Five-year impacts of group-based financial education and savings promotion for Ugandan youth[†]

Samantha Horn, Julian C. Jamison, Dean Karlan, Jonathan Zinman[*]

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Abstract

We experimentally evaluate group-based financial education, savings account access, or both for members of Ugandan youth groups. We measure both short- and long-run impacts with one- and five- year endline household surveys. Education, but not account access, increases measured financial knowledge and trust at one-year. At five-years, knowledge effects essentially disappear, and trust effects weaken. However, savings and income increase for each treatment at both endlines, which is noteworthy given the interventions' low cost and the long time horizon of our second endline. Exploring potential mechanisms, we find evidence consistent with multiple pathways to behavior change and outcome improvement.

JEL Codes: D12, D91, O12

Keywords: financial education; financial literacy; financial access; savings

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[†] This paper supersedes two papers: (1) NBER Working Paper No. 20135, "Financial Education and Access to Savings Accounts: Complements or Substitutes? Evidence from Ugandan Youth Clubs" reported on the same experiment but with only one-year results and a different focus, and (2) NBER Working Paper No. 28011, "Does Lasting Behavior Change Require Knowledge Change? Evidence From Savings Interventions for Young Adults" reported on the five-year results but with more of a focus on the role of knowledge change. This experiment was registered in the American Economic Association Registry for randomized controlled trials (AEARCTR-0000080 – www.socialscienceregistry.org/trials/80). Institutional Review Board approval for human subjects protocols from Innovations for Poverty Action #113.10February-006 and Yale University #1002006384. As suggested by Asiedu et al. (2021), our online Structured Ethics Appendix discusses ethics questions beyond those covered by IRB. We thank the Financial Education Fund from DFID and Citi Foundation for funding. We thank the IPA field team, Zach Freitas-Groff, Sarah Kabay, Daniel Katz, Sana Khan, Charity Komujurizi, Matthew Lowes, Justin Loiseau, Javier Madrazo, Joseph Ndumia, Doug Parkerson, Pia Raffler, Elana Safran, Noor Sethi, Marla Spivack, Glynis Startz, and Sneha Stephen from Northwestern University and Innovations for Poverty Action for research assistance and management support. We thank the Freedom from Hunger and Straight Talk team for collaboration on development of the financial education curriculum, FINCA for the provision of the bank accounts, and four dioceses of the Church of Uganda for their cooperation throughout.

[*] Horn: Carnegie Mellon University (email: samihorn@cmu.edu); Jamison: University of Exeter (j.jamison@exeter.ac.uk); Karlan: Kellogg School of Management, Northwestern University, (karlan@northwestern.edu); Zinman: Dartmouth College (jzinman@dartmouth.edu).

Financial inclusion remains an important development goal, with most of the world's population lacking basic financial literacy and bank account access. Two prevalent financial inclusion interventions are financial education and basic savings account promotion. Financial education presumes the importance of building financial knowledge for navigating previously unfamiliar and increasingly complex formal markets. Basic savings account interventions presume the importance of facilitating formal market access. For both, short-run policy interventions naturally aim for long-run improvements in households' financial conditions, but less is known on the persistence of such interventions.

We use a four-arm randomized evaluation alongside extensive primary data collection one-year and five-years after intervention onset to measure impacts of financial education and basic savings account promotion. Specifically, we randomly assigned 240 Church of Uganda youth clubs to four arms: a financial education ("education-only"), group access to a bank savings account ("account-only"), both ("account+education"), or neither.

Group-based financial education delivery is common through schools, workplaces, and NGOs. Group-based savings mechanisms are also common, both traditionally through informal institutions as well as via microfinance and other formal institutions. In 2018, for example, CARE launched a plan to scale-up informal savings groups to reach over 65 million individuals across 50 countries. Religious clubs feature prominently in Uganda and neighboring countries, with 50% or more of young adults belonging to one. Our interventions and sample are thus broadly interesting for researchers and policymakers working on financial inclusion.

Our baseline survey of 2,810 club members reveals low levels of textbook financial knowledge and formal financial bank account usage, and moderate income levels. The account intervention offered groups easy access to a basic group savings account with a local affiliate of an international microfinance institution. The financial education intervention was a 10-week, 15-hour curriculum, designed by three international and local NGOs, focusing on the formal financial system, savings costs and benefits, budgeting and planning, and communicating with others about money.

We administered two follow-up surveys to measure textbook financial knowledge and other decision inputs, savings, income, and other pre-registered "downstream" behaviors and outcomes. These surveys occur roughly one-year (N=2,680) and five-years (N=1,969) after random assignment, with little evidence of differential attrition rates.

We find substantial take-up and utilization of both interventions; e.g., club members attended about half of ten education sessions and about half of clubs actively used their savings accounts. The high engagement[1] is likely due to piggybacking on pre-existing group meetings (alternatively, but we posit unlikely, the high engagement could be due to increased group-based economic activity). The first-stage results provide sufficient statistical power for identifying moderately-sized treatment effects on decision inputs, behaviors, and downstream outcomes.

We focus on decision inputs covered by the financial education curriculum. After one year, each education arm produces large increases in financial knowledge and trust in banks (0.16 to 0.32 SD increases, SEs ~0.06). In contrast, the account-only arm shifts neither financial knowledge, planning, agency (control over household resources), nor trust in banks (e.g., the treatment effect on a financial knowledge index is 0.01 SD, SE=0.06). After five years, the education impacts dissipate: the four point estimates are all substantially lower than their one-year counterparts, with the knowledge point estimates near zero albeit imprecisely estimated. Within-treatment arm tests that the one-year and five-year treatment effects are equal yield *p*-values from 0.03 to 0.26.

Next, we estimate average and quantile treatment effects on several measures of saving behavior and assets.[2] The point estimates suggest each treatment substantially and persistently increases savings activities, though the confidence intervals often include small effect sizes as well. There are hints that the education arms might produce larger increases in savings than the account-only arm; increasing financial knowledge is likely valuable. But we cannot rule out equal effects from, or economically large (30% or more) savings balance increases in, the account-only arm. We also estimate treatment effects on borrowing and, finding none, infer increases in wealth, although our null effects on debt are imprecisely estimated.

[1] Our savings account take-up rate is comparable to other studies in Sub-Saharan Africa, but with substantially higher utilization (see, e.g., Dupas et al (2018)). Around 40% of members make transactions, suggesting the club utilization rate is not hiding low usage for the average participant. For financial education, we are not aware of any systematic review of take-up or engagement rates but several papers find low participation rates (Lara Ibarra, McKenzie, and Ruiz-Ortega forthcoming; Burke et al. 2020; Bruhn, Lara Ibarra, and McKenzie 2014).

[2] We define savings to include liquid financial and durable assets, both formal and informal. We do not measure many illiquid fixed assets, as such assets are not likely important stores of value for youth.

We then estimate average and quantile treatment effects on income, motivated by the mixed evidence from prior work on the downstream effects of savings interventions. We find large, positive, and persistent average effects on total income in each of the three treatments, though the confidence intervals also include modest increases.

We examine several mechanisms to explain our results, and ultimately do not draw a strong conclusion about any one path. Which we assert likely reflects the reality that there is no one path dominant enough to "win" in an empirical sense.

Knowledge does increase in the short-run, and then dissipates in the long-run (whereas behavior change persists). Although not all aspects of those results are precisely estimated, this pattern is consistent with knowledge increase (or knowledge increase and maintenance) not being a necessary path to behavior change. Much akin to physics knowledge for Friedman's billiard player (Friedman 1953): do agents behave "as if" they have learned some underlying principles, without demonstrating gains in "textbook knowledge" as measured by traditional tests of financial literacy? And, which interventions are effective at improving downstream outcomes like income and wealth, particularly over longer horizons?

Recent work attempts to disentangle mechanisms via mediation analysis. In particular, Carpena and Zia (2020) examines the role of attitudes towards personal finance, financial awareness, and numeracy in explaining the overall impact of financial education. They find support for the mediating roles of changes in awareness and attitudes, but no evidence that changes in numeracy mediate treatment effects. Rather than focus on components of financial knowledge, we examine mechanisms outside of knowledge, specifically: altruism, patience and self-control, and risk aversion; business activity and investment; non-business investments and spending patterns; and, formal labor market effort. Because of the multitude of viable pathways, we conduct this analysis by examining treatment effects on these mechanisms, rather than mediation analysis as done by Carpena and Zia (2020). We find suggestive evidence consistent with Schaner's (2018) entrepreneurship channel and Callen et al.'s (2019) labor effort channel. And although we cannot rule out that the account-"only" arm treatment provided something besides account access *per se*, its lack of treatment effects on any observable decision input—agency, attitudes/preferences, knowledge, or planning—is noteworthy.

Given many favorable conditions - relatively high intervention take-up rates, two follow-up surveys, large treatment effects on downstream outcomes, and a sample of about 2,000 - our inability to identify mechanisms is sobering. But our results remain enlightening in the sense that they are consistent with several of these mechanisms being important. Indeed, we collected data on many decision inputs and outputs because savings interventions are posited to work through multiple mechanisms.

Based on Kaiser et al.'s (forthcoming) meta-analysis of randomized evaluations of financial education interventions, we primarily contribute by helping to fill three gaps in that literature. First and most importantly, we extend impact measurement horizons with our five-year endline. Second, we provide in-sample evidence on relative effectiveness of and interaction between account access and education, and find similar treatment effects on savings activity and income with little evidence of complementarity and some evidence for substitutability. Three, we provide evidence on the effects of financial education on income generation and trust.[3]

We also build on a large literature on savings encouragement interventions.[4] First, we provide evidence on whether market experience alone produces measurable changes to decision inputs like financial knowledge or trust and find no evidence that it does. (This contrasts with Bachas et al. (2020) which finds that issuing debit cards increases trust, and Dupas et al. (2018) which finds mixed evidence from fee-free savings accounts.) Second, we extend impact measurement horizons to five years, although there are at least three other studies with three- or four-year measurement horizons for savings and income (Beaman, Karlan, and Thuysbaert 2014; Schaner 2018; Field et al. 2019). Third, we add to the mixed evidence on whether improving savings access leads to lasting increase in income. Previous work finds positive effects from direct deposit and commitment (Brune et al. 2016), temporary yield incentives (Schaner 2018), deposit collection (Callen et al. 2019), fee-removal and targeting female market vendors (Dupas and Robinson 2013a), and direct deposit from a public workfare program for women alongside training (Field et al. 2019). But several other studies have not found as robust a causal link (e.g., Aggarwal,

[3] See also Galiani et al.'s (2020) randomized evaluation of a three-hour training session designed specifically to build trust in financial institutions.

[4] Steinert et al. (2018) provides an excellent meta-analysis on savings promotion interventions. To better position our work in terms of outcome measurement and evaluation horizon, we detail our contribution with respect to the 46 papers described in Appendix Table 1.

Brailovskaya, and Robinson 2020; Banerjee et al. 2020; Bastian et al. 2018; Beaman, Karlan, and Thuysbaert 2014; Dupas et al. 2018; Prina 2015; Somville and Vandewalle 2019).

Three papers have similar 2x2 experimental designs but are unable to focus on the primary question we are posing, whether short-run results from financial education and/or savings account encouragement can generate impact at five years. Abarcar et al. (2020) implements a similar design in the Philippines for transnational households with relatively high baseline rates of financial inclusion, measuring outcomes one to four months after intervention end, but finds no change in financial literacy as a by-product of the financial education treatment alone (potentially because the training was relatively short[5]); it also has low take-up rates of its encouraged savings account (around 1%) and so limited power to detect any consequent effects. Abebe et al. (2018) uses savings reminders instead of a savings access treatment with Ethiopian micro-entrepreneurs with substantial financial access at baseline, but has limited power to detect downstream impacts, and also does not find improvement in financial literacy from the financial education-only treatment arm at a four month endline. Cole et al.'s (2011) seminal paper uses financial incentives to encourage account opening among unbanked Indonesian households and follows up with households two years after intervention, but is underpowered for detecting effects on savings and does not estimate effects on financial knowledge or downstream outcomes.

I. Research Design and Implementation

Appendix Figure 1 details sample sizes, treatment assignments, and survey timing.

A. Club Sampling and Baseline Survey

We created our sample by obtaining permission from The Church of Uganda to work with its youth clubs. Clubs typically have about 40 members and engage in activities including community service and continuing education. According to 2012 Afrobarometer data, 50% of Ugandans aged 18-25 belong to a religious community group.

We identified 267 clubs that satisfied three criteria: (1) Located within a 60-minute walk of public transportation to the district capital (thus reasonably accessible to a FINCA branch); (2) Active, defined as meeting at least twice a month (thus allowing the financial education to piggyback on already-attended meetings); (3) Large enough, defined as having at least 12 members

[5] The financial education treatment comprised a 1-day workshop lasting 6-8 hours.

over the age of 16 (to reach target sample size).[6] We randomly selected 240 of these 267 clubs to be in our study sample.

B. Club Member Sampling, Baseline Survey, and Randomization

We created a sample frame for surveying active club members by surveying club leaders to identify all members attending club meetings during both school terms and holidays. We then randomly selected 12 members and 4 alternates aged 16 and up from each club, for a baseline survey sample frame of 240*16= 3,840 members. Surveyors approached selected members at club meetings and administered the survey around the club's regular meeting. We completed 2,810 baseline surveys.

Surveyed club members average 24 (SD=7) years old, with 31% a household head, 43% female, and 38% currently attending school. Financial knowledge and trust are low; e.g., baseline survey respondents answer only two of five basic financial literacy questions correctly, and only 43% say that bank savings definitely would not be stolen. 37% of the sample owns a formal bank account, and only 29% of these owners report frequent use, so only about 11% of the sample is an active formal account user at baseline. About half the sample are classifiable as poor.

We randomly assigned clubs evenly to education-only, account-only, account+education, and control, stratifying on region and an indicator for above-median baseline savings. We find little evidence of imbalance across our four arms. Appendix Table 2 reports baseline statistics and randomization balance checks.

C. Financial Education Treatment

Innovations for Poverty Action (IPA) developed the financial education course in cooperation with the nonprofit organizations Freedom from Hunger and Straight Talk Foundation (STF). Developing and delivering the course cost about US$63 per person in 2020 dollars.[7]

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[6] Appendix Figure 2 provides a map showing study areas.

[7] Cost estimates are calculated for the study sample as (total cost of treatment)/(number of study participants). As the treatments were delivered to groups including additional members who were not part of the study sample, the estimates are conservatively high. Trainer and manager compensation and expenses account for about 80%.

Mean attendance is 4.6 sessions out of ten (SD: 3.9) with a median of five. 75% of attended at least one session, and mean attendance conditional on attending at least one meeting is 6.2.[8]

Our key takeaway from attendance data is that we have a reasonably powerful and symmetric first stage: substantial levels of course engagement, and similar treatment intensity across the education arms.

See Appendix: Treatment Intervention for more details on the financial education content and implementation.

D. Savings Account Treatment

The savings accounts were offered by FINCA, a microfinance institution. IPA and FINCA designed the account to be group-based in order to minimize costs (pecuniary and otherwise) while enabling FINCA to deliver basic account services. Group delivery of *formal* accounts was novel amongst Ugandan financial institutions, but group savings is familiar to the participants because of extensive promotion of *informal* group-based savings. A recent survey with a representative sample of 3,000 Ugandan adults (age 16 and over) found that informal savings groups were the most popular savings location, used by 43% (FSD Uganda 2018); and, 63% of the clubs in our sample had one or more members already participating in informal group savings.[9]

We estimate this intervention cost US$29 per person in 2020 dollars.[10]

FINCA data indicate 60% and 72% of clubs open accounts in the account-only and account+education arm, with 52% and 53% of clubs, having non-zero balances after one year.[11]

Our key takeaways from FINCA data are a reasonably powerful first stage that may have operated somewhat differently across the two account arms.

See Appendix: Treatment Intervention for more details on the group savings accounts.

[8] Appendix Table 3 reports session-level attendance statistics, Appendix Figure 3 illustrates participant perceptions of course content from focus group data.

[9] Prior work has examined the impact of group-based savings interventions (Giné and Karlan 2014; Dupas and Robinson 2013b; Beaman, Karlan, and Thuysbaert 2014; Karlan et al. 2017).

[10] This covers marketer and manager compensation and expenses and equals the subsidized portion of intervention cost under the assumption that FINCA makes weakly positive profits on the margin. As with the education intervention, cost estimates are per person in the study sample and thus highly conservative.

[11] See Appendix Table 3 for additional usage statistics.

E. Endline Surveys and Attrition

We administered one-year endline surveys 9-12 months after the last financial education sessions, and 7-10 months after the start of account marketing. We attempted to re-survey all baseline survey respondents and obtained 2,680 completed surveys (95% retention) at one-year, and 1,969 (70%) at five-years.

We find little evidence of differential attrition rates across study arms: the biggest pairwise difference in the retention rate, across the four arms and two endlines, is two percentage points. Regressing a survey completion indicator on the three treatment assignment indicators to formally test for differential rates yields p-values of 0.59 at one-year and 0.85 at five-years. We also explore changes in sample composition across study arms by testing whether the means of key baseline variables, which were balanced at baseline, remain balanced at endlines. Univariate tests indicate weak evidence of compositional changes, and multivariate tests reject orthogonality with p-values of 0.02 and 0.03.[12] We control for each outcome's baseline value when estimating treatment effects.

II. Treatment Effects and Mechanisms

A. Estimation Strategy and Table Organization

We estimate average impacts, using OLS models of the form:

$$(1)\ Y_{ijt} = \beta_{1t}EdAcct_j + \beta_{2t}EdOnly_j + \beta_{3t}AcctOnly_j + \phi Y_{ij0} + \gamma StratVars_j + \varepsilon_{ijt}$$

where Y_{ijt} is an outcome variable, for member i of club j in time period t (either the one-year or five-year endline) or 0 (baseline).[13] The treatment arm variables indicate if individual i was randomly assigned to that study arm, and all estimates are intent-to-treat (ITT). We cluster standard

[12] Appendix Table 4 has the analysis on attrition. Rejection seems driven mostly by financial knowledge (mean of 0.06 in control; -0.11 in account-only, -0.04 in education only and 0.03 in account+education). The pattern across treatment arms is not consistent with other variables that ought to be correlated with financial knowledge, such as education and financial planning. Thus we do not infer there to be strong evidence of differential attrition. Further, in Appendix Tables 18, we present IPW-adjusted treatment effects and Lee Bounds for financial knowledge, savings, and income. Our results are broadly robust to these adjustments.

[13] We pre-specified primary outcomes of interest in the AEA Registry (AEARCTR-0000080) prior to the five-year endline. Pre-registration was not yet the norm at the time of the one-year endline.

errors at the youth club level. *StratVars*$_j$ are the stratification variables described in Section I-B. Our quantile regressions take the same form, replacing Y_{ijt} with one of its deciles.

Each table covers an "outcome class": decision inputs, saving, income, and other mechanisms. We adjust for multiple hypothesis testing by reporting a false discovery rate (FDR) adjusted p-value for each ITT estimate, defining a family as either the full set of components in each table-endline or the summary measure in each table-endline.[14] One-year endline estimates are always in Panel A and five-year in Panel B. Each panel-column in Panels A and B presents results from a single regression. At the bottom of each of these panels we report p-values for tests of equality across treatment arms and for complementarity. Panel C reports p-values on the difference between the one- versus five-year effects, for each treatment arm.

B. Key Decision Inputs

Table 1 presents estimates of treatment effects on four decision inputs covered in the financial education curriculum: knowledge, planning, agency, and trust. These also could be affected by market experience (induced by, e.g., account access). Each outcome measure is a standardized index of several related measures of one of the four inputs.[15]

The financial knowledge index in Column 1 is a standardized score of 20 questions regarding bank regulation and basic financial concepts like budgeting, interest, and collateral. The questions include tests of different definitions (e.g., "What is the word for the extra money that you have to pay if you borrow money from a bank?") as well as tests of financial numeracy (e.g., calculating interest rates). Financial knowledge is a multi-faceted concept that is measured differently across studies (Carpena et al. 2011; Carpena and Zia 2020). Our measure is designed to assess the textbook knowledge covered in the financial education curriculum and so may miss some important components of financial literacy, such as financial attitudes and awareness. The control group mean is 9.7 correctly answered (SD= 2.8) at one-year and 10.0 at five-years.[16] At one-year, the education arms each increase knowledge, by 0.16 and 0.17 SDs (SEs of 0.06, adjusted p-values 0.02 and 0.01), relative to either the control arm or account-only arm (the p-values on the

[14] We calculate adjusted p-values using the two-stage procedure in Benjamini, Krieger and Yekutieli (2006).
[15] Appendix Tables 5-8 report results separately for each index component.
[16] Appendix Figure 4 shows estimated financial knowledge levels for each arm at each survey. Focusing on the Control Group in Panel B, there is little evidence of strong secular increases over time.

differences between the account-only arm and each education arm are each <0.01). These one-year magnitudes are similar to the mean estimated effect of 0.20 SD of financial education on financial knowledge in Kaiser et al.'s (forthcoming) meta-analysis, where the median impact measurement horizon is about a half-year. Our clear one-year effects are no longer present at five years (the point estimates fall to 0.04 and -0.01 relative to control; 0.13 and 0.08 relative to account-only), with *p*-values on the within-arm difference between one- vs. five-year treatment effects of 0.26 and 0.03. We find no evidence that account-only affects knowledge, and the five-year confidence interval does not contain a substantial positive effect size.

The financial planning index averages across four component measures of tracking, routine and emergency planning, and plan implementation. At one-year, 64% of the control group report regularly keeping track of money, and 18% report regularly making any preparation for emergencies. There is little evidence of treatment effects on financial planning, although these nulls are imprecisely estimated.

The financial agency index averages across three component measures of financial household decision-making power. At one-year, 73% of the control group reports that others in their household would not be angry if the respondent saved alone, and 58% report always making their own financial decisions. There is little evidence of treatment effects on financial agency, although we cannot rule out substantial and persistent positive effects from account+education.

The financial trust index averages two questions about the security of bank deposits. At one-year, only 44% of the control group says that bank savings definitely would not be stolen, and only 43% that savings definitely would be repaid if the bank were robbed.[17] The education arms each increase trust at the one-year follow-up, by 0.22 and 0.32 SD (SEs 0.05, adjusted *p*-values < 0.01) relative to either the control group or the account-only group. Panel B shows smaller point estimates in year five--0.12 and 0.20 (SEs 0.06, adjusted *p*-values = 0.31 and 0.02, respectively)--but evidence for dissipation is only suggestive, with *p*-values of 0.13 and 0.14. The estimates for the account-only arm suggest no effect but are imprecisely estimated.

[17] Appendix Figure 3 shows estimated trust levels for each arm at each survey. Focusing on the Control Group in Panel C, there is little evidence of strong secular increases over time.

Altogether, the results suggest that financial education produces a large increase in knowledge even after one year but then dissipates by five years, and large and more lasting increases in trust in banks. We find no evidence that account access alone changes decision inputs.

C. Savings

Table 2 reports impacts on a standardized savings index (Column 1) comprising various pre-registered measures of assets and liabilities (Columns 2-7).[18] (Dis)savings is notoriously difficult to measure in surveys, as the asset and liability*institution space is large, respondents may vary in their interpretation of certain assets, liabilities, and institution types, flows require recall, stocks require valuation. Moreover, low-frequency surveys can miss important dynamics of accumulation and decumulation. We piloted extensively to create questions that, *taken together*, would proxy for overall savings behaviors and wealth accumulation. As such, we view the index as the most informative savings outcome rather than any one measure.

Thus, starting with the savings index (Column 1), each of the six point estimates across the two follow-ups are positive. Three have p-values <0.01, and two <0.10. We do not reject equality of treatment effects within-arm across the two follow-ups (Panel C). And, although the point estimates on account-only are weakly lower than those for the education arms, we do not reject equality across treatment arms (the p-values for the pairwise comparisons between account-only and the other arms are 0.17, 0.32, 0.34, and 0.72). The six point estimates each imply at least a 0.10 SD increase; for comparison, Kaiser et al.'s (2020) meta-analytic estimate of the effect of financial education on savings is 0.10 SD.

We measure the index components by first asking respondents whether they save in each of 13 different savings "locations" ascertained through piloting to be the most likely stores of financial and key resellable durable assets (see Appendix Table 9) and then how much they currently hold in each. We take a similar approach to the liability side of the individual's balance sheet.

Total savings balances (Table 2 Column 4) is the sum of the monetary value across all savings locations. Baseline savings balances are extremely heterogeneous, with a 1% top-coded mean of 118,000 UGX and SD of 335,000 (Appendix Table 2). As such, we consider treatment effects on alternative functional forms in Appendix Table 10 Column 1-4, finding similar results: uniformly

[18] We also pre-registered savings goals as an outcome and consider goal-setting and planning in the planning index in Table 1.

positive point estimates, some evidence that these increases are statistically significant, little evidence that any effects dissipate over time, and inconsistent evidence on whether treatment effects differ across arms. We also present quantile regression results (Figure 1, top panels). Treatment effects are weakly positive throughout the distribution, for each arm at each follow-up time horizon, and more positive towards the top of the distribution, with the strongest results from account+education and the weakest from account-only. The estimated null effects at lower deciles are not all due to a large mass of non-savers, as only 14% reports zero savings.

We find no evidence of treatment effects on borrowing, suggesting that any increases in assets are increases in wealth. But we cannot rule out increases of 0.1 SD on the extensive margin of borrowing (Table 2 Column 7; note that only about 50% of our sample has any debt). Nor do we find evidence for treatment effects on instances of borrowing or total amount borrowed in the last six months (Appendix Table 11).

Lastly, we consider treatment effects on *how* people save, subject to caveats about measurement error in categorization discussed above. First, there are positive treatment effects on the number of different locations (Table 2 Column 3), of about 0.1 to 0.2 (SEs: 0.05-0.06) locations on a base of 1.3. Two related questions are: how much of the treatment effects on savings result from FINCA group account use? And where else do people save when induced to save more by our treatments, particularly in the Education Only arm? FINCA data show active use of the account (mean=3.87 (SE: 0.60) and 4.20 (SE: 0.72) transactions conditional on opening account for the account only and account+education arm, respectively). This is reflected in our follow-up survey, where the only evidence of treatment effects on specific savings locations are increases in group account usage for the two account arms at the one-year follow-up.[19] The FINCA data also show how much club members save (Appendix Table 3). Group-level balances average about 145,000 UGX around the time of our one-year survey. With 30 members (the median), this implies a treatment effect of about 4,800 UGX per member - an order of magnitude smaller than the survey-estimated treatment effects on total savings balances (Table 2 Column 4). Together with the lack of treatment effect persistence on group savings in the account arms (no evidence of effects at five years in Appendix Table 9), it seems likely that our treatments induced savings through a variety of means, with the location varying across people and thus difficult to pin down.

[19] ROSCA usage increases as well as formal group accounts; respondents may categorize the FINCA account as an informal.

Altogether, we infer that the interventions persistently increase savings activity.

D. Income

Table 3 reports impact estimates for various pre-registered measures of income. To elicit income, the surveys start by asking whether they have recently done any activities to earn money, before asking for details on each activity, including the amount earned in the past 90 days.

Total income (Column 1) shows the sum of the sources in Columns 2-5.[20] Baseline earnings average about 110% of the individual poverty line, with substantial heterogeneity. Several patterns are evident. The point estimates are uniformly positive across all six arm-endline combinations and similar across arms within-endline. Three have p-values <0.05, and two <0.10. They each imply increases of about 15-20% over the control group mean, with confidence intervals including gains between 0% and 35%[21], and they are uniformly larger in levels at five-years than one-year.

Because total income is arguably our most important earnings measure, we estimate treatment effects on alternative functional forms, finding similar results (Appendix Table 10 Columns 5-8). We also present quantile regression results (Figure 1, bottom panels). As with savings, we see weakly positive effects throughout the distribution, for each arm and endline, although at five-years we see more evidence of effects from account-only.

Altogether, we infer that the interventions persistently increase income, with no strong evidence that effects differ across arms. If we take the treatment effect point estimates literally, they imply annual earnings increases of roughly 1 shilling per 1 shilling of account subsidy and per 2 shillings of education subsidy.

E. Mechanisms

The results presented thus far do not clearly identify mechanisms underlying the treatment effects, in part because we see increases in income and (to a suggestive but statistically weaker extent) savings in the account-only arm, which did not experience changes in the key decision inputs (Table 1).

[20] Total income also includes "other" income, which includes club-generating income (1% of total income).
[21] The control group trends considerably upward over the five years, we suspect from life-cycle patterns, inflation and other macro trends (e.g., about 25% real GDP growth over our study period).

We start by examining whether financial knowledge and trust (which improved in the short-run from the financial education treatment arms) are key mechanisms for the long-run change. Although some of the results are weak, our results suggest that increasing textbook financial knowledge and/or trust may be valuable but not necessary for producing lasting changes in saving and earning behavior. Three results provide the basis for this interpretation, although some of the statistical inference is weak: (1) The account-only treatment does not change measured knowledge or trust, but does increase savings and income with magnitudes similar to those in the financial education arms; (2) The financial education treatments increase measured knowledge and trust after one year, but those effects dissipate after five years, with knowledge effects falling to an imprecise zero; (3) Nevertheless, the financial education arms' effects on savings and earnings persist at five years. However, we emphasize that the dissipation of treatment effects on knowledge and trust is imprecisely estimated, and that we cannot rule out the possibility of positive and persistence treatment effects on some unmeasured component of financial knowledge.[22]

Hoping to uncover which mechanisms *are* influential in both short- and long-run, we estimate treatment effects on: altruism, patience and self-control, and risk aversion; business activity and investment; other investments and spending patterns; and, various measures of formal labor market effort. Table 4 Columns 6-8 consider changes to preferences and/or beliefs. We were motivated to pre-register these inputs by the possibility that the financial education curriculum's focus on saving, planning and agency could indirectly affect discounting (patience and self-control), risk tolerance, and altruism.[23] Yet we find no evidence of such treatment effects.[24] Account access alone could also change these inputs, by changing motivation via increased salience of savings or through a feedback loop with behavior. Alternatively, financial education and account access may have independently increased savings motivation through a salience channel, without a change in the outcomes considered in Tables 1 and 4, but we do not have a measure that can speak explicitly to changes in the underlying motivation to save.

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[22] We caution the reader to not interpret the trend in the control group too much because only five out of twenty questions were used for all surveys. However, the questions asked consistently we observe an upward trend in the control arm as well as a slight decrease in the treatment arm, thus weakening the interpretation that knowledge dissipated. We do not believe the upward trend in the control is due to information transmission from treatment, as the groups are fairly disparate geographically.

[23] Subsequently, several education evaluations have estimated effects on youths' preferences: e.g., Bover et al. (2018), Luhrmann et al (2018), Kim et al. (2018), and Alan and Ertac (2018).

[24] Appendix Tables 13-16 present treatment effect estimates for components. Appendix Table 17 reports estimates for aspects of financial knowledge and expectations not explicitly covered in the curriculum.

We do find suggestive evidence consistent with Schaner's (2018) entrepreneurship channel and Callen et al.'s (2019) labor effort channel. Table 4 Column 5 reports imprecise null effects on an index of expenditures and consumption (although our survey was not a full inventory of either).[25] This lack of cutback in spending, combined with the lack of an increase in borrowing (Table 2 Column 7 and Appendix Table 11), suggests the savings balance increase may have come from the increase in income à la Callen et al. (2019). We find no evidence that treated members change income source (Columns 1 and 2), and the confidence intervals rule out big changes. Increases in work effort - specifically, working more often - are a more likely candidate, in the sense that five of six point estimates in Column 3 are positive and the confidence intervals contain increases that would be sufficient to explain the treatment effects on income, but none individually is statistically significant. Another channel runs from saving to income, à la Schaner (2018): initial increases in saving might fund high-return investments that generate income before our first endline. Table 4 Column 4 (investment) and Table 3 Column 3 (business income) are consistent with this hypothesis in the sense that all point estimates are positive, albeit substantially smaller than those for total income.

III. Conclusion

Our results suggest that short-run interventions on financial education and encouragement to open a group-based savings account can generate impacts on savings and income that persist at five years. To explore mechanisms, we examine whether effects on financial knowledge and trust also persist at five years. The evidence is mixed: we find evidence that the effects dissipate, but the dissipation is noisily estimated.

Our results also suggest the interventions studied here are cost-effective, particularly when considering the persistent long-run impacts and the short-run and small costs of the program. The financial education costs about an order of magnitude less than many multi-faceted grant-based programs yet produce long-run impacts on wealth and income of similar magnitude (e.g., see Bandiera et al. 2017; Banerjee et al. 2015). Moreover, the tested interventions likely have economies of scale: we estimate the marginal cost per participant of the financial education intervention if delivered at scale at US$20 per person, compared to the estimated average cost per

[25] Appendix Table 12 reports results for each index component

participant of US$63 incurred for this study; for the account marketing intervention, we estimate a marginal cost of US$10 per participant if delivered at scale, compared to the estimated average cost per participant of US$29 incurred for this study. Appendix Table 19 provides more detail.

Although encouraging, we caution inferring too confidently from any one study. Further replication and refinement of intervention design, delivery, and evaluation would sharpen inferences regarding whether, how, and where such programs can generate the magnitude of effects found here. Additionally, many of the decisions inputs and mechanisms we consider are difficult to measure, especially in field settings. Although we try to mitigate these concerns by aggregating individual questions into standardized indices, it is very possible that our indices are not fully capturing the full scope of each concept.

Further research could focus on learning more about specific mechanisms, including mediation analysis, such as done in Carpena and Zia (2020) and Sayinzoga et al. (2016). Our interventions have multiple plausible paths to impact, and so even larger samples, higher-frequency data, and/or additional identification strategies may be required to identify which, if any, decision inputs or behaviors must change for downstream outcomes to improve.

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Table 1. Treatment Effects on Knowledge and Other Inputs Covered by the Financial Education Curriculum

	(1)	(2)	(3)	(4)
	Financial Knowledge Index	Financial Planning Index	Financial Agency Index	Financial Trust Index
Number of questions in index	20	4	3	2
Results for index components in	AT5	AT6	AT7	AT8
Panel A. One-Year Endline				
Account Access Only (T1)	0.01	0.03	-0.05	-0.01
	(0.06)	(0.06)	(0.06)	(0.06)
	[0.85]	[0.63]	[0.46]	[0.80]
Education Only (T2)	0.16***	0.09	0.01	0.22***
	(0.06)	(0.06)	(0.06)	(0.05)
	[0.02]	[0.23]	[0.80]	[<0.01]
Account + Education (T3)	0.17***	-0.06	0.10*	0.32***
	(0.06)	(0.06)	(0.06)	(0.05)
	[0.01]	[0.41]	[0.18]	[<0.01]
Control Group Mean	0.00	0.00	0.00	0.00
Control Group SD	1.00	1.00	1.00	1.00
N	2680	2680	2680	2680
p-values: T1 = T2	<0.01	0.40	0.25	<0.01
p-values: T1 = T3	<0.01	0.17	<0.01	<0.01
p-values: T2 = T3	0.83	0.03	0.12	0.07
p-values: T1 + T2 = T3	0.94	0.04	0.10	0.16
p-values: Any Account = 0	0.82	0.21	0.68	0.30
p-values: Any Education = 0	<0.01	0.98	0.05	<0.01
Controls for Baseline Values	Yes	Yes	Yes	Yes
Panel B: Five-Year Endline				
Account Access Only (T1)	-0.09	0.08	-0.03	0.06
	(0.07)	(0.06)	(0.07)	(0.07)
	[0.64]	[0.64]	[0.88]	[0.78]
Education Only (T2)	0.04	0.07	-0.11	0.12**
	(0.07)	(0.08)	(0.07)	(0.06)
	[0.88]	[0.72]	[0.54]	[0.31]
Account + Education (T3)	-0.01	0.02	0.08	0.20***
	(0.08)	(0.07)	(0.06)	(0.06)
	[0.88]	[0.88]	[0.64]	[0.02]
Control Group Mean	0.00	0.00	0.00	0.00
Control Group SD	1.00	1.00	1.00	1.00
N	1969	1969	1969	1969
p-values: T1 = T2	0.08	0.92	0.26	0.39
p-values: T1 = T3	0.34	0.32	0.10	0.05
p-values: T2 = T3	0.53	0.45	<0.01	0.19
p-values: T1 + T2 = T3	0.71	0.16	0.02	0.77
p-values: Any Account = 0	0.20	0.81	0.10	0.13
p-values: Any Education = 0	0.29	0.94	0.99	<0.01
Controls for Baseline Values	Yes	Yes	Yes	Yes
Panel C: Comparisons across One-Year and Five-Year Endlines				
p-values: T1 One-year = T1 Five-year	0.26	0.52	0.82	0.37
p-values: T2 One-year = T2 Five-year	0.11	0.88	0.11	0.13
p-values: T3 One-year = T3 Five-year	0.03	0.35	0.82	0.14
p-values: Any Account One-year = Any Account Five-year	0.19	0.25	0.30	0.57
p-values: Any Education One-year = Any Education Five-year	0.07	0.94	0.18	<0.01

Notes: Unit of observation is a club member-endline. Standard errors in parentheses, clustered at the unit of randomization (the youth club), and FDR adjusted p-values in square brackets with a family of hypotheses defined as all treatment effects for an endline survey (i.e. 12 hypotheses per endline survey). Each column-panel in Panels A and B reports results for a single OLS regression of the dependent variable listed in the column heading on the treatment variables listed in the row headings (control group is the omitted category), the baseline value of the dependent variable if available (with a dummy for missing baseline value where needed), and the stratification variables for randomization: an indicator for the club's members having above median total savings at baseline and region indicators. Item non-response rates are low and our indices average across non-missing components. The financial education curriculum covers one topic per meeting: (1) myths about the formal financial sector, (2) bank regulation by the Bank of Uganda, (3) how banks function as businesses, (4) the relative costs and benefits of saving versus borrowing, (5) targeted/goal-oriented saving, (6) budgeting and record keeping, (7) prioritizing spending decisions, (8) addressing challenges to saving, (9) making informed decisions about where and how to save, and (10) how to communicate about money. *** p<0.01, ** p<0.05, * p<0.10.

Table 2. Treatment Effects on Savings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Savings Index of Columns 2-7	Any Savings (1/0)	Total Number of Savings Locations	Total Savings ('000 UGX): 1% top-coded	Any Resellable Asset (1/0)	Formal Account (1/0)	No Debt (1/0)
Panel A. One-Year Endline							
Account Access Only (T1)	0.12*	0.01	0.09*	45.00	0.01	0.05**	0.04
	(0.07)	(0.02)	(0.05)	(37.33)	(0.02)	(0.02)	(0.03)
		[0.48]	[0.15]	[0.27]	[0.48]	[0.06]	[0.16]
Education Only (T2)	0.18***	0.02	0.15**	104.37**	0.00	0.05**	0.04
	(0.07)	(0.02)	(0.06)	(41.83)	(0.02)	(0.02)	(0.03)
		[0.27]	[0.06]	[0.06]	[0.50]	[0.07]	[0.16]
Account + Education (T3)	0.18***	0.04**	0.14**	44.30	0.00	0.09***	0.03
	(0.06)	(0.02)	(0.06)	(33.59)	(0.02)	(0.02)	(0.03)
		[0.07]	[0.06]	[0.23]	[0.50]	[<0.01]	[0.27]
Control Group Mean	0.00	0.84	1.28	221.94	0.12	0.16	0.48
Control Group SD	1.00	0.37	0.88	606.00	0.32	0.37	0.50
N	2680	2680	2680	2678	2680	2680	2680
p-values: T1 = T2	0.34	0.51	0.29	0.14	0.83	0.75	0.92
p-values: T1 = T3	0.32	0.12	0.36	0.98	0.84	0.14	0.72
p-values: T2 = T3	0.99	0.29	0.86	0.10	0.99	0.07	0.78
p-values: T1 + T2 = T3	0.18	0.71	0.20	0.05	0.79	0.75	0.20
p-values: Any Account = 0	0.18	0.37	0.30	0.77	0.77	<0.01	0.38
p-values: Any Education = 0	<0.01	0.06	0.01	0.05	0.98	0.01	0.46
Proportion of Obs Equal Zero	0.00	0.14	0.14	0.14	0.88	0.79	0.49
Controls for Baseline Values	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Five-Year Endline							
Account Access Only (T1)	0.10	0.02	0.15*	99.26	-0.00	-0.00	0.04
	(0.07)	(0.02)	(0.09)	(78.88)	(0.02)	(0.03)	(0.03)
		[0.71]	[0.58]	[0.58]	[0.71]	[0.71]	[0.58]
Education Only (T2)	0.12*	0.01	0.12	123.41	0.02	0.03	0.01
	(0.07)	(0.02)	(0.09)	(91.02)	(0.02)	(0.03)	(0.03)
		[0.71]	[0.58]	[0.58]	[0.71]	[0.58]	[0.71]
Account + Education (T3)	0.19***	0.02	0.18**	188.15**	0.03	0.04	0.04
	(0.07)	(0.02)	(0.08)	(84.08)	(0.02)	(0.03)	(0.03)
		[0.58]	[0.31]	[0.31]	[0.58]	[0.58]	[0.58]
Control Group Mean	0.00	0.86	1.60	552.14	0.13	0.23	0.51
Control Group SD	1.00	0.35	1.14	1202.70	0.33	0.42	0.50
N	1969	1969	1956	1960	1969	1956	1969
p-values: T1 = T2	0.72	0.83	0.77	0.79	0.38	0.19	0.32
p-values: T1 = T3	0.17	0.81	0.70	0.31	0.11	0.10	0.83
p-values: T2 = T3	0.30	0.62	0.49	0.50	0.47	0.72	0.42
p-values: T1 + T2 = T3	0.81	0.82	0.50	0.78	0.50	0.74	0.69
p-values: Any Account = 0	0.09	0.37	0.09	0.18	0.67	0.86	0.14
p-values: Any Education = 0	0.03	0.56	0.22	0.09	0.10	0.05	0.92
Proportion of Obs Equal Zero	0.00	0.13	0.13	0.13	0.87	0.75	0.47
Controls for Baseline Values	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel C: Comparisons across One-Year and Five-Year Endlines							
p-values: T1 One-year = T1 Five-year	0.75	0.74	0.54	0.49	0.70	0.05	0.99
p-values: T2 One-year = T2 Five-year	0.44	0.76	0.75	0.83	0.63	0.67	0.46
p-values: T3 One-year = T3 Five-year	0.88	0.55	0.62	0.07	0.25	0.13	0.93
p-values: Any Account One-year = Any Account Five-year	0.67	0.94	0.33	0.13	0.86	0.03	0.56
p-values: Any Education One-year = Any Education Five-year	0.82	0.37	0.74	0.37	0.15	0.95	0.66

Notes: Unit of observation is a club member-endline. Standard errors in parentheses, clustered at the unit of randomization (the youth club), and FDR adjusted p-values in square brackets with a family of hypotheses defined as all treatment effects for an endline survey (i.e. 18 hypotheses per endline survey, excluding the savings index). We do not adjust p-values for the savings index because the index itself reduces the number of hypotheses tested. Each column-panel in Panels A and B reports results for a single OLS regression of the dependent variable listed in the column heading on the treatment variables listed in the row headings (control group is the omitted category), the baseline value of the dependent variable if available (with a dummy for missing baseline value where needed), and the stratification variables for randomization: an indicator for the club's members having above median total savings at baseline and region indicators. Our survey asks about 13 different savings locations (please see Appendix Table 8 for details). Total savings here is top-coded at the 99th percentile; please see Appendix Table 10 for results on other functional forms of savings balances. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 3. Treatment Effects on Income

	(1)	(2)	(3)	(4)	(5)
	Earnings ('000 UGX) last 90 days, top-coded at 99th percentile				
	Total	Formal Wage	Business	Farm	Informal
Panel A. One-Year Endline					
Account Access Only (T1)	31.06*	-1.39	10.29	10.13	9.13
	(16.22)	(9.07)	(7.51)	(7.56)	(5.81)
		[0.78]	[0.57]	[0.57]	[0.57]
Education Only (T2)	32.45**	15.12*	2.76	5.62	9.11
	(16.44)	(8.80)	(7.56)	(6.50)	(6.40)
		[0.57]	[0.64]	[0.57]	[0.57]
Account + Education (T3)	36.34**	16.55*	7.25	4.07	2.96
	(17.01)	(9.48)	(7.59)	(6.42)	(5.76)
		[0.57]	[0.57]	[0.57]	[0.57]
Control Group Mean	200.79	70.07	38.51	42.93	29.90
Control Group SD	337.78	217.66	120.53	103.85	100.42
N	2661	2661	2661	2661	2661
p-values: T1 = T2	0.93	0.09	0.30	0.58	1.00
p-values: T1 = T3	0.76	0.08	0.68	0.45	0.21
p-values: T2 = T3	0.83	0.89	0.55	0.83	0.27
p-values: T1 + T2 = T3	0.26	0.84	0.59	0.26	0.06
p-values: Any Account = 0	0.15	1.00	0.16	0.41	0.71
p-values: Any Education = 0	0.12	0.02	0.98	0.97	0.71
Proportion of Obs Equal Zero	0.11	0.67	0.77	0.54	0.74
Controls for Baseline Values	Yes	Yes	Yes	Yes	Yes
Panel B: Five-Year Endline					
Account Access Only (T1)	75.47*	-22.25	6.46	37.21*	34.31**
	(43.46)	(22.76)	(16.89)	(20.15)	(14.69)
		[0.49]	[0.87]	[0.22]	[0.13]
Education Only (T2)	71.70	12.06	24.32	-1.25	23.19*
	(44.41)	(25.09)	(20.32)	(16.58)	(13.95)
		[0.87]	[0.37]	[0.97]	[0.24]
Account + Education (T3)	95.13**	8.95	33.35*	-0.34	44.42***
	(43.15)	(24.74)	(18.43)	(16.89)	(14.87)
		[0.87]	[0.22]	[0.97]	[0.04]
Control Group Mean	482.02	148.29	105.38	112.03	97.27
Control Group SD	673.52	400.81	282.07	273.56	217.91
N	1963	1963	1963	1963	1963
p-values: T1 = T2	0.94	0.11	0.38	0.07	0.47
p-values: T1 = T3	0.69	0.14	0.15	0.09	0.53
p-values: T2 = T3	0.64	0.89	0.68	0.96	0.17
p-values: T1 + T2 = T3	0.43	0.56	0.93	0.19	0.54
p-values: Any Account = 0	0.13	0.43	0.57	0.16	<0.01
p-values: Any Education = 0	0.17	0.19	0.07	0.16	0.12
Proportion of Obs Equal Zero	0.09	0.78	0.67	0.59	0.62
Controls for Baseline Values	Yes	Yes	Yes	Yes	Yes
Panel C: Comparisons across One-Year and Five-Year Endlines					
p-values: T1 One-year = T1 Five-year	0.28	0.33	0.82	0.15	0.10
p-values: T2 One-year = T2 Five-year	0.34	0.90	0.30	0.67	0.30
p-values: T3 One-year = T3 Five-year	0.13	0.74	0.14	0.78	<0.01
p-values: Any Account One-year = Any Account Five-year	0.29	0.41	0.98	0.25	0.01
p-values: Any Education One-year = Any Education Five-year	0.38	0.74	0.06	0.14	0.15

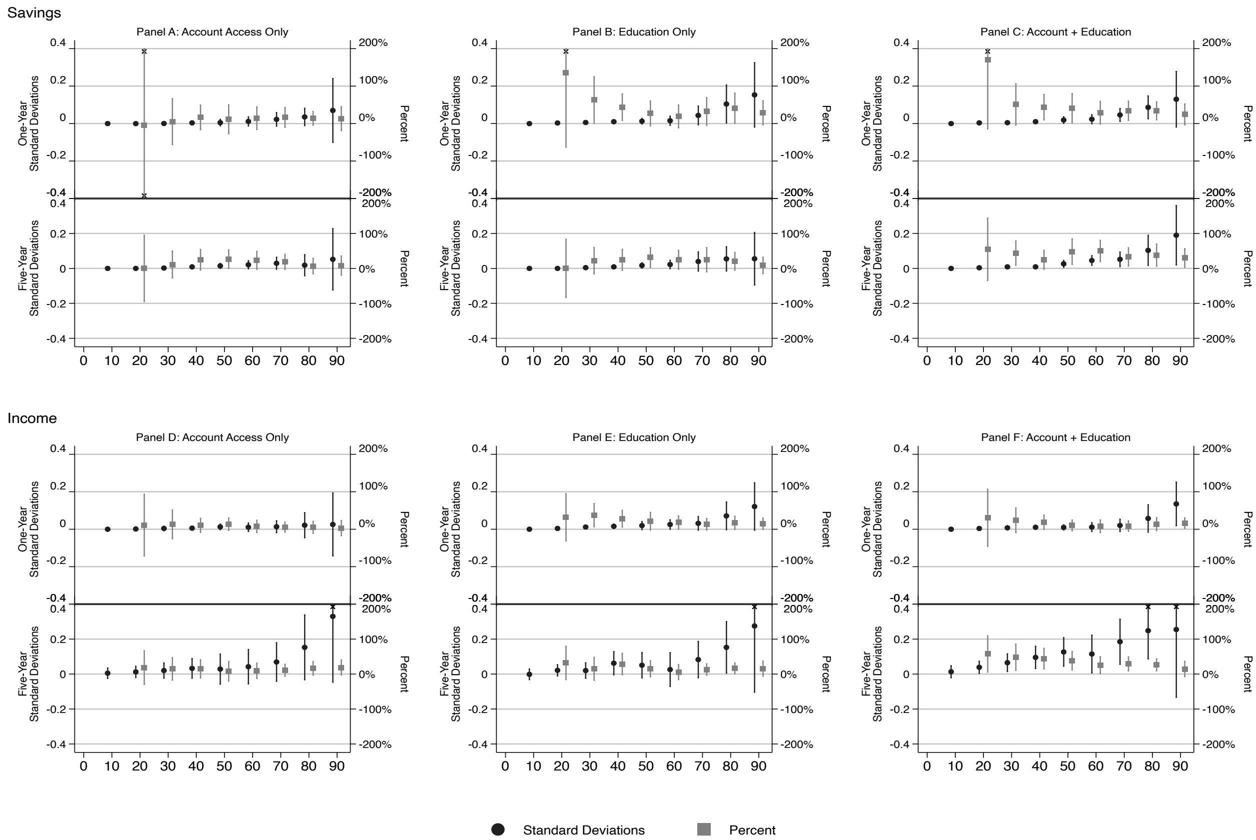
Notes: Unit of observation is a club member-endline. Standard errors in parentheses, clustered at the unit of randomization (the youth club), and FDR adjusted p-values in square brackets for all treatment effects on earnings components for an endline survey (i.e. 12 hypotheses per endline survey). We do not adjust p-values for total earnings as it is a combination of the other columns. Each column-panel in Panels A and B report results for a single OLS regression of the dependent variable listed in the column heading on the treatment variables listed in the row headings (control group is the omitted category), the baseline value of the dependent variable if available (with a dummy for missing baseline value where needed), and the stratification variables for randomization: an indicator for the club's members having above median total savings at baseline and region indicators. Please see Appendix Table 10 for results on other functional forms of income. *** p<0.01, ** p<0.05, * p<0.10.

Table 4. Treatment Effects on Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary Income Source Changed from Baseline	Number of Income Streams in Last 90 Days	Total Days Worked (in last 90)	Business Investment in Last 12 Months	Expenditures and Consumption Index	Patience and Self-Control Index	Risk Tolerance Index	Altruism Index
Number of questions in index					3	4, 6	3	2
Results for index components in					AT12	AT13, AT14	AT15	AT16
Panel A. One-Year Endline								
Account Access Only (T1)	-0.03	0.03	3.66	19.54	0.02	0.04	0.02	-0.08
	(0.03)	(0.05)	(2.79)	(33.04)	(0.06)	(0.06)	(0.06)	(0.06)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Education Only (T2)	0.02	0.04	3.19	35.33	0.00	-0.00	-0.07	-0.05
	(0.03)	(0.05)	(2.75)	(30.82)	(0.04)	(0.06)	(0.06)	(0.06)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Account + Education (T3)	0.00	0.02	1.85	37.21	0.01	0.04	-0.07	-0.10
	(0.03)	(0.05)	(2.62)	(34.84)	(0.04)	(0.05)	(0.06)	(0.06)
	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
Control Group Mean	0.52	1.41	46.70	178.59	0.00	0.00	0.00	0.00
Control Group SD	0.50	0.87	45.22	531.71	1.00	1.00	1.00	1.00
N	2013	2680	2660	2674	2680	2680	2677	2680
p-values: T1 = T2	0.22	0.91	0.88	0.61	0.79	0.50	0.18	0.70
p-values: T1 = T3	0.40	0.86	0.54	0.61	0.96	0.88	0.16	0.73
p-values: T2 = T3	0.67	0.77	0.64	0.95	0.71	0.40	0.98	0.46
p-values: T1 + T2 = T3	0.77	0.50	0.21	0.71	0.96	0.89	0.82	0.73
p-values: Any Account = 0	0.38	0.80	0.56	0.65	0.69	0.29	0.86	0.17
p-values: Any Education = 0	0.36	0.67	0.73	0.25	0.98	0.94	0.08	0.39
Proportion of Obs Equal Zero	0.49	0.11	0.11	0.52	0.00	0.00	0.00	0.00
Controls for Baseline Values	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Panel B: Five-Year Endline								
Account Access Only (T1)	-0.06	0.10	4.64	29.95	0.11	-0.04	0.11	0.04
	(0.04)	(0.06)	(3.48)	(73.14)	(0.07)	(0.07)	(0.06)	(0.08)
	[0.41]	[0.41]	[0.41]	[0.73]	[0.41]	[0.73]	[0.41]	[0.73]
Education Only (T2)	-0.08**	0.03	-1.25	162.57**	0.15*	-0.01	0.04	0.01
	(0.04)	(0.06)	(3.41)	(71.35)	(0.08)	(0.07)	(0.07)	(0.07)
	[0.41]	[0.73]	[0.73]	[0.41]	[0.41]	[0.82]	[0.73]	[0.78]
Account + Education (T3)	-0.06	0.11*	7.21*	83.69	0.07	-0.04	0.08	0.06
	(0.03)	(0.06)	(3.78)	(83.78)	(0.07)	(0.07)	(0.07)	(0.07)
	[0.41]	[0.41]	[0.41]	[0.47]	[0.44]	[0.73]	[0.41]	[0.65]
Control Group Mean	0.60	1.52	69.41	398.39	0.00	0.00	0.00	0.00
Control Group SD	0.49	0.91	57.96	1071.70	1.00	1.00	1.00	1.00
N	1504	1968	1968	1924	1962	1969	1969	1969
p-values: T1 = T2	0.66	0.25	0.08	0.11	0.63	0.70	0.30	0.78
p-values: T1 = T3	0.97	0.92	0.49	0.57	0.67	0.95	0.72	0.77
p-values: T2 = T3	0.61	0.17	0.02	0.38	0.39	0.73	0.52	0.53
p-values: T1 + T2 = T3	0.15	0.81	0.44	0.35	0.11	0.90	0.52	0.93
p-values: Any Account = 0	0.48	0.03	0.01	0.68	0.80	0.53	0.11	0.45
p-values: Any Education = 0	0.17	0.70	0.79	0.07	0.32	0.97	0.88	0.72
Proportion of Obs Equal Zero	0.45	0.08	0.08	0.38	0.00	0.00	0.00	0.00
Controls for Baseline Values	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Panel C: Comparisons across One-Year and Five-Year Endlines								
p-values: T1 One-year = T1 Five-year	0.50	0.33	0.81	0.89	0.29	0.40	0.30	0.22
p-values: T2 One-year = T2 Five-year	0.03	0.84	0.29	0.08	0.06	0.93	0.23	0.42
p-values: T3 One-year = T3 Five-year	0.19	0.21	0.18	0.57	0.41	0.35	0.07	0.05
p-values: Any Account One-year = Any Account Five-year	0.92	0.09	0.07	0.54	1.00	0.21	0.26	0.09
p-values: Any Education One-year = Any Education Five-year	0.06	0.99	0.99	0.16	0.36	0.93	0.17	0.35

Notes: Unit of observation is a club member-endline. Standard errors in parentheses, clustered at the unit of randomization (the youth club), and FDR adjusted p-values in square brackets with a family of hypotheses defined as all treatment effects for an endline survey (i.e. 24 hypotheses per endline survey). Each column-panel in Panels A and B reports results for a single OLS regression of the dependent variable listed in the column heading on the treatment variables listed in the row headings (control group is the omitted category), the baseline value of the dependent variable if available (with a dummy for missing baseline value where needed), and the stratification variables for randomization: an indicator for the club's members having above median total savings at baseline and region indicators. Item non-response rates are low and our indices average across non-missing components. The smaller sample size in Column 1 is due to the number of respondents reporting no earnings at baseline. *** p<0.01, ** p<0.05, * p<0.10.

Figure 1. Quantile Treatment Effects for Savings and Income



Notes: Treatment effects on the left axis in standard deviation units of the outcome variable, standardized with respect to the full control group. On the right axis we present treatment effects for the unadjusted outcome (i.e. valued in UGX) as a percentage of the relevant control group percentile. Bars represent 95% confidence intervals. We cap confidence intervals that exceed +/-0.4 standard deviations or +/-200% percent for clarity and indicate where confidence intervals have been capped with an x. Each quantile regression controls for the baseline outcome (with a dummy for missing baseline value where needed) and stratification variable with standard errors clustered at the unit of randomization (the youth club).