Can a Budget-Recording Tool Teach Financial Skills to Youth? Experimental Evidence from a Financial Diaries Study[*]

Veronica Frisancho[†]   Alejandro Herrera[‡]   Silvia Prina[§]

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Abstract

We study the impact of a mobile app to record daily financial transactions, coupled with enumerator monitoring visits every two weeks, on youth's investment in financial literacy and financial behavior. The treatment led to a positive and statistically significant effect on financial literacy scores and greater awareness of market prices. Youth in the treatment group experienced significant improvements in access to credit. These effects persist eight months after the intervention is over.

Keywords: financial inclusion, financial diaries, financial literacy, youth

JEL Codes: : C93, D90, G41, G53, O12, O16.

[†]Corresponding author: Research Department - Inter-American Development Bank - 1300 New York Ave. NW, Washington, DC 20577. Contact: vfrisancho[at]iadb.org.

[‡]Institute for Advanced Development Studies (INESAD) - La Paz, Bolivia.

[§]Department of Economics, Northeastern University, 310A Lake Hall, Boston, MA 02115-5000.

1 Introduction

Youth in developing countries face limited access to economic opportunities, high poverty rates, and low employment levels. In addition, they often lack access to quality and suitable financial services and have insufficient capabilities to navigate an increasingly complex financial system.[1] These barriers to formal financial services perpetuate their poor economic conditions, limiting their investments in education, businesses, and other productive activities. To cope with their credit constraints, many youths turn to inadequate financial services or informal providers, which often puts them at risk of debt spirals, with dire consequences for their future access to credit.

Recent meta-analysis have shown that financial education not only improves financial skills, but it can also have positive impacts on downstream financial behavior (Kaiser and Menkhoff, 2017; Kaiser et al., 2022). While traditional lecture-based formats have proven effective to deliver financial education in the school setting (Kaiser and Menkhoff, 2019; Frisancho, 2019), there is limited evidence on how to reach out-of-school youth successfully using programs suited for more and more technologically-savvy generations.[2]

Our paper contributes to fill this gap by studying the effects of a user-friendly budget recording tool on financial literacy and behavior. We designed a smartphone application (app, from now on) that enables users to track their daily financial transactions. We randomly assigned graduating high school students (16-18 years old) in Peru to a treatment and a control group. The treatment group received access to the financial app and to biweekly individual monitoring visits. The financial app allowed youth to record all their financial transactions over six months, thus creating a *financial diary*.[3] The control group received access to a placebo app to record daily meals and one monitoring visit.

Keeping frequent records of financial transactions has the potential to develop healthy financial habits and help youth self-assess their level of financial knowledge and skills. Being aware of one's limitations could drive curiosity and interest that leads to searching for helpful financial knowledge in daily life. Thus, our intervention could affect youth's investments in financial literacy that subsequently trigger behavioral changes. Hence, we hypothesize that our financial app might serve as a behavioral nudge, propelling users to search for financial education content that the app does not deliver.

Our experimental sample is a subsample of students originally included in a larger randomized controlled trial that evaluated the effects of school-based financial education. We cross-

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[1] Four out of 10 unbanked people are in the age group 15–24 (Demirguc-Kunt et al., 2018). Moreover, the OECD-PISA 2018 financial literacy test results show that 1 in 4 15-year-old students in developing countries cannot make even simple everyday spending decisions (OECD, 2020).

[2] Voluntary out-of-school programs show limited attendance and success among youth (Berry et al., 2018).

[3] Financial diaries were developed over the past two decades to collect more accurate and high-frequency information on spending and income patterns. Collins et al. (2009) were pioneers in using these data collection strategies, which have since then been used by many to study financial habits among diverse populations. See Somville and Vandewalle (2019) as well as `https://bfaglobal.com/portfolio/` and `https://www.microfinanceopportunities.org/` for several examples.

randomize access to the budget-recording app on top of the financial education program. This allows us to measure not only the treatment impacts of the app, but complementarity/substitution patterns between the budget- recording app and school-based financial education.

The impact of the treatment is measured using a rich survey, a financial literacy test, and credit bureau data that allow us to measure the immediate and sustained effects of the intervention. Survey records and financial literacy tests were collected right before the intervention was launched and two months after the end of the recording period. These data collect sociodemographic characteristics and information on shopping, savings, and consumption habits, and market price awareness. Credit bureau data capture credit and delinquency behavior up to eight months after the end of the intervention. Furthermore, financial diaries' data provide us with supporting information on app usage and youth's daily income, spending, and savings patterns over six months.

Even though the app did not provide financial education, we find impacts on both literacy and behavior. Access to a financial diary improves financial literacy test scores by 0.08 standard deviations and knowledge of market prices by 0.34 standard deviations. Looking at the effect of the treatment by exam section reveals that the impact on financial literacy comes exclusively from improvements in the module on financial calculations. This result and the effect on market price knowledge suggest that our treatment prompts youth to search for useful financial knowledge and market information. The treatment does not lead to significant changes in the probability to save, and, despite the nature of the treatment, access to the app does not foster budgeting habits.

Our results suggest that the impact of the app seems to be driven by graduating students without exposure to school-based financial lessons while in high school. At baseline, these students lag behind in terms of their financial literacy levels: the baseline gap in exam scores relative to those who benefited from financial education content is about 0.20 standard deviations. Access to the recording app yields a 0.22 standard deviation increase in financial literacy scores among those who did not take financial education lessons in the classroom, while those with previous exposure to financial education fail to register any significant learning. In other words, the app seems to work as a substitute tool to school-based financial education, rather than a complement. However, we cannot reject that the impact of the app is significantly different across students with and without previous exposure to school-based financial education.

Credit bureau data from eight months after the end of the intervention show that exposure to our treatment leads to greater levels of inclusion in the formal financial system. The financial app increases the probability of accessing credit in regulated banks two months after the intervention period is over. We also find that outstanding debt increases for the treatment group. The effect on access to credit appears to be concentrated among individuals with no prior exposure to financial education, but once more we cannot reject the equality of the coefficients. In turn, the effect on debt balances is mainly recorded on those who received financial education in school.

Overall, the app may have led students to search for financial knowledge, particularly in the cases in which larger pre-treatment gaps were present. This is in line with previous research showing

that the use of smartphones as measurement tools during context-based education activities (i.e., use of real-life and fictitious examples to facilitate learning through practical experience) generates curiosity and interest in seeking more information about related concepts (Hochberg et al., 2018). Two findings support this channel. First, the financial literacy and behavioral effects triggered by the treatment were untargeted by the budget recording tool. Second, the app does not complement learning from school-based financial education: the treatment only yields financial literacy gains among those without previous exposure to financial literacy content.

Our contribution to the literature on financial education, literacy, and behavior among youth is three-fold. First, we move beyond existing work that focuses on school-based financial education (Bruhn et al., 2016; Bover et al., 2018; Frisancho, 2021) or personalized coaching (Modestino et al.) to foster financial capabilities among youth. We study a hands-on strategy that relies on basic digital technology to target hard-to-reach out-of-school youth. Our paper is closer to the scarce literature on financial education delivery through experiential learning (Hinojosa et al., 2009; Batty et al., 2020). Second, we rely on access to the rich survey, exam, and administrative data. This allows us to measure both the immediate effects of the intervention on financial literacy and behavior and the persistence of the behavioral effects over time. Finally, our experimental design allows us to measure both the treatment impacts of access to the budget recording tool and its complementarity/substitution patterns concerning school-based financial education.

2 Experimental Design

2.1 The treatment

Our bundled treatment consisted of a smartphone app to record daily financial transactions and biweekly visits from an enumerator. The financial app allowed users to record their cash flows easily. First, the app required users to tag each amount recorded within three general categories: income, expenses, and financial tools. Next, depending on the category selected, the app displayed a second list of subcategories to pick from. The income subcategories included: family or friends' allowances, cash gifts, permanent or temporary employment income, and sale of own assets. Expenses were subdivided into food, personal items, health, education, entertainment, clothing, gifts, and mobile internet. Finally, financial tools were classified into: savings deposits, savings withdrawals, lending to third parties, receiving payments on loans to third parties, receiving loans from third parties, and repaying loans to third parties.[4] The data stored in the app were sent to a server when the phone was connected to the internet; otherwise, this data was stored in the smartphone's memory until a connection was established.

Each individual in the treatment group received biweekly visits from an assigned enumerator. These visits had four aims. First, enumerators had to verify that all transactions recorded

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[4]Appendix B.1.1 describes in more detail the intervention materials, including screenshots of the apps that were installed in the smartphones of treatment and control individuals.

during the previous weeks were stored on the server. Second, the enumerators verified that the net balance of income and expenditures matched the net amount recorded under financial tools. Third, enumerators had to add missing transactions or edit incorrect ones at the users' request. Fourth, the enumerators provided support to solve issues or challenges associated with using the financial app. After each visit, the enumerators sent a text message to each user to schedule the next visit.[5]

To foster compliance with the usage of the apps, we offered monetary incentives to the entire experimental sample. These incentives were provided in the form of monthly mobile internet recharges equivalent to one dollar and were sustained as long as the participant did not drop out from the study. In addition, financial and meal app users participated in a monthly raffle that was implemented starting in the second month of the intervention.

While compliance with the treatment was 79% during the first month of intervention, it modestly declined to approximately 66% of app users during the remaining five months (see Table A.1, row A).[6] Students' engagement levels also decreased over time: at the launch of the intervention, almost half of the transactions were directly recorded by the participants through the app. However, toward the end of the study, virtually all transactions were entered by the enumerator during the monitoring visits (see rows B and C in Table A.1).

The app provided to the control group required participants to enter information on their daily meals. This app was visually similar to the financial app and was used to record all meals eaten by the user, classifying them as breakfast, lunch, dinner, or snack. The control group only received an enumerator visit at the beginning of the intervention period to support participants with installing the app and training them in its usage. Compliance in this group was much lower, at 20%.

2.2   Study sample and timeline

The experimental sample in our study consists of graduating students from public secondary schools in Piura, one of the most populated departments in Peru. Piura is predominantly urban and has a relatively young population. Almost one-third of the households in this region are in poverty according to official poverty lines, and services such as mobile internet are concentrated in the most densely populated urban areas (see appendix Figure A.1).

Our study considers a subsample of 60 public schools in Piura. We targeted 11th grade students during the two months before their graduation in 2018. Students were provided with an informed

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[5]During the last two months of the intervention, users also received text messages that provided feedback on their income and expenditures balances as well as they savings flows. These messages also included a personalized diagram with the expenditure distribution across sub-categories and a closing sentence encouraging savings, depending on the situation of each individual (see Appendix B.1.2). Event study analysis shows that these nudges did not affect savings (see Figure A.2).

[6]A recurrent challenge to comply with the treatment was cellphone ownership. A few individuals reported loss, theft, or malfunction of their cellphones. The field team provided notebooks as an alternative to record financial transactions when facing any of these difficulties. At the end of the study, 55 participants (30% of the treatment group) used notebooks to record their transactions.

consent form as an initial step to participate in our intervention. The consent provided a generic description of the study but did not provide information on which apps would be assigned to the participant. Since potential participants were mostly minors when they were first contacted at school, they were asked to take the consent form home so that their legal guardians could review and provide their signed approval to join the study.

We ranked schools according to the quality of internet connectivity in the area, and the number of consents received. Schools were grouped into three strata: 1) areas with high connectivity, independent of the consents received ($N_1 = 31$); 2) areas with moderate-low connectivity but with at least one signed consent ($N_2 = 15$); and 3) areas with low connectivity and no signed consents ($N_3 = 14$). Within each of these strata, we randomized access to the financial app at the school level. Since our intervention depended on smartphone use, we used baseline survey data on smartphone ownership to determine eligibility. The final sample of eligible students consisted of 982 students, of which one-quarter submitted a signed consent before graduation.

The field team aimed to recruit 400 students among those eligible by December 2018. Priority was given to students from schools in the first stratum to ensure needed access to the internet to upload the financial diaries data to our server.[7] The field team was instructed to visit eligible students without consent at their homes to try to obtain signed consent from their legal guardians and enroll them in the study. The team was able to recruit 253 students from the first stratum (65%), 79 from the second (20%), and 58 from the third (15%), for a total of 390 students from 47 schools. Forty-three percent of the recruited participants resided in districts where more than one-third of the population was under the poverty line, while 20% of them lived in small districts with fewer than 20,0000 households.

Our experiment was overlaid onto the experimental design of a larger randomized controlled trial aiming to evaluate the impact of school-based financial education on high schools students. This larger experiment ran between 2016 and 2018 in Piura and the other five regions in the country.[8]

Access to the budget recording app was randomized at the school level within strata. As expected, Table A.2 shows that the randomization process ensured a balance between the 203 participants assigned to the treatment group and the 187 assigned to the control group. Even though these strata did not consider prior treatment assignment to the financial education intervention, the randomization process also yielded balance in prior exposure to the school-based financial education.

Figure A.3 reports the activities related to the intervention (in bold) and the data collection (in italics). The baseline survey and the financial literacy test were collected between October and November 2018, toward the end of the 2018 academic year, while participants were still attending school. Then, participant recruitment began and lasted until early December 2018. Next, enumerators visited recruited participants (mid-December 2018) to help them install the

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[7]Internet access did not restrict daily use of the app to record transactions. Connectivity was only necessary to retrieve the recorded information from the app and submit it to the server automatically.

[8]For further information about the more extensive intervention, see Frisancho (2021).

assigned app (i.e., financial diaries or meals) on their smartphones and train them to use it. The recording of transactions and the monitoring visits began in late December 2018 and continued over the next six months until June 2019. The endline survey and the financial literacy test were collected two months after the intervention was over, between August and September 2019. Since individuals in the experimental sample were already out of school, enumerators visited them at their homes to conduct the survey and the financial literacy exam. Credit bureau records were collected twice, in August 2019 and February 2020.[9]

2.3   Data

Our data come from four sources: baseline and endline surveys; baseline and endline financial literacy tests; financial diaries records collected through the app; and individual credit records from the largest private credit bureau in Peru.

The baseline survey collected socioeconomic characteristics as well as parental and household information. Since subjects were still attending school, the questionnaire asked about educational and employment prospects. The survey also collected information on shopping, savings, consumption, and budgeting habits. The instrument also included a question to measure knowledge of market prices of goods and services regularly purchased by youth. The endline survey collected the same information but, since the sample had already graduated, it also included two sections about current study/work status. Moreover, an additional section for financial app users inquired about their motivation to keep using the app, their difficulties when using it, and whether they continued to use the app after the enumerators' visits concluded.

The baseline financial literacy exam contained 21 questions covering topics such as financial consumer rights, the best savings options, situations that justify debt, savings objectives, savings/purchasing capacity, spending plans in situations of income constraints, basic financial calculations, and responsible use of credit cards. Both the baseline survey and exam were administered in the classroom as the sample was still in school.

The endline financial literacy test was administered immediately after completing an endline survey at each individual's home. The endline exam consisted of 12 questions. Four questions were taken from the baseline exam and referred to the ability to save, the best use of savings, adequacy of acquiring loans to finance negative net income, and budgeting for future goals.[10] The rest of the questions, borrowed from the PISA 2018 financial literacy assessment, asked respondents to choose the best consumption choices based on the information provided (OECD, 2018).[11]

We also rely on high-frequency data from the financial diaries app, which features 17,204 finan-

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[9]All data collection efforts were conducted once the Advarra Institutional Review Board (IRB) determined that the evaluation activities were exempt from IRB oversight (protocol number Pro00029927).

[10]This selection resulted from an Item Response Theory analysis, in Table B.1, which identified the questions with the greatest variability under the criteria of discrimination and difficulty among the baseline test questions.

[11]Table B.2 presents the topics included in our endline test and compares them with those covered by the instruments used to measure financial literacy in related studies (Bruhn et al., 2016; Frisancho, 2021; Batty et al., 2015; Hinojosa et al., 2009).

cial transactions recorded by the treatment group over six months. For each transaction, the app captures the value in local currency, the type of transaction (income, expense, or financial tools), the date of the transaction, and the date and time of recording.

Assessing the behavioral effects of the treatment relying on survey data poses two challenges: potential biases present in self-reported data and a short period to allow for changes between the end of the intervention and outcome measurement. We supplemented our data with credit bureau data from EQUIFAX, a leading private credit bureau in Peru, to deal with these issues.

EQUIFAX's data capture an individual's credit standing at the time in which she is searched, providing both positive and negative records. Positive records correspond to loan balances by default status and source of the funds. Loan balances with financial institutions also reflect credit card debt, mostly provided by banks. We obtained this credit information for two points in time, August 2019 and February 2020, as detailed in Section 2.2. The search in EQUIFAX's records relied on an algorithm that matched students based on their names and national identification documents. The match rate of the control group with EQUIFAX records is 98.9% in August 2019 and February 2020. Virtually all debts held by youth in our sample are obligations with formal lenders.[12]

The data collected from the surveys and financial literacy tests allow us to evaluate the impact of access to the financial app on financial literacy and shopping and savings behavior. The credit bureau data allow us to evaluate the effect of the intervention on financial behavior and inclusion both soon after the intervention is over and a few months down the road. We focus on four outcome variables that allow us to assess the impact of the treatment on outcomes: the probability of having an outstanding loan, the number of lenders in the borrowing portfolio, the amount of outstanding debt, and the probability of delinquency.

The high-frequency transaction data captured through the app enable us to identify app usage patterns over the six months of the intervention. These records also allow us to characterize the population under study in terms of their financial lives. Table A.3 shows that youth in our sample are fairly financially active, even at a young age. Column 1 shows that over half of their transactions correspond to expenditures, while 38% are income-related. However, when considering the monetary value of transactions (see column 2), income-related transactions represent 46% of the total budget. Column 4 shows that average monthly income amounts to US$62 while average expenses are US$43. Average flows in/out of the financial tools account for nearly as much money as allocated to expenditures, signaling that youth engage in sophisticated loans and savings transactions. These data also show that income flows and transactions related to savings and loans seem to be lumpier than expenditures. Relative to expenditures, income and savings/loans generate fewer transactions (column 3). However, the average value mobilized by income and saving/loans transactions corresponds to US$7.2 and US$15.1, respectively, while this number is to US$3.6 in the case of expenditures.

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[12]Other variables such as credit scores are proprietary to the banking institutions and are not available to us.

2.4 Self-reported experience with the financial app

The endline survey measured the degree of satisfaction with the financial app among treated individuals. As shown in Column 1 of Panel A, Table A.4, almost all app users provided positive feedback about the contribution of the app to their understanding of diverse financial matters. Even though the intervention did not provide educational content or personalized financial advice, almost all users perceived that the recording process helped them better understand the use of money and the need to save more and spend less on some things. Users also reported that the app helped them learn the importance of saving, its challenges, and how to plan their expenses better.

Panel B in Table A.4 shows that almost all users perceived the app as user-friendly and that it made it easy to keep track of income and expenses. A large majority of the users also declared that the financial diaries app was more helpful than other apps they had installed on their smartphones.

Even though engagement with the app declined steadily over the six months of the intervention, active participation of the treated participants in the study was always high, fluctuating between 63.1% and 68.5%. Despite enumerators bearing most of the burden of entering the transactions, students kept attending the monitoring visits and providing detailed information on their financial transactions. One may think that the monetary incentives we provided were the main driver of their compliance. However, Panel C in Table A.4 reveals that only 40% of the subjects kept using the app to continue to receive data phone recharges, and 45% did so to participate in the cellphone raffles. Surprisingly, 90% of the users declared that they continued to use the app because it helped them to understand their money management better.

The endline survey also collected information about app fidelity once the observation period was over and incentives were discontinued. Two months after the biweekly visits were concluded, almost 43% of the treatment group continued to use the app, 60% of which used it at least four times per month on their own, as shown in Appendix Figure A.4. This result reinforces the claim that users found the app helpful to keep track of their financial transactions.

All in all, the app seems to be an effective way to arouse interest in financial matters among youth. Moreover, youth declared that they were actually able to learn more about diverse financial issues that were not targeted by the intervention. In fact, users said that the most important reason to stay as an active participant in the study was that the recording exercise helped them to better understand their own money management.

3 Empirical strategy

We assess the impact of the financial diaries intervention on financial knowledge and behavior by estimating the following OLS regression:

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$$Y_{ij} = \alpha + \beta T_{ij} + \delta X_{ij} + \phi_j + \mu_{ij} \tag{1}$$

where Y_{ij} is the outcome of interest and T_{ij} is equal to one whenever individual i in strata j graduated from a school that was randomized into the treatment group and zero otherwise. Regressions include individual- and household-level controls, X_{ij},[13] and fixed effects at the strata level, ϕ_j. μ_{ij} denotes the error term, which is clustered at the school and strata levels. We also include the baseline level of the outcome variable as control whenever possible.[14] The coefficient β captures the Intention-To-Treat (ITT) effect.

We also estimate the Treatment-on-the-Treated (TOT) effects defining *effective treatment* through a dummy, Z_{ij}, which is equal to one if the individual was a frequent user of the financial app and zero otherwise. Frequent user status is defined as having an average of at least three user-entered transactions per month over the six-month period of the intervention.[15]

TOT effects can then be obtained from estimating β^{TOT} by instrumenting Z_{ij} with the random assignment of the treatment:

$$Y_{ij} = \alpha + \beta^{\text{TOT}} \widehat{Z_{ij}} + \delta X_{ij} + \phi_j + \varepsilon_{ij} \tag{2}$$

Following Anderson (2008), sharpened False Discovery Rate (FDR) q-values are computed for each family of outcomes to deal with the potential issue of simultaneous inference. The FDR is the expected proportion of rejections that are type I errors (false rejections).

3.1 Expected channels and outcome variables

We hypothesize that recording transactions through the financial app generates a self-assessment of the user's level of financial knowledge and skills. Increased financial awareness has the potential to trigger the curiosity and interest to explore and autonomously acquire more financial knowledge. In fact, Hochberg et al. (2018) shows that the use of smartphones as measurement tools during context-based education activities arouses curiosity and self-interest to seek more information about the related concepts.

Since the app did not provide financial educational content, any improvements in conventional measures of financial literacy yielded by the treatment can be attributed to this channel. In other words, a self-assessment of the level of personal financial knowledge and skills may act as a behavioral nudge and drive the user to seek information that can reduce the knowledge gaps

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[13]Controls include: gender, age, baseline working status, dwelling overcrowding, a household asset index, and an indicator that both parents live with the participant.

[14]Implementation of an analysis of covariance (ANCOVA) to estimate the treatment effects leads to considerable improvements in power compared to a difference-in-difference specification (McKenzie, 2012). The outcomes for which we do add the baseline level as a control are the credit and delinquency outcomes.

[15]This cutoff corresponds the top 25th percentile in the distribution of user-entered transactions. The highest number of monthly transactions entered by a user within this group is 45. Total number of monthly transactions for this group (i.e. user and enumerator-entered) ranged between 4 and 68.

identified. Indeed, the descriptive evidence presented in Section 2.4 supports this: even though the intervention did not provide educational content or personalized financial advice, app users reported that recording their financial transactions increased their interest in financial matters and that they were actually able to *learn* more about diverse financial issues.

There is robust evidence showing that financial literacy is closely and causally linked to financial behavior (van Rooij et al., 2012; Bianchi, 2018; Lusardi et al., 2017). Thus, we expect the intervention to foster higher investments in financial literacy that may translate into financial behavior.

Intensive use of the app and the financial awareness effects it generates can also directly influence youth choices. For instance, the ability to review and reflect on their transactions may help them look more critically at previous myopic behavior. Even if treatment individuals cannot answer questions about inflation or interest rates correctly, they could still become more conscious and responsible consumers due to the diaries' use.

Evidence from the health literature supports the existence of this channel of impact when using self-monitoring apps. Indeed, self-monitoring of dietary intake is strongly correlated with weight-loss and weight-maintenance,[16] which is why it has become one of the critical components of programs focusing on weight-loss behavioral strategies.

We first measure the impact of the financial diaries app on the endline financial literacy score (see Online Appendix B for more details on the exam instruments.). Next, we estimate the treatment effects on four outcomes capturing financial behavior at endline: the probability to keep a budget, a shopping habits index that captures consumer savviness,[17] the degree of knowledge about market prices, and the probability of saving.

To measure the degree of knowledge about market prices, we considered a list of items and asked respondents to give their best price guess in each case. A respondent scored higher when the price she reported was within the range of minimum and maximum market prices derived from price data coming from two local retailers.[18] The respondent's score was then standardized relative to the control group. The probability of saving included both formal and informal savings.

Regarding the data from the credit bureau records, we considered four variables. First, we construct the probability to hold outstanding debt, defined as a binary variable equal to one if an individual holds outstanding debt with at least one financial institution by the observation date and zero otherwise. Second, we gather the number of financial institutions with whom an individual contracted a debt as a proxy for the degree of diversification of her sources of

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[16]See Yu et al. (2015); Carter et al. (2013); Dunn et al. (2019).

[17]We use a standardized index to aggregate five variables that capture if the individual: bargains before shopping, compares prices before shopping, does not buy unplanned items, compares prices online, and plans purchases.

[18]The list of items included food staples such as a kilo of rice, a can of evaporated milk, a kilo of potatoes, a dozen eggs, a kilo of fish, a kilo of poultry, a liter of oil, and a liter of sugar, as well as products/services that youth were more likely to demand such as a liter of ice cream, a movie ticket, a pair of tennis shoes, and 200 MB of cellphone data. To benchmark the prices provided by our sample, we consulted the websites https://preciosmundi.com/peru/ and https://www.metro.pe/ at the time of the interview.

financing. Third, we look at the outstanding debt expressed in US\$, using the August 2019 exchange rate, fixed for both data rounds. Finally, we define the probability of delinquency as a binary variable equal to one if the individual holds a positive share of her debt in arrears and zero otherwise.

4   Results

Table 1 reports the average ITT effects on financial education and behavior. The first row in Panel A shows that the treatment had a positive and statistically significant effect on the endline financial literacy score (0.08 standard deviations). This effect confirms that although our treatment does not deliver specific content on financial education *per se*, self-assessment of financial knowledge and skills triggered a search for improvement in these skills. In fact, looking at the effect of the treatment by exam section reveals that the impact on financial literacy comes exclusively from improvements in the module on financial calculations. This suggests that our treatment prompts youth to try to close knowledge gaps that the recording process makes evident.

Panel B focuses on behavioral outcomes. The habit formation channel does not seem to have been activated, as we fail to register a treatment effect on the probability of using a budget. Access to the app did not improve shopping strategies, nor affect the probability to save among youth. Nevertheless, access to a financial diary did induce greater awareness about market prices. Relative to the control group, the treatment group became considerably savvier when searching for price information, recording a marginal improvement in their knowledge of prices equivalent to a third of a standard deviation. Hand in hand with the previous findings, a greater awareness of market prices among the treatment group reveals that using the financial app drives users to seek more information to plan future expenditures.

The TOT effects reported in Appendix Table A.5 confirm and reinforce the treatment impacts identified for financial literacy and price knowledge. Among high-intensity users, the effect of the provision of the financial app on the financial literacy endline test amounts to 0.13 standard deviations. Similarly, the average effect on price knowledge among high-intensity users increases to 0.56 standard deviations.

Table 2 focuses on youth's credit and delinquency outcomes up to 8 months after the intervention is over. Column 3 portrays the immediate effects of the treatment at the endline, while column 4 checks the persistence of these effects six months later. The treatment has a small positive effect equivalent to 1.2 percentage points on the probability of holding outstanding debt, which is statistically significant and consistently increasing over time.[19] The treatment also leads to greater and sustained diversification in youth's borrowing portfolio: there is a positive and significant effect on the number of lenders both at endline and six months later. In fact, the effect size increases over time, showing that the treatment group increasingly diversifies its sources of debt. Access to the app also raises the amount of outstanding debt by 24% eight

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[19]The p-value of the coefficient in column 4 is 0.11.

months after the intervention was over.

Overall, the results in Table 2 signal that app users experience greater levels of inclusion in the formal financial system that materialize right after the intervention is over. Even though the effect on access to credit is small, we see that, over time, treated students do benefit from higher levels of portfolio diversification and greater outstanding debt from formal lenders. Since we do not find changes in the probability of delinquency nor the debt-income ratio, we argue that the expansion of the debt portfolio did not have harmful effects on treated youth.

The TOT effects presented in Table A.6 show larger effects on credit outcomes among high-intensity users. The probability of holding outstanding debt increases by 2.3 percentage points by endline, and the effect remains positive 8 months after the intervention concluded. By February 2019, the diversification effect among frequent users amounts to 0.7 more lenders in their portfolios, and debt levels increase by 29% relative to the control group.

4.1 Heterogeneity analysis

To further explore what is driving the results, we estimate heterogeneous treatment effects in our sample. Table 3 presents the impact of the treatment depending on previous exposure to school-based financial education during high school. Since a random sub-sample of our study sample was previously exposed to personal finance lessons delivered during the school day, we explore whether the financial app complements or substitutes this effort.

Students who were not previously exposed to financial education start off behind in terms of their financial literacy levels: the baseline gap in exam scores relative to those who benefited from financial education content is about 0.20 standard deviations. The results show that the financial diaries app helps close this gap. In fact, the average financial literacy gains recorded in Table 1 can be attributed to those who did not receive the financial education program and subsequently received access to the financial app. Within this subsample, access to the recording app yields a 0.22 standard deviation increase in financial literacy scores, while beneficiaries of the school-based financial education program experience no change in scores. The effect among those who did not receive lessons during high school is economically important and robust to multiple hypothesis testing. Similarly, the gains in price knowledge are only robust among those who were not exposed to the financial education program. Nevertheless, column (5) shows that we have not enough statistical power to reject that the effects are the same across the sub-samples defined by previous exposure to school-based financial education.

These findings suggest that the financial diaries app does not complement learning from school-based financial education, but rather works as a substitute. They also support the hypothesis that recording daily transactions raises awareness among those lagging behind, making them realize their shortage of financial knowledge and motivating them to invest in acquiring additional skills.

Moving on to the analysis of credit outcomes, Table 4 shows that greater access to credit is also

concentrated among those without prior exposure to financial education at school. However, the average number of lenders in the borrowing portfolio increases regardless of previous exposure to financial education. In turn, average outstanding debt significantly increases by US$21.82 among those with a financial education background, while it does not change among those excluded from the school-based program.[20]

Overall, the use of smartphones as measurement tools seems to generate curiosity and interest in seeking more information and knowledge about financial matters. Even though we lack follow-up data on youth's investments in financial literacy, two findings support this channel. On the one hand, the financial literacy and behavioral effects triggered by the treatment were untargeted by the budget recording tool. Youth were not provided with specific content (beyond fostering budgeting habits and savings towards the end of the treatment) but still learned new financial skills, particularly those related to financial calculations. On the other hand, the treatment appears to yield larger financial literacy gains among those without previous exposure to financial literacy content. This might suggests that the treatment was more effective in improving knowledge levels among those who started lagging behind and were more likely to notice their shortcomings through exposure to the treatment.

5   Cost analysis

One of the advantages of the app is that it can reach out-of-school-youth in a decentralized manner at a potentially low cost. This section focuses on the cost analysis of the intervention, which is particularly useful for future scalability efforts.

Appendix Table 5 reports the per capita costs of implementation and monitoring for each treatment group over six months (see row G). We also present these costs adjusted by the inflation rate (see row H) as well as by the inter-temporal discount rate (see row I).[21]

Relative to the meals app, the cost per capita of providing the financial app (adjusted by inflation and discount rates) was US$185.3. If we look at the cost categories in the treatment group, almost 45% of the total costs of implementing the treatment correspond to enumerators' salaries and per diem.

Even though our pilot included the role of enumerators to guarantee higher levels of compliance, their role can be eliminated in settings where youth are more digitally savvy, more used to relying on recording apps in other areas of their lives, and have more extended access to smartphones. If the intervention replaces the role of enumerators with that of reminders, peer support, and other low-cost strategies, the main sources of costs will be those coming from app development and data storage and transfer. As both of these are fixed costs, the per capita cost decreases as the scale increases. Compared to financial education programs that require resources to develop

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[20]See Appendix Tables A.7 and A.8 for the TOT effects related to the heterogeneity analysis.

[21]Both inflation and inter-bank interest rates between January and June 2019 were extracted from the Central Reserve Bank of Peru (BCRP) website. Last accessed April 2021.

and print materials and train instructors, fostering the use of the financial recording app offers a potentially lower-cost strategy.

6   Conclusion

Financial education has become a popular prescription for fostering financial inclusion strategies in developing countries. In recent years, a focus on youth has shown promising and robust effects on financial literacy and downstream behavior. However, most interventions rely on lecture-based formats while youth are still in school. We study the impact of a budget-recording tool intended to foster habit formation and financial awareness on out-of-school youth's investment in financial literacy and financial behavior.

Our sample is comprised of recent high school graduates in urban areas in Peru. We randomize access to a mobile app to record daily financial transactions, coupled with enumerator monitoring visits every two weeks. We argue that the intervention has two channels of impact: it directly fosters budgeting and saving habits, and makes financial transactions more salient. We thus hypothesize that the app led students to search for financial knowledge, particularly in cases where larger pre-treatment gaps were present.

The treatment led to a positive and statistically significant effect on financial literacy scores and greater awareness of market prices. Moreover, youth in the treatment group experienced significant improvements in access to credit and increased the number of lenders in their borrowing portfolios. These effects persist eight months after the intervention is over and materialize into higher levels of outstanding debt.

The heterogeneity analysis suggests that the financial diaries app does not complement learning from school-based financial education but instead works as a substitute. The treatment effects estimated for both financial literacy and greater access to credit might be attributed to those who did not receive financial education in school. Even though our findings are only suggestive, they point towards a promising avenue of action to reach out-of-school youth who were not exposed to financial education while in school.

Our findings suggest that access to the financial app and monitoring can help youth realize the knowledge gaps they face, motivating them to search for financial information and leading to effects on financial behavior. The budget-recording tool has the potential to achieve two goals. First, it could trigger financial knowledge acquisition. Second, it is a potentially low-cost way to reach young people who can no longer be targeted at school. This is particularly important as it is a more suitable strategy to reach dropouts or school graduates without exposure to financial education.

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Table 1: **Intention-to-treat (ITT) effects on financial literacy and habits**

	Control mean (1)	ITT (2)
Panel A: Effects on financial literacy		
Financial Literacy	0.000	0.079*
	(0.076)	(0.025)
Savings	0.000	-0.032
	(0.079)	(0.070)
Debt	0.000	0.042
	(0.077)	(0.097)
Investment	0.000	0.079
	(0.076)	(0.092)
Financial calculations	0.000	0.162*
	(0.078)	(0.044)
Financial choices	0.000	0.070
	(0.086)	(0.103)
Panel B: Effects on shopping and savings habits		
Pr(Budgeting)	0.413	0.012
	(0.038)	(0.029)
Shopping index	-0.000	-0.175
	(0.086)	(0.120)
Price knowledge	-0.000	0.335*
	(0.078)	(0.081)
Pr(Save)	0.497	0.014
	(0.039)	(0.017)

NOTE: total number of observations $N = 349$. All outcomes measured during the endline survey. Financial Literacy reflects the standardized score on the financial literacy test. Pr(Budgeting) is a binary outcome equal to whether the individual does a personal budget and zero otherwise. The shopping index captures several shopping strategies, such as negotiating the sale price, comparing prices, and asking several businesses to search for the best price. Price knowledge collects a list of essential items and compares the price guessed by the respondent to a range of actual prices in markets. Pr(Save) is a binary outcome equal to one if the person has personal savings. All specifications include a set of controls: gender, age, currently working, ratio of household members to bedrooms, lives with both parents, strata fixed effects, and the value of the dependent variable level at baseline. Standard errors clustered at the strata and school level are reported in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

Table 2: **Intention-to-treat (ITT) effects on access to credit, delinquency rates and number of lenders**

	At endline		8 months later	
	Control mean (1)	ITT Effect (2)	Control mean (3)	ITT Effect (4)
Pr(Credit)	0.021	0.012*	0.043	0.019
	(0.011)	(0.004)	(0.016)	(0.007)
Number of lenders	0.021	0.012*	0.043	0.038*
	(0.011)	(0.004)	(0.019)	(0.011)
Debt (USD)	3.057	1.472	11.011	13.660***†††
	(1.656)	(1.167)	(6.466)	(0.480)
Pr(Default)	0.005	0.002	0.011	0.002
	(0.005)	(0.001)	(0.007)	(0.008)
Debt/income ratio	0.257	−0.135	0.510	0.231
	(0.122)	(0.092)	(0.224)	(0.126)

NOTE: total number of observations $N = 390$. All outcome variables are constructed using information from EQUIFAX. "At endline" corresponds to August 2019, while "8 months later" corresponds to February 2020. All outcome variables in the table reflect records in the regularized bank exclusively. Pr(Credit) is a binary outcome equal to one if the person has at least one credit with at least one financial institution and zero otherwise. Number of lenders reflects the number of financial institutions with which the individual has a debt. Debt reflects the outstanding debt of the individual expressed in USD at the August 2019 exchange rate and Winsorized at the 1% and 99% percentiles. Pr(Default) reflects a binary outcome equal to one if the outstanding debt or part of it is classified under the problem delinquency categories. The debt-to-income ratio is calculated using the baseline reported income. All specifications include a set of controls: gender, age, currently working, ratio of household members to bedrooms, lives with both parents, and strata fixed effects. Standard errors clustered at the strata and school level are reported in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

Table 3: **Heterogeneous effects on financial literacy and habits**

| | Exposure to School-based FinEd in 2016 | | | | |
| | Control | | Treated | | P-value |
	Control mean (1)	ITT Effect (2)	Control mean (3)	ITT Effect (4)	difference (2)-(4) (5)
Panel A: financial literacy					
Financial Literacy	0.007	0.218***†††	−0.008	−0.108	0.160
	(0.105)	(0.013)	(0.112)	(0.140)	
Panel B: shopping and savings habits					
Pr(Budgeting)	0.337	0.080	0.500	−0.072	0.454
	(0.052)	(0.040)	(0.056)	(0.132)	
Shopping index	−0.027	−0.172	0.030	−0.172	0.999
	(0.118)	(0.069)	(0.126)	(0.289)	
Price knowledge	0.016	0.225***†††	−0.019	0.484	0.345
	(0.107)	(0.013)	(0.114)	(0.220)	
Pr(Save)	0.506	0.009	0.487	0.023	0.896
	(0.053)	(0.025)	(0.057)	(0.074)	

NOTE: total number of observations $N = 349$. All outcomes measured during the endline survey. Financial Literacy reflects the standardized score on the financial literacy test. Pr(Budgeting) is a binary outcome equal to whether the individual does a personal budget and zero otherwise. The shopping index captures several shopping strategies, such as negotiating the sale price, comparing prices, and asking several businesses to search for the best price. Price knowledge collects a list of essential items and compares the price guessed by the respondent to a range of actual prices in markets. Pr(Save) is a binary outcome equal to one if the person has personal savings. All specifications include a set of controls: gender, age, currently working, ratio of household members to bedrooms, lives with both parents, strata fixed effects, and the value of the dependent variable level at baseline. Standard errors clustered at the strata and school level are reported in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

Table 4: **Heterogeneous effects on access to credit, delinquency rates and number of lenders (8 month after the endline)**

| | Exposure to School-based FinEd in 2016 | | | | P-value |
| | Control | | Treated | | |
	Control mean (1)	ITT Effect (2)	Control mean (3)	ITT Effect (4)	difference (2)-(4) (5)
Pr(Credit)	0.021	0.035*	0.067	0.002	0.317
	(0.022)	(0.012)	(0.023)	(0.014)	
Number of lenders	0.021	0.044*	0.067	0.035*	0.453
	(0.027)	(0.014)	(0.028)	(0.009)	
Debt (USD)	4.891	7.897	17.607	21.823**	0.193
	(8.984)	(3.943)	(9.327)	(4.887)	
Pr(Default)	−0.000	0.009	0.022	−0.005	0.716
	(0.010)	(0.006)	(0.011)	(0.026)	
Debt/income ratio	0.099	0.576*	0.954	−0.140	0.112
	(0.310)	(0.165)	(0.322)	(0.097)	

NOTE: total number of observations $N = 390$. Outcomes in the columns, and heterogeneity cuts in the rows. All outcome variables are constructed using information from EQUIFAX (February, 2020). All outcome variables in the table reflect records in the regularized bank exclusively. Pr(Credit) is a binary outcome equal to one if the person has at least one credit with at least one financial institution and zero otherwise. Number of lenders reflects the number of financial institutions with which the individual has a debt. Debt reflects the outstanding debt of the individual expressed in USD at the August 2019 exchange rate and Winsorized at the 1% and 99% percentiles. Pr(Default) reflects a binary outcome equal to one if the outstanding debt or part of it is classified under the problem delinquency categories. The debt-to-income ratio is calculated using the baseline reported income. All specifications include a set of controls: gender, age, currently working, ratio of household members to bedrooms, lives with both parents, and strata fixed effects. Standard errors clustered at the strata and school level are reported in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Spades denote significance levels († 10%, 5% ††, 1% † † †) based on sharpened FDR q-values.

Table 5: **Cost-effectiveness analysis of the financial diaries app**

	Nutrition app	Financial app	
	N=187	N=203	
	(1)	(2)	
Cost	Amount in US$		
(A) App development	$ 3,300.00	$ 3,300.00	
(B) Data storage and transfer	$ -	$ 23,530.00	
(C) Fieldwork staff	$ 7,634.00	$ 22,792.00	
(D) SMS reminders	$ -	$ 255.26	
(E) Data credits	$ 1,840.80	$ 1,840.80	
(F) Total cost	$ 12,774.80	$ 51,718.06	Differential (2)-(1)
(G) Per capita cost	$ 68.31	$ 254.77	$ 186.5
(H) Per capita cost (adjusted by inflation)	$ 69.7	$ 260.1	$ 190.4
(I) Per capita cost (adjusted by inflation and intertemporal discount rate)	$ 67.9	$ 253.2	$ 185.3

SOURCE: authors' calculations. Inflation rate and inter-bank interest rate for the period January-June 2019, extracted from the Central Reserve Bank of Peru (BCRP).

A   Appendix

Figure A.1: **Poverty, Mobile Internet Coverage and Sampling in the Districts of Piura**

(a) Monetary Poverty (% of households)

(b) Mobile Internet Coverage

(c) Sample Distribution
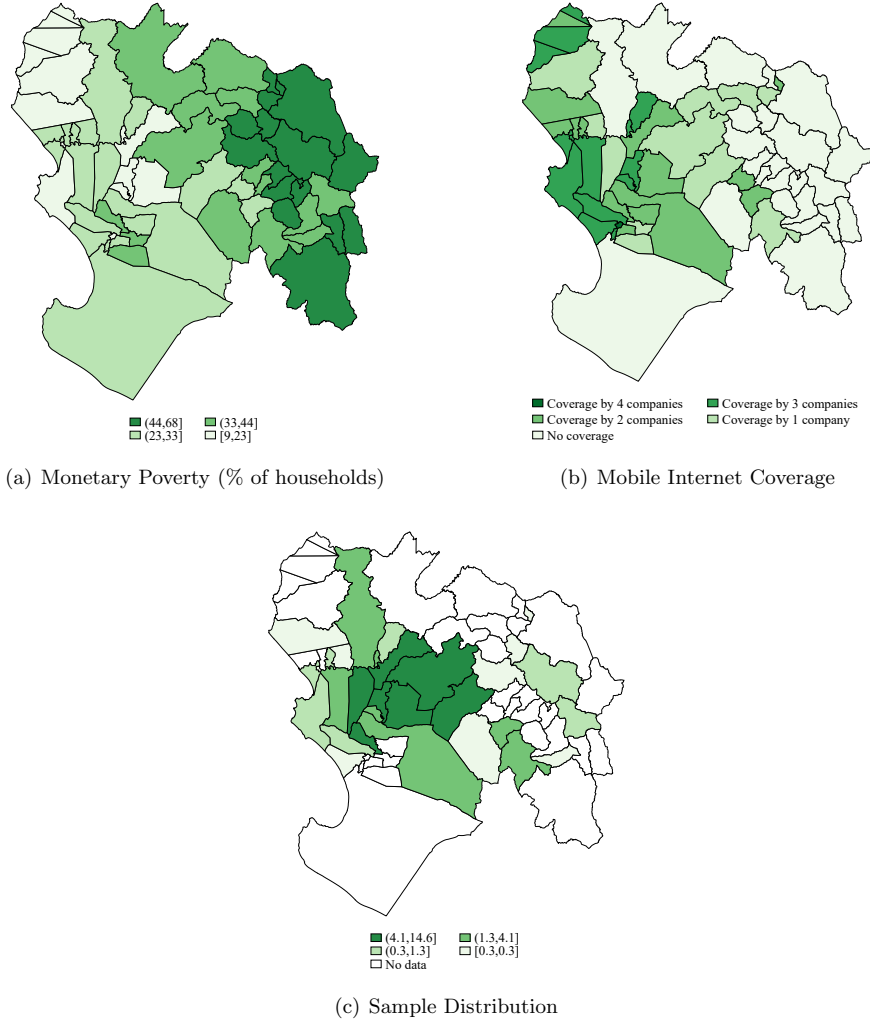
NOTE: panel (a) own elaboration based on the Poverty Map INEI (2018). Panel (b) own elaboration based on data from the Supervisory Authority for Private Investment in Telecommunications (OSIPTEL), on the mobile internet signal coverage of the four most important telephone companies in the country: Claro, Bitel, Movistar and Entel. The map reflects the median number of companies with coverage in the localities of each district of Piura, according to the last access to OSIPTEL in June 2021.

Figure A.2: **Effect of pie charts & savings nudges**
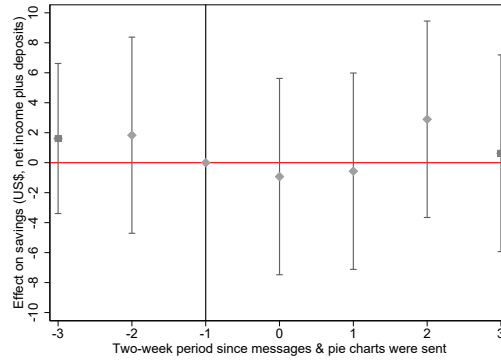
NOTE: plotted dots reflect the estimation of an event study to evalute the effect of text messaging during the last two months of the intervention. The horizontal axis labels denote two-week periods around the moment in which the nudges started (labelled as zero).

Figure A.3: **Study timeline**
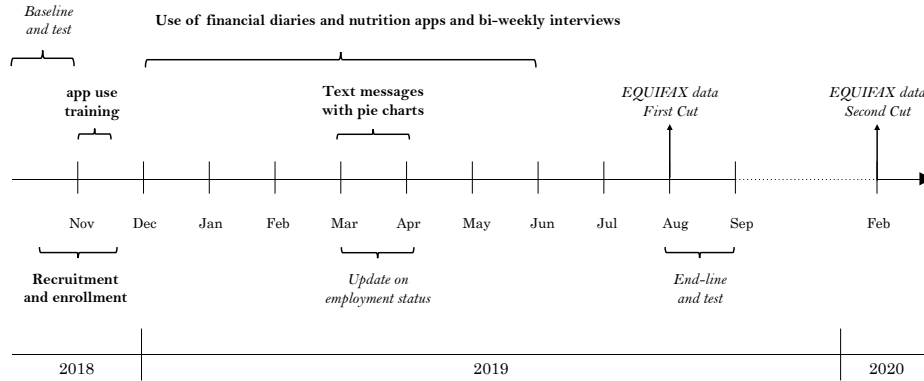
NOTE: intervention activities in **bold** and data collection activities in *italics*.

Table A.1: **Treatment compliance**

	Jan	Feb	Mar	Apr	May	Jun
(A) Compliance	0.793	0.685	0.631	0.655	0.641	0.665
(B) Transactions recorded by user	0.459	0.405	0.243	0.154	0.052	0.034
Non-edited transaction (as % of (B))	0.727	0.667	0.777	0.712	0.650	0.597
Edited transactions (as % of (B))	0.273	0.333	0.223	0.288	0.350	0.403
(C) Transactions recorded by enumerator	0.541	0.595	0.757	0.846	0.947	0.966

NOTE: row (A) shows the percentage of compliers over the total number of treated. Rows (B) and (C) shows the fraction of monthly transactions recorded by the user and those recorded by the surveyor, respectively.

Table A.2: **Balance check**

Variable	Control mean	T-C
Male	0.567	0.059
	[0.497]	[0.056]
Age	16.572	-0.055
	[0.873]	[0.101]
Works	0.209	0.151
	[0.407]	[0.067]**
Ratio of household members to bedrooms	2.059	-0.024
	[1.080]	[0.132]
Lives with both parents	0.690	0.059
	[0.464]	[0.049]
Asset index	-0.000	-0.119
	[1.000]	[0.197]
High level of parental supervision	0.813	-0.079
	[0.391]	[0.053]
Has dinner with parents 7 days a week	0.374	0.025
	[0.485]	[0.055]
Financial autonomy (0-100)	49.385	-0.213
	[11.396]	[1.220]
Time inconsistency: hyperbolic	0.235	0.065
	[0.425]	[0.044]
Risk averse	0.701	0.014
	[0.459]	[0.054]
Self-control	-0.000	0.050
	[0.858]	[0.123]
Impulsiveness: planning	0.000	0.105
	[0.813]	[0.103]
Financial literacy raw score (0-15)	12.123	0.128
	[2.937]	[0.405]
Price knowledge (0-9)	6.176	-0.196
	[1.645]	[0.225]
Prepares a personal budget	0.719	0.006
	[0.451]	[0.059]
Saves	0.615	0.040
	[0.488]	[0.059]
Bargains on purchases	0.733	-0.033
	[0.444]	[0.051]
Saves to buy something unaffordable	0.930	-0.004
	[0.255]	[0.027]
Compares prices before shopping	0.722	-0.082
	[0.449]	[0.062]
Buys something not planned for	0.283	0.052
	[0.452]	[0.058]
Total expenditures last month (US$)	95.211	32.156
	[115.106]	[16.440]*
Total earnings last month (US$)	32.250	15.000
	[43.864]	[6.389]**
Financial education program	0.481	-0.003
	[0.501]	[0.165]

NOTE: total observations $N = 390$, except for "Ratio of household members to bedrooms" ($N = 386$) and for "Prepares a personal budget" ($N = 381$). Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors in brackets.

Table A.3: **Transaction statistics by category**

|	All months		Monthly average	
	Fraction of total number of transactions (1)	Fraction of total amount in USD (2)	Mean number of transactions (3)	Mean amount in USD (4)
Income	0.381	0.462	8.6	62.0
Expenditures	0.522	0.302	12.5	43.1
Financial tools	0.097	0.237	2.7	41.0
Total	1.000	1.000	7.9	48.9

NOTE: the first two columns reflect the percentages of the totals recorded between the months of January to June 2019. The last two columns show the average between the six monthly averages.

Table A.4: **Self-reported perceptions about the app by financial education program**

	Overall (N=182)	
	Mean (1)	S.D. (2)
Panel A: Experiential learning		
Helped to better understand money usage	0.890	(0.314)
Understood the need to save more money	0.934	(0.249)
Understood the need to spend less on some things	0.923	(0.267)
Learned the importance of saving	0.918	(0.276)
Learned that saving is not so easy	0.885	(0.320)
Learned how to better plan expenses	0.923	(0.267)
Made think about savings use	0.929	(0.258)
Panel B: App feedback		
Easy to keep track of income/expenses	0.918	(0.276)
More useful than other installed apps	0.885	(0.320)
App user-friendly and easy to use	0.896	(0.307)
Panel C: Reasons to keep using the app		
To better understand one's money use	0.896	(0.307)
Monthly cell-phone raffle	0.445	(0.498)
Peer motivation	0.291	(0.456)
Monetary incentives every two weeks	0.390	(0.489)
Other	0.060	(0.239)

NOTE: standard deviation in parentheses.

Figure A.4: **Average app use two months after biweekly visits finished**
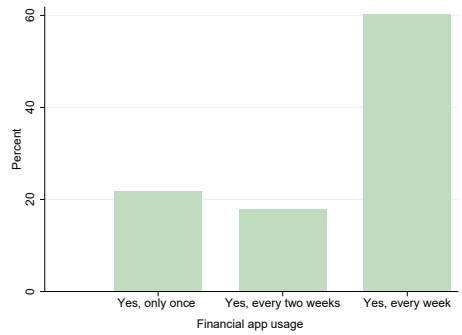
NOTE: result of the question *"Did you use the app during August (two months after the end of the monitors' visits)? How often?"*. The bars show the response of 78 out of 182 financial app users who responded to the endline survey and who continued to use the app on their own.

Table A.5: **Treatment-on-treated (TOT) effects on financial literacy and habits**

	Control mean (1)	TOT (2)
Panel A: financial literacy		
Financial Literacy	0.000	0.130*
	(0.076)	(0.035)
Savings	0.000	-0.055
	(0.076)	(0.114)
Debt	0.000	0.070
	(0.075)	(0.160)
Investment	0.000	0.127
	(0.073)	(0.142)
Financial calculations	0.000	0.265*
	(0.076)	(0.075)
Financial choices	0.000	0.116
	(0.076)	(0.169)
Panel B: shopping and savings habits		
Pr(Budgeting)	0.413	0.020
	(0.038)	(0.047)
Shopping index	-0.000	-0.287
	(0.086)	(0.203)
Price knowledge	-0.000	0.555*
	(0.078)	(0.163)
Pr(Save)	0.497	0.023
	(0.039)	(0.026)

NOTE: total number of observations $N = 349$. Financial Literacy reflects the standardized score on the financial literacy test. Pr(Budgeting) is a binary outcome equal to whether the individual does a personal budget and zero otherwise. The shopping index captures several shopping strategies, such as negotiating the sale price, comparing prices, and asking several businesses to search for the best price. Price knowledge collects a list of essential items and compares the price guessed by the respondent to a range of actual prices in markets. Pr(Save) is a binary outcome equal to one if the person has personal savings. All specifications include a set of controls: gender, age, currently working, ratio of household members to bedrooms, lives with both parents, strata fixed effects, and the value of the dependent variable level at baseline. Standard errors clustered at the strata and school level are reported in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

Table A.6: **Treatment-on-treated (TOT) effects on credit outcomes**

	At endline		8 months later	
	Control mean (1)	TOT Effect (2)	Control mean (3)	TOT Effect (4)
Pr(Credit)	0.006	0.023*	0.021	0.035
	(0.028)	(0.007)	(0.041)	(0.014)
Number of lenders	0.006	0.023*	0.049	0.070*
	(0.028)	(0.007)	(0.050)	(0.022)
Debt (USD)	0.342	2.697	19.359	25.032*** ††
	(4.263)	(2.105)	(16.751)	(1.404)
Pr(Delinquency)	-0.001	0.004	-0.002	0.004
	(0.013)	(0.002)	(0.019)	(0.014)
Debt/income ratio	0.257	-0.248	0.510	0.423
	(0.122)	(0.170)	(0.223)	(0.240)

NOTE: total number of observations $N = 390$. All outcome variables are constructed using information from EQUIFAX. "At endline" corresponds to August, 2019, while "8 months later" corresponds to February, 2020. All outcome variables in the table reflect records in the regularized bank exclusively. Pr(Credit) is a binary outcome equal to one if the person has at least one credit with at least one financial institution and zero otherwise. Number of lenders reflects the number of financial institutions with which the individual has a debt. Debt reflects the outstanding debt of the individual expressed in USD at the August 2019 exchange rate and Winsorized at the 1% and 99% percentiles. Pr(Default) reflects a binary outcome equal to one if the outstanding debt or part of it is classified under the problem delinquency categories. The debt-to-income ratio is calculated using the baseline reported income. All specifications include a set of controls: gender, age, currently working, ratio of household members to bedrooms, lives with both parents, and strata fixed effects. Standard errors clustered at the strata and school level are reported in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

Table A.7: **Heterogeneous TOT effects on financial literacy and habits**

| | Exposure to School-based FinEd in 2016 | | | | P-value |
| | Control | | Treated | | |
	Control mean (1)	ITT Effect (2)	Control mean (3)	ITT Effect (4)	difference (2)-(4) (5)
Panel A: financial literacy					
FinLit Score	0.007	0.392***††	-0.008	-0.077	0.141
	(0.105)	(0.032)	(0.112)	(0.208)	
Panel B: shopping and savings habits					
Pr(Budgeting)	0.337	0.142	0.500	-0.114	0.434
	(0.052)	(0.072)	(0.056)	(0.201)	
Shopping index	-0.027	-0.311	0.030	-0.268	0.943
	(0.118)	(0.113)	(0.126)	(0.464)	
Price knowledge	0.016	0.391***††	-0.019	0.745	0.379
	(0.107)	(0.039)	(0.114)	(0.350)	
Pr(Save)	0.506	0.015	0.487	0.036	0.908
	(0.053)	(0.047)	(0.057)	(0.115)	

NOTE: total number of observations $N = 349$. Outcomes in the columns, and heterogeneity cuts in the rows. FinLit Score in first column reflects the standardized score on the financial literacy test. Pr(Budgeting) is a binary outcome equal to whether the individual does a personal budget and zero otherwise. The shopping index captures several shopping strategies, such as negotiating the sale price, comparing prices, and asking several businesses to search for the best price. Price knowledge collects a list of essential items and compares the price guessed by the respondent to a range of actual prices in markets. Pr(Save) is a binary outcome equal to one if the person has personal savings. All specifications include a set of controls: gender, age, currently working, ratio of household members to bedrooms, lives with both parents, strata fixed effects, and the value of the dependent variable level at baseline. Standard errors clustered at the strata and school level are reported in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, † † † 1%) based on sharpened FDR q-values.

Table A.8: **Heterogeneous TOT effects on credit outcomes (8 month after the end-line)**

	Exposure to School-based FinEd in 2016				P-value
	Control		Treated		
	Control mean (1)	ITT Effect (2)	Control mean (3)	ITT Effect (4)	difference (2)-(4) (5)
Pr(Credit)	0.021	0.084*	0.067	0.060	0.317
	(0.022)	(0.030)	(0.023)	(0.016)	
Number of lenders	0.021	0.069*	0.067	0.002*	0.453
	(0.027)	(0.026)	(0.028)	(0.024)	
Debt (USD)	4.891	14.393	17.607	38.607*	0.193
	(8.984)	(8.951)	(9.327)	(9.176)	
Pr(Default)	-0.000	0.018	0.022	-0.009	0.716
	(0.010)	(0.014)	(0.011)	(0.046)	
Debt/income ratio	0.099	1.135	0.954	-0.278	0.112
	(0.310)	(0.352)	(0.322)	(0.127)	

NOTE: total number of observations $N = 390$. Outcomes in the columns, and heterogeneity cuts in the rows. All outcome variables are constructed using information from EQUIFAX (February, 2020). All outcome variables in the table reflect records in the regularized bank exclusively. Pr(Credit) is a binary outcome equal to one if the person has at least one credit with at least one financial institution and zero otherwise. Number of lenders reflects the number of financial institutions with which the individual has a debt. Debt reflects the outstanding debt of the individual expressed in USD at the August 2019 exchange rate and Winsorized at the 1% and 99% percentiles. Pr(Default) reflects a binary outcome equal to one if the outstanding debt or part of it is classified under the problem delinquency categories. The debt-to-income ratio is calculated using the baseline reported income. All specifications include a set of controls: gender, age, currently working, ratio of household members to bedrooms, lives with both parents, and strata fixed effects. Standard errors clustered at the strata and school level are reported in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Dags denote significance levels († 10%, †† 5%, ††† 1%) based on sharpened FDR q-values.

B Online Appendix (not for publication)

B.1 Intervention materials

B.1.1 Smartphone apps

Individuals in the treatment group recorded their daily financial transactions in the financial app, entering as many transactions as they had each day. The app was organized into three main categories of transactions: income, expenses, and financial tools, as seen in Panel (a) of Figure B.1. On the other hand, individuals in the control group just recorded their daily meals in the app, entering as many meals as they had each day. The app was organized into four categories: breakfast, lunch, dinner, and snacks, as seen in Panel (b) of Figure B.1.

Figure B.1: Treatment and control groups apps illustrations

(a) Treatment (b) Control
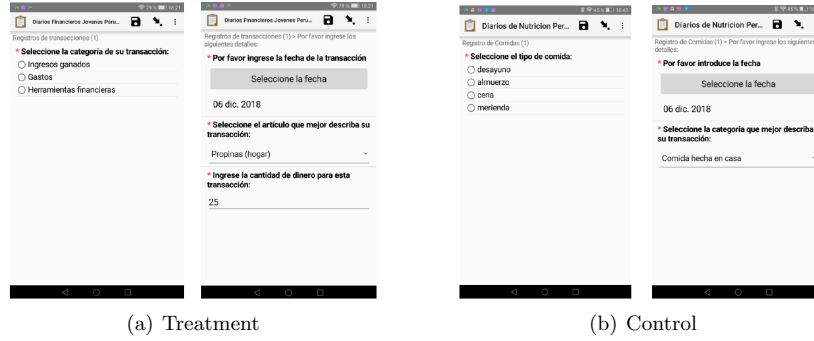
B.1.2 Personalized text messages to encourage savings

We briefly describe the procedure for sending text messages to encourage savings. The first part of the message contained general information analyzed in the last visit:

> "Hello, thank you for recording your daily transactions in the Financial Diaries app. You are doing it very well. Let me remind you of the information you shared in our last meeting. During the past two weeks you earned 203 soles and spent 147 soles..."

The second part of the message was conditional on how individual savings levels. The messages were always positively framed, avoiding shaming that could backfire. Depending on the different scenarios, three types of messages were delivered:

a) Users who did not save in the last two weeks and had cumulative savings equal to zero were encouraged to save:

> "... and you saved 0 soles. We encourage you to try hard and start developing the habit of saving. You can do it!"

b) Users who did not save in the last two weeks, but had a positive balance of cumulative savings, were reminded of their total balance:

> "... and you saved 0 soles. Your current savings balance is 170 soles. Congrat-ulations!"

c) Users who saved something in the last two weeks were told how much they would get if they were to keep saving at the same rate:

> "... and you saved 7 soles. If you keep saving the same amount until the end of May, you will have 35 soles. Your current savings balance is 13 soles. Congratulations!"

Figure B.2: Pie chart example
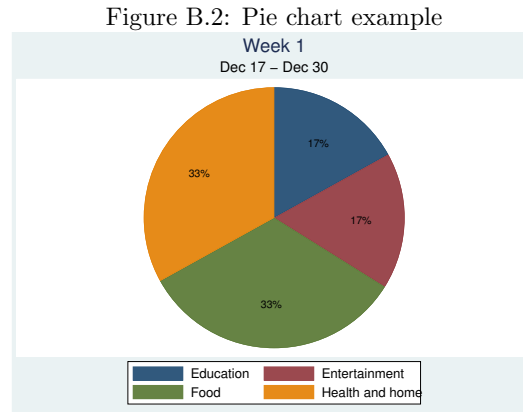
In addition to the text messages fostering savings, we also sent pie charts that summarized participants' expenditure patterns by category (see example in Figure B.2 in the Appendix). This tool intended to highlight non-essential expenditures as a potential margin to adjust and redirect resources towards savings. The pie charts were sent with the following message:

> "Hello. We send you this graph with the detail of your expenses in the past two weeks. It is essential to know what we are spending on and to have some reasonable control over our financial lives. This graph is the first step in building a budget that can help you meet your needs and could guide you in deciding whether to spend less on some items and thus save more. During our next meeting, we can comment and discuss any questions you have about the graph. See you soon!"

B.2   Characteristics of the financial literacy test

Table B.1: **IRT analysis on baseline financial literacy test**

Subscore	Item	Difficulty Parameter - Control	Discriminatory Parameter - Control	Difficulty Parameter - Treatment	Discriminatory Parameter - Treatment
Financial Literacy	3	-1.349***	1.585***	-1.421***	1.356***
	5	-0.353***	0.825***	-0.551***	1.125***
	7	0.261	0.746***	-0.570***	0.996***
	14	-1.943***	0.580***	-3.500***	0.444***

NOTE: table shows the baseline test questions with the highest degree of difficulty and discrimination based on an IRT estimate using a two-parameter model. Significance levels * 10%, ** 5%, *** 1%.

Table B.2: **Financial literacy/proficiency tests topics**

General Topics	This paper	Bruhn et al. (2016)	Frisancho (2021)	Batty et al. (2015)	Hinojosa et al. (2009)
Account balance	X	X	X	X	
Basic financial computations	X	X	X	X	X
Budgeting	X	X	X	X	
Basic economics concepts	X	X	X	X	X
Credit cards	X	X	X		
Family´s financial well-being		X			
Financial consumer's rights			X		
Investment strategies	X	X	X		X
Savings strategies	X	X	X		
Owning and renting	X	X	X	X	
Purchasing strategies	X	X			

SOURCE: own elaboration.