomized experiment to study the differential impact of the two financial education interventions on financial behaviors. The main outcomes of interest are changes in *financial behavior* within five sub-domains addressed by the financial education interventions, i.e., (i) budgeting, (ii) usage and handling of credit, (iii) savings, (iv) business investing and formalization, and (v) the use of formal payment- and other financial services, such as formal insurance. Randomization is done at the market-level, because these markets are too small to avoid spillovers between groups. There are 83 rural marketplaces in the seven districts of the Rwenzori Region in Western Uganda, collectively forming 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.<sup>2</sup> Because prior information about the marketplaces (such as exact 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.

Vendors were mobilized to participate in our survey and the financial education sessions by the local market-chairpersons. These chairpersons were blinded to the experimental condition and only told that there would be a half-day event organized by the local university including a survey. As the experimental group was unknown to the market-chairperson, no differential selection (mobilization) should be in place. However, the mobilization by itself may include a selection bias if market-chairpersons favor specific groups (such as their peers) over others in general. This would indeed impact the *external* validity of our experiment. Moreover, it may be expected that the program impact could be overestimated to some degree, as it is likely that chairpersons may select individuals whom they expect to be compliant. We cannot quantify this distortion but report that through all stages of our study compliance was quite high; for example, basically nobody left between survey and training in the treatment groups. While these implementation details may have implications for the external validity of the findings, the *internal* validity is not affected because these selection mechanisms will occur in all three groups.

After baseline survey, the treatment groups received either financial education treatment “active learning” or “lecturing” on the same day, immediately after the interviews. The “control

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<sup>2</sup> There is no administrative data available regarding the location, size, or vendors in these marketplaces.

group” did not receive any treatment. After baseline survey and subsequent financial education treatments, we conducted the midline as follow-up surveys approximately 6 months later. The endline followed another 3 years and 3 months later in the form of a telephone survey.

We report *ex ante* 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. Baseline intra-cluster-correlation (ICC) ranges from 0.030 (financial literacy score and budgeting index) to  $< 0.001$  (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. To put these minimal detectable effect sizes into perspective: The average effect size on financial behaviors in recent meta-analyses is about 0.1 SD units (Kaiser and Menkhoff, 2017; Kaiser et al., 2020).

### 2.3 Empirical strategy

To estimate the effect of the financial education interventions on financial literacy and behavior, we compare the two treatment groups “active learning” and “lecturing” with the “control group” (no treatment) at the time of the follow-up surveys. As selection into treatment is randomized, with balanced groups at baseline (see Section 3.2 and Appendix A), 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 (see 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 behavior) for individual  $i$  in cluster  $c$  at the time of follow-up ( $t$ ).  $y_{ic(t-1)}$  controls for the value of the outcome at baseline and  $\theta_d$  are district-level fixed-effects.  $A_{ic}^T$  and  $B_{ic}^T$  are dummy variables equal to one for respondents situated in a cluster assigned to the two treatments, respectively. Thus,  $\beta_1$  captures the (ITT) treatment effect of the active learning intervention, and  $\beta_2$  captures the (ITT)

treatment effect of the traditional lecturing intervention.  $\varepsilon_{ict}$  denotes the error-term. Standard errors are clustered at the market-level to account for the level of randomization.

To avoid problems inherent to testing multiple hypotheses (type-I-error inflation), we aggregate multiple related outcomes into index-measures of outcome 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  $\mu_k$  denoting the mean of  $y_k$  for the control group (C) and  $\sigma_k$  denoting the standard deviation of  $y_k$  for the control group. Following standard procedures (e.g., Kling et al., 2007; Karlan and Valdivia 2011), if some but not all of the components for an index for the dependent variable have missing values, we set the missing components to the mean value of the control group (i.e., zero). 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 relative to control.

### 3 Data

After mapping of the markets, piloting the survey tools and interventions, as well as randomization, we conducted a comprehensive baseline survey between November 2 and December 19, 2015. This dataset covers all vendors invited to participate in our survey (n=1,292). The questionnaires were translated into three local languages widely spoken in the area and the enumerators conducting the face-to-face interviews in the local languages were trained extensively prior to the field-activities.

### 3.1 Response rates

Following the baseline-survey and the subsequent financial education treatments, we conducted midline surveys between April 6 and July 19, 2016. Including extensive tracking efforts, our final response rate is high, especially given the relatively mobile target group (see timeline [Figure A1](#)): We follow up with 1,162 vendors, i.e., 90 percent of the initial sample at midline survey (see Table 1). Regarding the endline survey in September 2019, we did not have the resources to repeat the earlier surveys in the field. Instead, we conceptualized a shorter telephone survey that was conducted by a professional survey firm in Kampala, Uganda, on our behalf. Due to the long follow-up period and limited tracking possibilities, the response rate to the telephone survey is much lower: 53 percent of the midline sample and 48 percent of the baseline sample.

<Table 1 about here>

Attrition rates appear to vary experimental condition and survey round. Thus, we study attrition in more detail in [Table A2](#) and use the predicted probabilities to attrite based on these observables as inverse probability weights to account for the selection into the survey-round to all our regressions. In general, we observe that few observables predict attrition but that those in the lecture treatment are less likely to respond to the survey (significant at the 10-percent level), while those with higher savings and higher levels of financial literacy at baseline are more likely to respond to the survey sample at midline. We find no interaction effects of outcomes or other covariates with the treatments. At endline, we observe that those in the control group are more likely to be tracked with no difference in attrition between the two treatment groups. Additionally, the relevant outcomes are not predictive of attrition but more educated individuals and those with higher household consumption are more likely to be tracked at endline. Given the importance of attrition, we also probe the sensitivity of our results

by estimating bounds on the treatment effects with several scenarios imputing missing observations at the midline and endline. Details are provided in the robustness Section 5 and Appendix B.

### **3.2 Descriptive statistics and randomization balance**

Table 2 reports summary statistics at baseline for the three groups (i.e., control, active learning and lecturing) of the full sample and of the endline sample. The statistics for the midline sample are provided as [Table A3](#). The top panel in Table 2 shows variables that measure characteristics at the household level. For example, the average household size in the control group is 6.86 with an average of 2.12 adults contributing to the household's income. Several currency denominated outcomes had a long right tail, possibly indicating enumeration errors. Therefore, we winsorize all currency denominated outcomes at the 99<sup>th</sup> percentile. The resulting mean monthly household consumption value in control is about 593,000 UGX (\$519 in 2017 PPP).

<Table 2 about here>

The second panel reports variables at the respondent-level. The mean monthly individual income is around 220,000 UGX (\$192 in 2017 PPP). Household consumption is higher than added individual incomes because of subsistence farming, as reported by 83 percent of the sample. Our sample predominantly comprises women (80 percent) and the average age is 37 years. Only 28 percent report having participated in education beyond primary school. Our survey also includes a measure of financial literacy and psychological variables, such as self-control and financial confidence, which are standardized to have a mean of zero and a standard-deviation equal to one for the control group (see [Table A4](#)). These are used as process outcomes (Section 4).

The last panel shows descriptive statistics for outcome measures of 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 [Table A5](#)).

Causal inference within the estimation framework introduced in Section 2.4 rests on the random assignment of clusters to the treatment conditions. Randomization balance is probed by comparing the means of the three groups, i.e., control, active learning, and lecturing, as reported in F-tests of equality of means in columns (4) and (8) of Table 2. Overall, randomization seems to have worked reasonably, as there are no statistically significant differences across the three groups. Reassuringly, a joint test of orthogonality results in a p-value of 0.15 for the full sample and 0.68 for the endline sample.

## **4 Exploring process outcomes**

### **4.1 Financial literacy, self-control, and financial confidence**

As a first step toward understanding the potential mechanisms by which financial education may impact behavior, we report results on process outcomes (not pre-specified at experiment registration). These indicate three possible cognitive and non-cognitive mechanisms. The first candidate is an improvement in financial literacy that enables individuals to make better financial decisions, i.e., financial education would impact financial behavior through a cognitive channel. Evidence on this possible causal pathway is documented (cf. Fort et al., 2016; Sayinzoga et al., 2016) and appears to be supported by a larger sample of experimental work (see Kaiser and Menkhoff, 2017). However, it is a robust insight of (financial) education research that a good transfer of knowledge into behavior is fostered by additional non-cognitive elements (cf. Berg and Zia, 2017; Carpena et al., 2019). Thus, as a second potential mechanism, evidence shows that better self-control and, in line with this, future-oriented time-preferences seem to be associated with more savings (e.g., Ashraf et al.,

2006; Alan and Ertac, 2018; Lührmann et al., 2018). Third, financial confidence and attitudes may play an important role in financial behavior (e.g., Berg and Zia, 2017; Carpena et al., 2019; Carpena and Zia, 2020).

In the following we describe the formation of the three variables. “Financial literacy” is assessed through the sum of correct answers to five standard questions on financial literacy, which are transformed to a z-score, then scaled by the mean and standard deviation for the control group (see Table A4). “Self-control” is assessed by a survey item asking respondents to reply to the question: “If you get money, do you tend to spend it too quickly?” on a 1 (often) to 4 (never) rating scale. Responses are transformed into a z-score, scaled by the mean and standard deviation for the control group (see descriptive statistics in Table 2 and in Table A4). “Financial confidence” is assessed by multiple items that are aggregated into an unweighted z-score-index of its components. The index covers responses to binary questions and statements that are answered on a rating-scale. Questions were asked on whether 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, as well as whether respondents consider various products and options before making a financial decision (see definitions and descriptive statistics in Table A4).

Results are presented in Table 3. Active learning appears to have an effect on self-control and financial confidence, lecturing only impacts the measure of financial confidence, and the impact of both treatments on financial literacy appears to be small and not statistically significant.

<Table 3 about here>

Next, while there are no treatment effects on the aggregate financial literacy index, we examine if certain content areas of financial literacy were better understood by those individuals



who trained with the active learning program. We find a significant treatment effect on the probability of solving an item measuring the understanding of compound interest (see [Table A6](#)). Thus, one may speculate whether this effect contributes to an increased willingness to save and invest available funds. The increase in self-control may support this willingness. Somewhat differently from these process outcomes, the increase in financial confidence appears to be driven by a uniform effect across all index components (see [Table A7](#)). This may increase the willingness to take investment risks and to use financial products, however, it may also be related to some degree of overconfidence in financial matters and resulting financial behavior.

#### **4.2 Teacher behavior and motivation**

Finally, we investigate whether the two treatments may have contributed to differential teacher motivation and participant satisfaction. If teachers developed a preference for one program over the other, this could result in differences in motivation and teaching quality over time. To probe this hypothesis, we ask the endline-survey participants to retrospectively rate on a scale from 1 (poor) to 10 (excellent): (i) the overall quality of the training, (ii) their overall satisfaction with the training, (iii) the perceived motivation of the teacher, and, lastly, (iv) their own interest in attending another training session. We observe high average ratings of quality, satisfaction, and teacher motivation (see Table 4). We do not find evidence for differences in subjective evaluation of the two programs, while a similar experiment with introductory college physics courses shows even higher satisfaction with the lecturing approach (see Deslauriers et al., 2019). Finally, 87 percent of the respondents report being interested in attending another training, again, with no differential effect between the two treatments.

<Table 4 about here>

## **5 Results**

## 5.1 Main treatment effects at a six-month horizon

Table 5 reports the average treatment effects of financial education interventions “active learning” and “lecturing” on five domains of financial behavior plus a summary effect aggregating the information across these five domains into a single index. The five domains of financial behavior are each measured by an index (results on all items forming these indices are shown in Appendix A).

<Table 5 about here>

Among these domains of financial behavior, the overall effects are strongest for the *investment index* (column 4), with effect sizes being relatively large (a quarter of a standard deviation). Active learning results in a statistically significant change in behavior. Lecturing is estimated to have a statistically insignificant effect size of about 0.11 standard deviation units.

Similarly, the effect on the *savings behavior index* (column 2) is sizeable for the active learning treatment (about 0.15 standard deviation units) and significant at the 5-percent level. Again, the lecturing treatment is statistically and economically insignificant (effect size of 0.02). In this context, we note that we do not observe significant increases of income over the six months observation period (see [Table A8](#)). However, the point estimate for consumption in the active learning treatment is negative and amounts to an estimated reduction of about 129,000 UGX over a 6-month period. This magnitude, while imprecisely estimated, may account for the observed increase in total savings (of about 110,000 UGX) over the same horizon. Thus, increased savings are not an indirect consequence of higher income but likely due to a change in (consumption and spending) behaviors.

In line with low expectations from the literature on the effectiveness of financial education to change *borrowing behavior* (Fernandes et al., 2014; Miller et al., 2015; Kaiser and Menkhoff, 2017; Kaiser et al., 2020), we do not find any significant effect for both treatments (column 3). Regarding training effects on the *budgeting index* (column 1), the effect size

estimates are positive but small and statistically not significant. The coefficient levels are higher for the *financial services index* (column 5); this is the only case where the coefficient of the lecturing treatment is higher than that of the active learning treatment, but both coefficients are estimated with a large standard error and, thus, remain statistically not significantly different from each other.

Finally, we aggregate information on all outcome domains into a *summary index* (column 6) that is defined as an equally weighted z-score index of the five financial behavior indices, informing about the overall impact of both treatments. The effect size of lecturing is about 0.06 standard deviation units and, thus, of plausible size given that the training lasts only two hours relative to the average treatment effect of 0.1 standard deviation units at an average intensity of 1 to 2 days in a meta-analysis of 76 RCTs (Kaiser et al., 2020). The effect size of active learning is about 0.19 SD. Thus, the overall treatment effect of active learning is significantly different from zero.

To summarize, we see that the active learning treatment results in a significant effect on two out of five financial behaviors addressed in the training. As lecturing is, on the other hand, associated with smaller estimated effects and larger standard errors, it does not have a statistically significant (at the 5%-level) effect on any of the addressed financial behaviors

## **5.2 Treatment effects at a four-year horizon**

The endline survey took place almost four years (exactly three years and nine months) after the baseline and was conducted by telephone. Thus, the survey is restricted to a minimum and focuses on variables where the midline indicates changes of interest, i.e., savings (two variables), investment, late payment on loans, and the use of mobile money. Accordingly, Table 6 reports results on these five variables. Panel A provides estimates for the outcomes at midline, Panel B shows outcomes at the endline survey.

<Table 6 about here>

**Overview.** The outcome pattern after three years is similar to the short-term results. The coefficients are mostly positive, i.e., in line with the treatment goals, except for zero effects on “any savings” (at a high level of 92 percent for the control group); moreover, we find adverse effects, as the higher late payment rates observed in both groups are not intended by the trainings. The effect sizes are generally larger for the active learning treatment than for the lecturing treatment. However, due to the reduced sample size, the estimates come with substantial uncertainty. Overall, these results suggest that both treatments may cause a longer-lasting effect that is, however, in our sample, only stronger and marginally significant for the active learning treatment.

**Savings and investment.** While the overall pattern of results mirrors the short-term findings, the magnitudes of estimated treatment effects on savings and investment appear inflated. However, the result looks more moderate when compared to the levels of the control group as this group also states quite high savings and investment levels relative to the baseline. The high absolute savings amounts in the control group at endline relative to baseline survey (see Table 5) confirms that the respondents to the endline survey are not a random sample of the baseline. At midline, the average total savings in the control group is about 510,000 UGX. The respective magnitude for the endline survey is 840,000 UGX for the control group. Thus, it appears that those who are relatively wealthy have been reached by the telephone survey in general (see also Table 2). For these individuals the relative increase at the mean due to the active learning treatment over the control group is about 54 percent for total savings (statistically insignificant) and about 62 percent for total investments (statistically significant at the 10-percent level) compared to 20 to 35 percent at the short-term horizon. Such an effect suggests that part of the treatment group has persistently changed behavior such that their savings and investments are not just stimulated for a short period of time.

**Late payments and mobile money.** Another interesting result of the endline survey is the increase in late payments on loans for both treatment groups. As with savings and investment, the level of the control group, at about 50 percent of individuals reporting late payments at least once, is much higher than at the midline survey, when around 27 percent of the control group report late payments (see Table 6). Lecturing increases the share of late payments by 13 percentage points and active learning by 16 percentage points, both increases being not only economically meaningful but also statistically significant. One element of this pattern, which can be analyzed at midline, seems to be that neither group increases the number of loans or total debt volume significantly (see [Table A14](#)); thus treated individuals may pay late regarding existing loan volumes rather than additional loans they aren't able to service. We also do not observe treatment effects on the total amount repaid at midline indicating that late payments could be temporary and may be resolved later.

Still, this result of increasing late payments is worrying. Reasons for this may be additional (self-induced) liquidity constraints at the cost of lenders, overconfidence of some retailers regarding immediate returns of their business investments or it could be possible that they even calculate with strategic defaults. While we cannot test these hypotheses, there is no correlation of late payments with the *total amount* of savings or investments at midline or endline, indicating that these may be two separate effects rather than the increases in investments being driven by loans which lead to late payments (see [Table A15](#)). Regarding the use of mobile money, we see no effects at the midline or endline survey.

**Interpretation.** It does not seem self-evident that a brief treatment has effects that can be measured almost four years after the intervention. We note that there may be four reasons for this result: (i) The target group has a high demand for financial education; for example, only about 40 percent had a correct understanding of interest rates. (ii) The target group has ability to improve their financial behaviors; for example, 85 percent of participants report savings at

the baseline. (iii) The results may be viewed considering selection of individuals into the study and survey rounds; at baseline, participants decide whether to spend (paid) time with researchers, while participants at endline have elevated socio-economic characteristics. These reasons (i) to (iii) may dampen external validity of this RCT but not its internal validity. While we do not fully understand why the active learning treatment may have led to persistent effects (see also Horn et al., 2020), (iv) we see that it stimulates self-control and financial confidence and changes in these non-cognitive factors (measured after six months) may translate into lasting effects; a mechanism consistent with our point estimates at midline is that treated households may increase savings by reducing their consumption.

### **5.3 Robustness**

We probe the robustness of results in comprehensive exercises. These show that results of the main paper are robust, but results must be viewed in light of limited power and high attrition at endline. While details are provided in [Appendix B](#), these checks are briefly reported here, covering three main areas: (i) As the sample is characterized by attrition, we address this issue by showing bounds analysis for the midline survey with midline results are generally robust to several scenarios. (ii) Showing unweighted regression results and results with additional covariate adjustments, we find that results are largely insensitive to these modifications. (iii) Finally, we address multiple hypothesis testing by providing adjusted p-values based on false discovery rate and family wise error rate corrections. These corrections generally lead to deflated p-values and, thus, higher uncertainty about the estimated effects mirroring the limited power of the cluster-RCT.

## **6 Conclusion**

Our research contributes to revealing the determinants of effective financial education interventions. As such a potentially crucial determinant, we compare an active learning approach with traditional lecturing. We explore possible process outcomes by which financial education may translate into changes in financial behavior. For this purpose, we examine the role of cognitive and non-cognitive process outcomes, i.e., financial literacy, self-control, and financial confidence. We find that while both treatments have no effect on financial literacy, active learning positively affects self-control and confidence, while lecturing only impacts financial confidence.

We then study five outcome groups of financial behavior, finding that active learning mostly leads to higher intended effect sizes than traditional lecturing. In two out of five outcome indices – savings and investment – active learning has an effect on financial behavior, while traditional lecturing has much smaller or zero effects. The economic effects at the midline tend to persist at endline survey for a limited subset of variables and respondents but are estimated with higher uncertainty. Moreover, we find a potentially adverse effect of financial education in the borrowing domain: late payments on loans appear to increase in both treated groups (see also Bruhn et al., 2016). Our design and data do not allow to fully understand if the late payments are real, and, if so, whether these indicate heterogeneous treatment effects or whether these effects coincide with potentially beneficial effects for the same individuals. Thus, welfare effects of the financial education programs remain ambiguous with current knowledge.

Overall, active learning methods seem to be a way to improve existing classroom programs without increasing the costs that result from other approaches, like lowering the student to teacher ratio or moving to strictly personalized interventions such as counseling. An important area for future research would be to study the effects of active learning for a broader set of target groups and circumstances.

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- Control group
- Treatment A: Active learning
- Treatment B: Traditional lecturing

Wave	Control group	Active learning	Lecture	Total	Attrition (% of baseline)
Baseline	456	414	421	1,291	
Midline	417	384	361	1,162	129 (9.99%)
Endline	239	184	197	620	671 (51.98%)

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**Table 2: Randomization-balance at baseline**

	Full baseline sample (n=1,291)				Endline sample (n=620)			
	Control	Active learning	Lecture	Equality of means (p-val.)	Control	Active learning	Lecture	Equality of means (p-val.)
	Mean (SD)	Mean (SD)	Mean (SD)		Mean (SD)	Mean (SD)	Mean (SD)	
Household size	6.86 (3.52)	7.00 (4.25)	6.64 (3.52)	0.35	7.16 (3.66)	7.08 (4.53)	6.67 (3.54)	0.50
No. of contributors	2.12 (2.01)	2.06 (1.95)	2.19 (1.98)	0.56	2.08 (1.94)	1.92 (1.64)	2.11 (2.00)	0.97
No. of children	4.29 (2.88)	4.29 (3.23)	3.94 (2.75)	0.40	4.50 (3.06)	4.55 (3.44)	3.86 (2.55)	0.12
Monthly consumption <sup>+</sup>	592,748 (404,893)	617,150 (450,766)	570,268 (372,067)	0.86	625,136 (442,031)	734,024 (559,547)	585,047 (411,224)	
Monthly income <sup>+</sup>	222,401 (337,539)	203,232 (283,402)	233,565 (355,164)	0.31	254,309 (367,257)	230,246 (311,366)	223,073 (306,436)	0.66
Female	0.80	0.79	0.80	0.98	0.78	0.73	0.78	0.92
Age	37.72 (12.36)	35.38 (11.53)	35.46 (11.60)	0.14	37.90 (11.66)	35.43 (10.70)	37.22 (12.12)	0.57
Education (>primary)	0.28	0.22	0.26	0.56	0.32	0.28	0.28	0.43
Self-control (z)	0.00 (1.00)	0.08 (1.02)	-0.04 (0.99)	0.66	0.10 (1.00)	0.09 (0.99)	-0.01 (1.01)	0.38
Fin. knowledge (z)	0.00 (1.00)	0.11 (0.99)	-0.02 (0.98)	0.86	0.07 (1.00)	0.35 (0.99)	-0.06 (0.92)	0.37
Fin. confidence (z)	0.00 (1.00)	-0.04 (0.97)	-0.13 (0.98)	0.19	0.06 (0.90)	0.07 (0.88)	-0.12 (0.99)	0.38
Cluster size	17.16 (3.90)	16.70 (4.36)	16.36 (4.35)	0.49	16.80 (3.60)	17.52 (4.18)	16.23 (4.31)	0.65
(1) Budgeting index	0.00 (1.00)	-0.10 (0.91)	0.04 (0.97)	0.68	-0.01 (1.03)	0.02 (0.97)	0.05 (0.97)	0.57
(2) Savings index	0.00 (1.00)	0.10 (1.12)	0.11 (1.01)	0.25	0.09 (0.98)	0.24 (1.24)	0.16 (1.05)	0.58
(3) Borrowing index	0.00 (1.00)	0.03 (1.24)	0.11 (1.39)	0.25	0.03 (1.01)	0.07 (1.34)	0.04 (1.35)	0.89
(4) Investment index	0.00 (1.00)	-0.04 (1.04)	0.04 (1.05)	0.72	0.16 (1.11)	0.17 (1.17)	0.10 (1.07)	0.71
(5) Fin. service index	0.00 (1.00)	-0.03 (0.99)	0.10 (1.07)	0.35	0.14 (1.06)	0.18 (1.05)	0.13 (1.03)	0.95
(6) Summary index	0.00 (1.00)	-0.01 (1.10)	0.14 (1.18)	0.24	0.14 (1.04)	0.24 (1.23)	0.17 (1.13)	0.84
Joint orthogonality (p-val.)	0.15				0.68			

Notes: <sup>+</sup> indicates that the currency denominated outcome (in Ugandan Shilling (UGX)) is winsorized at the 99<sup>th</sup> percentile. The F-test of joint orthogonality regresses a categorical variable indicating the three groups on the full set of variables. Standard errors are clustered at the market-level. Tests are unadjusted for multiple hypothesis testing.

**Table 3: Results on process outcomes at midline**

	(1) Fin. literacy (z)	(2) Self-control (z)	(3) Fin. confidence (z)
Active learning	0.090 (0.077)	0.143* (0.079)	0.147* (0.084)
Lecture	0.064 (0.073)	-0.055 (0.091)	0.169** (0.086)
$A - B = 0$ (p-value)	0.761	0.029**	0.800
Mean (SD) of $y_t$ in control	0.000 (1.000)	0.000 (1.000)	0.000 (1.000)
Observations	1,162	1,162	1,162
Clusters	83	83	83

Notes: Table shows WLS regression results weighted by the inverse probability of selection into the midline sample. All models include the lagged outcome ( $y_{t-1}$ ) 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: Ratings of training and teacher quality at endline**

	(1) Overall training quality	(2) Overall satisfaction	(3) Teacher motivation	(4) Interested in another training
Active learning	0.057 (0.212)	0.233 (0.187)	0.122 (0.225)	-0.002 (0.036)
Mean (SD) of $y_t$ in Lecture group	6.982 (1.518)	7.089 (1.634)	7.006 (1.734)	0.867
Observations	381	381	381	381
Clusters	55	55	55	55

Notes: Table shows WLS regression results weighted by the inverse probability of selection into the endline sample. All models include 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 5: Treatment effects on indices at midline**

	(1) Budgeting index (z)	(2) Savings index (z)	(3) Borrowing index (z)	(4) Investment index (z)	(5) Fin. services index (z)	(6) Summary index (z)
Active learning	0.052 (0.089)	0.151** (0.073)	-0.021 (0.066)	0.261*** (0.089)	0.098 (0.082)	0.194** (0.082)
Lecture	0.009 (0.087)	0.017 (0.081)	-0.079 (0.078)	0.112 (0.090)	0.164* (0.097)	0.059 (0.078)
$A - B = 0$ (p)	0.641	0.139	0.479	0.121	0.507	0.117
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	1,162	1,162	1,162	1,162	1,162	1,162
Clusters	83	83	83	83	83	83

Notes: The dependent variables ( $y_t$ ) are equally weighted z-score indices of financial behaviors and are standardized to have a zero mean and a standard deviation of one for the control group. All currency denominated outcomes (in Ugandan Shilling (UGX)) within the indices in columns (2), (3) and (4) are winsorized at the 99<sup>th</sup> percentile. Column (6) shows results for an equally weighted z-score index of all five financial behavior indices. All models include the lagged outcome at baseline and district-level fixed effects. Regressions are weighted by the inverse of the estimated probability of selection into the midline survey. Standard errors (clustered at the market-level) are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table 6: Mid- and endline results on selected index components**

	(1) Any savings	(2) Total savings <sup>+</sup>	(3) Total investments <sup>+</sup>	(4) Ever used mobile money	(5) Ever late pay- ment on loan
Panel A: Short-term treatment effects (after six months) (n=1,162)					
Active learning	0.037 (0.026)	106,137* (62,805)	94,791*** (34,137)	0.053 (0.038)	0.079** (0.035)
Lecture	0.043 (0.029)	-23,774 (60,615)	50,298 (43,359)	0.048 (0.042)	-0.009 (0.035)
$A - B = 0$ (p)	0.812	0.066*	0.324	0.913	0.015**
Mean (SD) of $y_t$ control	0.852	510,357 (926,964)	266,940 (503,136)	0.439	0.266
Observations	1,162	1,162	1,162	1,162	1,162
Clusters	83	83	83	83	83
Panel B: Long-term treatment effects (after 3.5 years) (n=620)					
Active learning	0.003 (0.029)	448,510 (294,101)	453,329* (247,896)	0.111 (0.067)	0.160** (0.069)
Lecture	0.025 (0.027)	243,740 (228,128)	64,294 (187,132)	0.035 (0.073)	0.130* (0.076)
$A - B = 0$ (p)	0.481	0.560	0.180	0.279	0.697
Mean (SD) of $y_{t+1}$ control	0.921	839,431 (1356,715)	747,040 (1170,581)	0.541	0.498
Observations	620	620	620	620	620
Clusters	83	83	83	83	83

Notes: Columns (1), (4) and (5) are linear probability models. All models include the lagged outcome ( $y_{t-1}$ ) at baseline as well as district-level fixed effects. <sup>+</sup> indicates that the outcome (in Ugandan Shilling (UGX)) is winsorized at the 99<sup>th</sup> percentile. Means and SD in control are weighted by the inverse of the estimated probability of selection into survey round. Regression estimates are weighted by the inverse of the estimated probability of selection into the endline survey. Standard errors (clustered at the market-level) are reported in parentheses. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .