Is School-Based Financial Education Effective? Short and Long-Term Impacts on Students, Parents, and Teachers*

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Abstract

This paper studies the potential of school-based financial education. Relying on a large-scale experiment in Peru and data from almost 20,000 high school students, the study identifies significant improvements on students' financial skills. Novel credit bureau data uncovers long-lasting effects on financial behavior: three years later, treated students reduce their delinquency rates. Teachers accrue financial literacy gains that double those identified among students and they become more likely to save, particularly through formal channels. Two years after the intervention, teachers borrow more from banks and reduce their delinquency rates, while parents transition away from expensive sources of credit.

Keywords: Financial Education, Youth, Self-Control, Savings, Credit records, Treatment Effects, Long-term impacts

JEL Codes: D14, D91, J24, O16

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1 Introduction

Financial education tends to trigger heated debates in academic and policy forums. After the financial crisis, it has been enthusiastically and intensely promoted as a means to foster financial stability. Recently, under the premise that they will face increasing financial risks and deal with more complex financial systems than previous generations, youth has become one of the priority targets in the arena of financial education [OECD/INFE, 2015; OECD, 2014]. By 2017, more than 70 countries were developing or implementing a national strategy on financial education, which often includes the introduction of the content in the school curricula [OECD, 2017].

Despite the evidence supporting a link between financial literacy and household wealth and economic outcomes [Behrman et al., 2012; Lusardi and Mitchell, 2014; Lusardi et al., 2017], many are skeptical that financial education programs can effectively improve financial skills. The increased availability of experimental studies in the school setting supports large and robust impacts on financial literacy [Batty et al., 2015; Bover et al., 2018; Bruhn et al., 2016] and preferences,[1] but the ability of these programs to yield long-lasting effects on financial behavior is still under scrutiny. Recent studies exploiting quasi-experimental variation in college course requirements in the United States identify a positive effect of financial education on long-term financial behavior [Bernheim et al., 2001; Brown et al., 2016; Cole et al., 2016; Urban et al., 2018; Urban and Stoddard, forthcoming], yet no experimental study has yet to track students over time and measure the long-term effects of school-based financial education.

This paper is timely, as it contributes to the ongoing debate on the effectiveness of financial education. Exploiting experimental variation in the delivery of mandatory personal finance lessons in Peru, the study measures the short-term and long-term impacts of school-based financial education on high-school students' financial skills and behavior. Moreover, this paper extends the focus on the direct beneficiaries of the program and measures its impact on the financial behavior of parents and teachers. This is a novel and important contribution, as these potential gains may alter the estimated effectiveness of these programs.

This paper relies on data from a large-scale randomized controlled trial (RCT) implemented in 300 public high schools, targeting grades nine through 11. The treatment was randomized at the school level and consisted of the delivery of financial education lessons

[1] Luhrmann et al. [2018] measure the impact of a financial education program on time inconsistency among German students. Jamison et al. [2014] consider the impact of school-based financial education on discounting, self-control, and risk tolerance while Bover et al. [2018] analyze the degree of students' patience through an incentivized task. Recently, Alan and Ertac [2018] evaluated the impact of an intervention specifically designed to improve third and fourth graders' ability to imagine their future selves and encourage forward-looking behavior.

during the school day. The instructors in charge of the lessons were school teachers who were trained in the materials. Therefore, teachers are treated both directly through the training they receive as well as indirectly when delivering the lessons in the classroom. Parents or guardians were not specifically targeted by the intervention but were exposed to the content both through homework and interactions with their children.

Two key advantages of this study are large sample sizes (approximately 20,000 students and 15,000 parents) and access to several data sources, including self-reported survey data as well as consecutive rounds of high-stakes administrative records. Students were tested on their financial knowledge and surveyed both at baseline and at the end of the school year. Survey data in both rounds included questions on personality traits related to financial behavior as well as on shopping and saving habits. Teachers' data came from an exit financial knowledge exam and an exit survey covering questions on financial attitudes and behavior. Access to administrative records provides information on students' cumulative grade point averages (GPAs) in three consecutive academic years (2015 through 2017). Furthermore, credit bureau data gathered between December 2017 and June 2019 provides information on access to credit and delinquency rates for students, parents, and teachers. While the short-term impacts of the program rely on exam and survey data, long-term impacts on financial behavior rely on high-stakes credit bureau records.

A simple framework of endogenous financial knowledge accumulation predicts that the financial education program will be effective among students and teachers due to the free-provision of the lessons, as well as its mandatory nature, which overcomes selection and participation issues generated by biased perceptions on the expected returns of the investment. Parents, on the other hand, only face a reduction in the price of acquiring financial skills by interacting with their children; however, even in the presence of spillovers, they are still potentially subject to participation constraints. Consequently, the impact on parental behavior is expected to be modest relative to that experienced by teachers.

The program had strong effects on the financial knowledge of young students. Relative to the control group, scores in the financial literacy exit exam increase by 0.16 SD. This effect is large when compared to voluntary after-school programs [Berry et al., 2018; Jamison et al., 2014] and in line with similar school-based interventions [Bruhn et al., 2016; Bover et al., 2018]. The impact of the program is equivalent to a 16-point improvement in the 2015 PISA financial literacy assessment of 15-year-old students, relative to the country's average score of 403 points. The introduction of financial education lessons did not hinder performance in other courses and had no effect on grade progression in neither the short-term nor the long-term. These results prove that the time diverted away from other courses and into personal finances did not jeopardize academic achievement.

Although not specifically intended, the intervention modestly improved the level of self-control, as measured by Tangney et al. [2004]'s scale. The treatment also led to improvements in students' shopping habits but the probability to save remained unaltered. By June 2018, almost two years after the intervention, no significant effect is identified in terms of credit behavior. This is not surprising, as only 41% of the students were legal adults who could actively participate in the financial system by that date. By June 2019, three years after the pilot's launching, this share was up to 71% and long-lasting effects are identified. Treated students experience a 6% drop in delinquency rates, as they become less likely to have records in the credit bureau due to unpaid/delinquent bills or credit card statements.

Although the treatment did not target parents directly, interaction within the household with their teenage children may facilitate the transmission of personal finance knowledge, leading to changes in financial behavior. Indeed, two years after the intervention, parents in the treatment group seem to transition away from more expensive sources of credit.

The effect of the program on teachers' knowledge and behavior is quite impressive. Getting trained and imparting the financial education lessons improved teachers' financial skills by 0.32 SD, which is twice as large as that identified among students. Teachers in the treatment group also recorded a 10% increase in the probability to save, with a disproportionate preference for formal mechanisms over informal mechanisms: the probability to save formally increases by 22%, vis-a-vis a 10% increase in the likelihood to save informally. Administrative credit bureau records two years after the launch of the intervention further reveal significant effects on teachers' credit behavior: the probability that teachers in the treatment group obtain formal bank loans increases by 13% and their delinquency rate decreases by 15%. These effects are still present by the third year after the intervention. Heterogeneous treatment effects reveal that the impact on teachers is concentrated among those who covered a greater share of the financial education curriculum.

The results suggest that the provision of financial education in school has great potential, due to its direct and multiplying effect on adults close to the targeted youth. At a cost per student of US$ 4.8, the program yields a very low cost-effectiveness ratio in terms of students' financial skills. The impact recorded on adults' financial behavior further contributes to the cost-effectiveness of the program. This study also identifies long-lasting effects on children's credit behavior. This is an important result since it is the first piece of experimental evidence supporting the ability of these programs to influence financial decisions during adulthood. All in all, this paper shows that school-based financial education is effective, has low delivery and opportunity costs (in terms of academic outcomes), and can shape youth's financial choices once out of school. These results clearly support the inclusion of financial education in the school curriculum and validate the role of financial education as a complement to

financial inclusion efforts. As access to formal financial services rapidly increases, take-up and usage rates lag behind [Karlan et al., 2016]. Improving consumers' financial knowledge and capabilities may help bridge the gap between underserved population segments and formal financial institutions.

This paper contributes to the literature on the effectiveness of financial literacy programs for youth. The two papers most closely related to this study are Bruhn et al. [2016] and Bover et al. [2018], which also exploit large-scale RCTs targeting high school students to quantify the impact of school-based financial education in Brazil and Spain, respectively. Bruhn et al. [2016] also identified reverse inter-generational knowledge transfers from children to parents, but their intervention directly targeted parents in the treatment group through a workshop designed to raise awareness about the importance of financial education. Another related paper is Luhrmann et al. [2018], which evaluates the effect of school-based financial education on German adolescents, focusing on the impact on time preferences.

This study differs from previous papers on the effectiveness of school-based financial education in two important ways. First, it relies on high-stakes data to measure the impact of financial education on financial behavior, both in the short and the long-run. This paper poses an advantage relative to other studies by complementing survey data with individual-level administrative academic and credit bureau records.[2] Academic records are crucial to measure the program's opportunity cost in terms of grades and passing rates. Credit bureau records provide reliable data to measure long-lasting changes in financial behavior due to the delivery of school-based financial education.

Second, this paper is the first to focus on the impact of financial education on the instructors delivering the training. The significant impact on financial skills identified among teachers and the heterogeneous effects by the rate of coverage of the lessons is informative for the design of adult financial education programs. Adults may need learning strategies that incorporate constant reinforcement and repetition to successfully alter their financial literacy and behavior. Moreover, this is also the first paper that studies the spillover effects of school-based financial education on parents when they are not directly targeted.

2 Experimental Design

2.1 The Intervention

In 2015, the Peruvian government launched the National Financial Inclusion Strategy, which included, as a high-priority goal, the provision of school-based financial education to all pri-

[2]Bruhn et al. [2016] collect passing rates from school records but only aggregated at the grade-level.

mary and secondary students by 2021. In this context, the Ministry of Education (MINEDU) partnered with the Superintendency of Banks and Insurance (SBS) and the Center of Studies (CEFI) of the Peruvian Association of Banks to develop a pilot to provide financial education to high school students. Together, they developed student workbooks for each of the last three high school grades (equivalent to ninth, tenth, and eleventh grades in the United States) as well as a teacher's guide. The team also designed and implemented a 20-hour teacher training plan divided into five sessions, which included a training component on the financial literacy contents (four sessions) as well as a pedagogical one (one session).[3] MINEDU encouraged teachers to attend the training sessions and school principals were requested to facilitate teacher participation in the training. Participants received both a transport subsidy (mostly in kind) and a full meal during the workshop.

The content of the workbooks varies by grade and it is fully detailed in Table A.1. The lessons provided to ninth-graders focused on the differences between needs and resources and budgeting. The lessons imparted to tenth graders focused on financial products and services and forward-looking choices. The curriculum for eleventh-graders covered responsible financial consumers and access to information in financial markets.

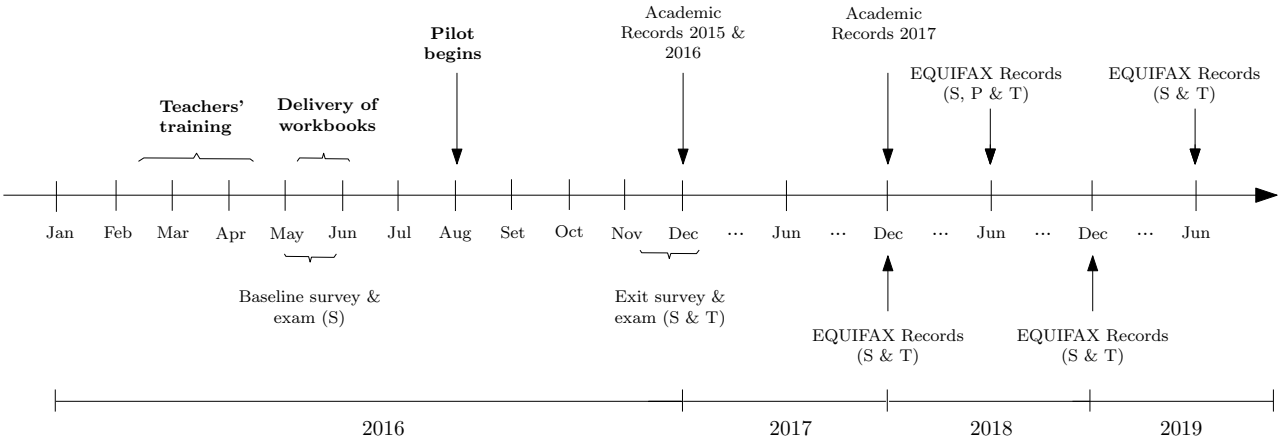
The sessions were delivered during the regular classes of the course "History, Geography, and Economics" (HGE). Teachers of HGE were instructed to incorporate the material in the Economics portion of the course. Since the content was not incorporated as a stand-alone course in the official curriculum, teachers were not bound to teach the material. However, once the lessons were introduced into a regular course, the content became subject to performance evaluation and was considered high-stakes by the students.

2.2 Study Timeline

Figure 1 organizes the intervention activities that took place during the 2016 calendar year (in bold) as well as the evaluation activities that were carried out between 2016 and 2019. Teachers' training workshops were conducted between mid-February and March, before the beginning of the school year. Additional replica sessions conducted by trained teachers were organized during the first month of classes to extend coverage of the training. The distribution of students' workbooks to schools started in May but the delivery of the sessions in class began during the second half of the 2016 school cycle; August through December. To ensure that compliance levels were high, regular monitoring phone calls took place September through November.

[3]The content of the pedagogical session included a review of the background of the program as well as the use of teaching tools such as charts, figures, and case studies.

Figure 1: Study Timeline



NOTE: Data collection activities may refer to the sample of students (S), parents (P), and/or teachers (T).

Self-administered students' baseline surveys and financial literacy entry exams were simultaneously collected during May. Exit surveys and exams for students and teachers were applied toward the end of the academic year (Nov-Dec).[4] Individual-level data on grades and passing rates for the academic years of 2015 and 2016 were requested at the end of 2016, while the records for 2017 were accessed at the end of that academic year. Credit bureau data was obtained from EQUIFAX, the leading private credit bureau in Peru. Their records capture a snapshot of clients' credit standing and allow us to observe loan balances by default status and type of lender. These data also include negative records due to late or skipped payments of loans, service bills (electricity, water, cell phone, personal taxes, etc.), and credit cards (mainly from department stores in the case of youth). EQUIFAX data were collected in December 2017, June 2018, December 2018, and June 2019. Due to budgetary restrictions, credit records of students' parents were only obtained in June 2018.

2.3 Data and Measurement

Exam and survey data were only collected for students and teachers. Within each school in the experimental sample, one classroom from each targeted grade was chosen at random. Students from that group were interviewed and tested at baseline and endline while the corresponding HGE teacher only took the exit exam and survey. In total, 900 classrooms (∼20,000 students and 453 teachers) comprise the main study sample.

[4]All data collection efforts were conducted once the Chesapeake Institutional Review Board (IRB) determined that the evaluation activities were exempt from IRB oversight.

Students' baseline survey collects basic information on socioeconomic characteristics of the household, students' future aspirations, parental supervision, truancy, and the number of hours the student works per week. Additionally, the questionnaire gathers information on five personality constructs and preferences that may influence financial choices: conscientiousness, self-control, intertemporal preferences, impulsiveness, and risk aversion.[5] The survey also measures students' school engagement[6] and collects data on previous exposure to financial education programs and pro-saving behavior [Mandell and Klein, 2009]. The survey measured financial behavior on several fronts: formal savings, budgeting, participation in household financial decisions, consumption and saving habits, and financial autonomy.[7] The baseline survey also collected the names, last names, and cell phone numbers of the student's father or guardian. The instrument used at endline was exactly the same as the one used at baseline, with the exclusion of the questions related to socioeconomic characteristics. Teachers were only tested and surveyed at endline. The survey questionnaire applied to them was very similar to the students' instrument, but additional questions were added to capture their professional background and experience, as well as their formal and informal savings holdings. Teachers in the treatment group also completed an additional survey module that questioned them about their progress with the financial education material in the classroom.

Students' financial literacy exams were grade-specific and consisted of 15 questions. Four questions on the topics of risk, return and liquidity, intertemporal spending choices, budgeting to save, and the importance of investing in skills and education were drawn from the 2008 National Jump$tart Coalition Survey of High School Seniors and College Students.[8] The remaining questions tested students on the topics covered in each grade-specific workbook. Most questions were drawn from a teacher entry exam designed by the SBS and CEFI,[9] but a few were developed by the author to cover all topics included in the workbooks. The same grade-specific exam was administered at baseline and endline. The psychometric properties

[5]Conscientiousness, which is closely related to deliberative thinking, was measured using the Big Five Scale for this attribute [Pervin and John, 1999]. Self-control is measured by Tangney et al. [2004]'s scale, while impulsiveness is measured by the Barratt Impulsiveness Scale (BIS-11) [Orozco-Cabal et al., 2010]. Time inconsistency is defined as in Ashraf et al. [2006]. These preferences and personality traits are measured relying on extensively tested scales that are specifically designed to be self-rated.

[6]The scale to measure student engagement comes from the Student Engagement in Schools Questionnaire and measures behavioral engagement: effort and persistence [Hart et al., 2011].

[7]The financial autonomy scale was borrowed from Bruhn et al. [2016].

[8]See Mandell [2009]. The Jump$tart Coalition for Personal Financial Literacy is a U.S. nonprofit coalition of 150 organizations that works to promote financial literacy among students. Its target population includes students between pre-kindergarten and college. Jump$tart publishes the National Standards in K-12 Personal Finance Education, which delineate the personal finance knowledge and ability that young people should acquire during their schooling years between kindergarten and twelfth grade. Since 2000, Jump$tart has administered the Survey of Personal Financial Literacy among high school students.

[9]SBS and CEFI developed an entry exam but it was only taken by teachers in the treatment group who attended at least one of the training sessions.

of the exam based on students' baseline data are presented in Table A.2. The exit exam taken by teachers was developed by the author and included the four questions from the Jump$tart questionnaire as well as questions from the students' exams for ninth grade (4), tenth grade (4), and eleventh grade (3).

MINEDU's academic records provide data for all high school students enrolled in any of the 300 schools of the sample in 2016. This data contains individual-level information on cumulative grades by course and grade progression at the end of three consecutive academic years; 2015 through 2017.[10] Access to these records offers the possibility to estimate treatment effects on academic outcomes among students in the survey sample as well as among the universe of students in the experimental sample of schools (∼60,000 at baseline).

Credit outcomes up to three years after the intervention were provided by EQUIFAX, a private credit bureau that concentrates loans data from almost all lenders in the Peruvian credit market.[11] EQUIFAX's data captures an individual's credit standing at the time in which he or she is searched, both in terms of their positive and negative records. Loan records report the current balance by default status and source of the funds, distinguishing between loans from banks and financial institutions supervised by the SBS from those that come from formal but non-regulated institutions such as microfinance NGOs and cooperatives. Even though the latter are not really informal lenders, they tend to relax the minimum requirements to get a loan at the cost of higher interest rates when compared to banks. In addition to loan balances, the credit bureau's data also captures late or skipped payments of service bills and credit cards. Individuals in the credit bureau's database can therefore have a credit history due to outstanding loan balances and/or delinquent behavior within the financial system or with other providers of services. For instance, youth will be less likely to borrow due to age constraints, but they could still obtain a (negative) credit history due to unpaid service bills or credit card statements.

The search in EQUIFAX's records relied on an algorithm that matched students, teachers, and parents based on names and last names and/or national identification documents. EQUIFAX's data is collected at four dates, every half-year between December 2017 and June 2019, for all students and teachers in the baseline and endline samples. The credit records of students' parents were only retrieved in June 2018. The construction of the outcome variables for students and teachers takes advantage of the availability of successive rounds of data. For example, the indicator variable of having bank credit by June 2018 is coded as one

[10] All grades are normalized by school quality to make them comparable across schools. See Appendix B for more details on the normalization implemented.

[11] EQUIFAX collects credit information from all regulated financial institutions and most of the non-regulated lenders in the market. Although non-regulated lenders are not mandated to share their borrowers' credit records, EQUIFAX is the only firm in Peru that includes over 90% of them in its records.

when the individual was reported to the credit bureau by a bank either in December 2017 or June 2018. Credit outcomes will try to capture an individual's likelihood to have a credit history (irrespective of its origin) and have a loan, as well as his or her delinquency rate. Access to credit will be distinguished by the quality of the loan, where quality is proxied by the source of the loan: conditional on access, banks are considered superior to microfinance lenders since they charge relatively lower interest rates.

2.4 Sample Selection and Randomization

The universe of interest was restricted to full-day public high schools in urban areas in six regions of the country: Lima and Callao, Arequipa, Piura, Junin, Puno, and San Martin. Due to logistics, the universe was further limited to schools that are close to cities. After imposing some additional restrictions (directly managed by the MINEDU, single-grade schools, and number of students by grade above the fifth percentile and below the 95th percentile), the final universe included 308 schools.[12]

The restricted universe was stratified by region. Following Bruhn and McKenzie [2009] and Bruhn et al. [2016], schools are paired by their similarity within each of the six strata.[13] This procedure returns 150 matched pairs, yielding an experimental sample of 300 schools. Within each pair, schools were randomly assigned to either the control or the treatment group. The spatial distribution of control and treatment schools is plotted in Figure A.1.

Tables A.3 and A.4 in Appendix A provide basic descriptive statistics at the student and teacher level, as well as balancing tests of the randomization. Consistent with the random treatment assignment, very few significant differences are detected across groups. In any case, the analysis will include background controls as well as initial levels of the dependent variable whenever available.

Provided that non-compliance and non-response are orthogonal to the magnitude of the treatment impact, the experimental design is robust to the exclusion of pairs in which at least one school does not comply with the treatment assignment and/or has incomplete records. Indeed, one pair of schools from the original sample is excluded from the analysis due to non-response in the endline exam and survey.[14] The final experimental sample thus consists

[12]To establish the number of schools required for the evaluation, power calculations were performed with the following parameters: significance level of 0.05, statistical power of 0.8, minimum detectable effect of 0.1SD, R^2 of the outcome equation of 0.1, intra-cluster correlation of 0.1, and a sample size of 40 students per grade. Under these assumptions, 300 schools were required, 150 in each treatment arm.

[13]The Mahalanobis' distance is minimized for 10 selected characteristics: electricity connection; water and drainage services availability; presence of a principal; number of desks in good condition; number of teachers; number of students in ninth, tenth, and eleventh grades; dropout rate; passing rate; and whether the school belongs to the experimental sample of any other ongoing pilot.

[14]Additionally, another school refused to participate in the baseline survey. After removing the two pairs

of 298 schools, with a total population of approximately 60,000 students. Baseline records are available for 20,641 students (7,008; 6,845; and 6,788 in ninth, tenth, and eleventh grade, respectively), roughly a third of the universe of interest. The exit survey and exam were applied to 19,487 students (6,634; 6,496; and 6,357 in ninth, tenth, and eleventh grade, respectively). Since the survey questionnaires were self-rated, higher levels of missing data are expected relative to face-to-face application through a surveyor. As shown in Table A.6, the share of missing records varies depending on the construct and the survey round; however, it is not significantly different by treatment arm (with the exception of one in 20 variables).

Only 17,215 students were present in both data collection rounds. The attrition rate between baseline and endline is 17%, but it is not differential by treatment status (see Table A.7). The sample of interest to evaluate the impact of the intervention includes all students with records in the follow-up survey and exam. When this sample is matched with performance records from 2016, the same academic year in which the intervention took place, the success rate is extremely high at 98%. The share of students that matched with 2015 academic records is still high, at 91%. Focusing on ninth and tenth graders who were still in school in 2017, the match rate is 87%.

Matching students with EQUIFAX's credit bureau data naturally yields a much lower success rate. Legal adulthood in Peru begins at age 18 and youth often face borrowing constraints due to low labor participation rates and income levels. Indeed, in 2017, 15% of people at age 15 and above in Peru borrowed money from a financial institution; however, this percentage was only 8% in the 15 to 24 age bracket [Demirguc-Kunt et al., 2015]. By June 2018, only 7,850 students in the follow-up sample were legal adults and the match rate in the control group was 23%. By June 2019, 13,714 students reached legal adulthood and the match rate with credit records in the control group increased to 30%.

The final sample of teachers consists of 453 individuals who were tested and surveyed at endline. Virtually all of them are matched with the EQUIFAX database (98% by June 2018 and 96% by June 2019). The sample of parents is drawn from the students' self-report of their names, amounting to 14,709 observations in the endline. Although the quality of the match with EQUIFAX data is inferior due to lack of information on their national identification document, the success rate is still high at 75%. The quarter of parents that remain unmatched may correspond to problems with the merge or may reflect lack of records in the financial system.

of schools with at least one school without survey records in at least one of the rounds, Table A.5 tests if the response rate in the student survey significantly differs by treatment assignment at the classroom level. Response rates are quite high at 87% and no significant differences by treatment assignment are identified.

2.5 Teachers' Compliance with the Treatment

Teachers were encouraged to attend the training sessions offered to them and to deliver the material in the classroom. However, the MINEDU could not impose either of these activities as mandatory. Despite these conditions, teachers' engagement with the pilot appears high. On one hand, about 73% of teachers in the treatment group attended at least one training session and 43% had perfect attendance. On the other hand, most teachers also complied with teaching the financial education material in the classroom. Teachers' self-report of their progress in the endline survey shows that 48% of the HGE teachers in the treatment group reported that they had taught all the lessons and 21% had covered part of the material. Only a third of the teachers report not teaching any lessons of the workbook. In the sub-sample of treatment schools in which at least one teacher attended all sessions, 55% of the teachers covered all the lessons. Due to partial compliance with the treatment, the measured impact of the pilot should be interpreted as a lower bound of the inclusion of the content in the regular school curriculum.

Teachers were instructed to include the financial education material in the Economics portion of the HGE class. However, they were not offered additional guidelines to adjust the time allocation to other topics covered in the course. Survey data reveals that, on average, teachers chose to incorporate the new material by significantly reducing the time allocated to teach history, politics, and world news, while leaving the time allotted to economics unchanged. This may respond to potential synergies between the economics portion of the course and the financial education material recognized by the teachers.

3 Framework and Estimation

3.1 Framework

Investment in financial literacy bears both costs and benefits that are differentially distributed over time. On one hand, consumers with a high stock of financial skills have access to investment opportunities with higher returns. On the other hand, acquiring financial skills is a costly investment, not only in terms of the pecuniary costs it imposes, but also due to the time diverted away from other productive activities. While the opportunity costs and the monetary costs of financial education are easily observed, consumers may face information gaps about the expected returns of financial skills.

Relying on the simple two-period model developed in Jappelli and Padula [2013], this section illustrates how this intertemporal trade-off leads to different optimal choices of investment in financial literacy depending on the (perceived) returns from financial savviness

and the price of acquiring financial knowledge. The model rationalizes the lack of success of voluntary financial education programs, both in terms of take-up rates and impact on financial literacy levels. In addition, the model is useful in generating predictions about the impact of the school-based mandatory program on students, parents, and teachers.

Consumers experience two periods throughout life. They are productive and earn income y in the first period ($t = 0$) and retire in the second ($t = 1$). At the beginning of their lives, they are endowed with an initial stock of financial literacy, Φ_0, that depreciates at a rate δ. Returns to assets, R, are increasing in the stock of financial skills:

$$R(\Phi_1) = \Phi_1^{\alpha}$$

where α is the elasticity of the interest factor with respect to financial skills in the retirement period. Investment in financial literacy is costly: consumers pay price p in period 0 to increase their stock of financial skills in period 1.

Consumers choose optimal levels of savings (s) and investment in financial literacy (ϕ) in period 0 to maximize log utility:

$$\ln c_0 + \beta \ln c_1$$

subject to $c_0 + s + p\phi = y$ and $c_1 = \Phi_1^{\alpha} s$, where β is the discount factor. The optimal level of ϕ is obtained when the marginal return equals p. Thus, the reduced form for optimal investment in financial literacy is given by:

$$\phi^* = \frac{1}{1 + \beta + \alpha\beta} \left[\frac{\alpha\beta y}{p} - \Phi_0(1 - \delta)(1 + \beta) \right]$$

Agents may underinvest whenever they have biased perceptions about the returns to financial skills in period 0 such that $E(\alpha|\Phi_0) < \alpha$. Whenever initial endowments are low in a population and/or access to financial education provision is limited, agents may be unable to accurately estimate the returns of investing in financial education. In these settings, the supply of such programs resembles the introduction of a new service with unknown attributes in the market: if there is missing information on a product, consumers will discount the attractiveness of the new option and weigh more heavily common attributes [Kivetz and Simonson, 2000].

Policy efforts to increase financial skills by providing voluntary free financial education reduce p by setting the pecuniary costs to 0 and reducing opportunity costs:

$$\frac{\partial \phi^*}{\partial p} = -\frac{\alpha\beta y}{(1+\beta+\alpha\beta)p^2} \tag{1}$$

Equation (1) shows the impact that price reductions will have on ϕ^* is increasing in $E(\alpha|\Phi_0)$. In settings with low initial levels of Φ_0 and, consequently, low levels of $E(\alpha|\Phi_0)$, voluntary programs will not be effective to increase financial skills. Take-up of such programs will be low and will come mostly from agents with high Φ_0.[15] Moreover, since agents who self-select into the program have greater initial endowments, they will also register modest impacts on financial literacy as their optimal level of ϕ^* will be lower.

Misperceptions about the value of financial literacy are expected to be stronger among young adults, who have low initial endowments as well as little experience with money and the financial system. This may explain the meager impacts yielded by voluntary financial education programs for youth, as reported in Berry et al. [2018] and Jamison et al. [2014]. Consistent with the model, take-up data from Berry et al. [2018] show that the demand for a voluntary financial education program offered to youth in Uganda was greater among those who were more financially literate, as well as those who saved and spent more at baseline.

School-based financial education programs are thus expected to be effective in increasing the level of financial literacy due to its mandatory nature [Willis, 2011]. These programs are able to overcome selection and participation issues due to low $E(\alpha|\Phi_0)$, while reducing the price of acquiring financial skills. By making the content available to everyone, school-based financial education programs can reach students with lower levels of Φ_0. These students would not have the demanded program under a voluntary scheme, but are predicted to have higher optimal levels of investment in financial skills when p goes down.

This simple model thus predicts that the financial education program delivered in secondary schools in Peru will lead to large treatment impacts among students, particularly relative to voluntary youth programs. Although teachers were not required to attend the training workshops or teach the lessons, they were highly incentivized by the Ministry, the school principals, and the implementation team (see Sub-Section 2.5). This changes the compulsory nature of the intervention relative to a voluntary adult program. Therefore, the

[15] Bruhn et al. [2013] confirm very low participation rates in an experiment with adults in Mexico and evaluate different strategies aimed at increasing take-up rates. They find that monetary incentives are effective in fostering participation. The authors also tried to increase the perception of α by sending potential participants a video compact disc containing positive testimonials from people who had previously attended the financial education course. Unsurprisingly, this treatment was not effective in increasing participation. On one hand, it is very hard to effectively change perceptions and the video may just be a light-touch incentive. On the other hand, even if the message delivered was strong enough, there is no way to enforce compliance with the video treatment.

model also predicts large treatment impacts on financial literacy among teachers. Parents, on the other hand, only face a potential reduction in p due to increased accessibility to financial education at home. However, this is a light-touch treatment and their exposure depends on the degree of spillover from their children and parental perceptions about the returns on the investment. Relative to students and teachers, parents are thus expected to experience muted treatment impacts.

3.2 Empirical Specification

The impact of the financial education program on different outcomes is measured as the difference across treatment arms, captured from an intention-to-treat, OLS regression:

$$y_{ijp} = \alpha + \beta T_{jp} + \gamma y_{ijp}^{\text{pre}} + \delta X_{ijp} + \sum_{p} \theta_p d_{jp} + \epsilon_{ijp}$$

where y_{ijp} could be financial knowledge, socioemotional traits, preferences, or financial behavior of student/parent/teacher i in school j from pair p. The regressor y_{ijp}^{pre}, the baseline value of y_{ijp}, is included when evaluating students' financial literacy, academic performance, and self-reported outcomes in the surveys. The impact of the treatment is measured by β, the coefficient on the indicator of treatment status, T_{jp}. All regressions include additional individual and background characteristics as controls, X_{ijp}, and a set of dummies, d_{jp}, identifying the pair of schools matched.

The intervention did not have perfect compliance levels within the treatment group (see sub-section 2.5). Non-compliance was one-sided as teachers in the control group did not attend the training workshops and students from the control group did not receive the lessons or the workbooks. Estimation of the average treatment effect on the treated (ATT) is feasible due to the availability of attendance data for the teachers' training workshops at the school level. This level of compliance is more appropriate than compliance at the student/classroom level: teachers' attendance to the training provides a primary measure of compliance since they need to be trained in order to deliver the content in class.[16] Alternatively, teachers' self-report of the coverage of lessons (see Section 2.5) could be used to measure compliance in the classroom. However, this report may be biased since teachers may have an incentive to over-report their progress due to social desirability bias.

For each grade, the preferred compliance measure is defined at the school level by Z_{jp}, which equals one if at least one of the teachers attended one or more of the training sessions. Average treatment on the treated (ATT) effects can then be obtained from estimating β^{TOT}

[16] Measuring compliance at the student level demands access to records on students' lesson attendance and these were not collected.

by instrumenting Z_{jp} with the random assignment of the treatment:

$$y_{ijp} = \alpha + \beta^{\text{TOT}} Z_{jp} + \gamma y_{ijp}^{\text{pre}} + \delta X_{ijp} + \sum_{p} \theta_p d_{jp} + \epsilon_{ijp}$$

4 Results

4.1 Treatment Impacts on Students and Their Parents

Table 1 presents the immediate effects of the school-based financial education program on financial literacy at the end of the implementation year. In general, the program was extremely effective in improving high school students' financial knowledge, as presented in the first column. The treatment increases scores in the exit financial literacy exam by 0.16 SD relative to the control group. These average gains are closely aligned to those reported by similar programs that also target high school students in Brazil [Bruhn et al., 2016] and Spain [Bover et al., 2018].[17] They are also comparable to those identified in Batty et al. [2017], who implemented an experiential intervention among primary students in the United States.

One recurring argument against the introduction of financial education lessons in the school setting is the substitution of time and resources away from other courses, potentially sacrificing student learning in other areas. Columns 2 through 5 in Table 1 suggest that the opportunity cost of introducing personal finance content is not high enough to hinder academic performance in other courses. Columns 2 and 4 show that the treatment has no significant effect on cumulative grades, neither in the short-term nor in the medium run. If anything, the personal finance lessons slightly boost language performance by 0.03 SD (see Table A.8 in Appendix A). The intervention does not yield unintended perverse effects on dropout either (columns 3 and 5). This result is in contrast with Bjorvatn et al. [2015], who identify negative effects on school retention. However, this could be explained by their focus on entrepreneurship in the curriculum imparted, which is absent in the Peruvian case.

The lack of impact on GPAs and grade progression is confirmed when the impact is estimated in the universe of students with academic records (see Tables A.9 and A.10 in Appendix A). One may argue that the content of the financial education program may complement some of the material taught in regular courses or that the innovative content can boost other dimensions of student engagement such as the affective component (liking learning and/or school). However, the intervention did not trigger these indirect effects:

[17] The treatment impacts are below those reported by Becchetti and Pisani [2012] but are more precisely estimated due to the availability of a much larger sample of students.

Table 1: ITT Effects on Students' Financial Knowledge and Academic Outcomes

		Academic Outcomes			
	Financial	2016		2017	
	Literacy	GPA	Pr(Promoted)	GPA	Pr(Promoted)
	(1)	(2)	(3)	(4)	(5)
Treatment	0.157***	-0.014	0.002	0.006	0.005
	[0.023]	[0.014]	[0.009]	[0.015]	[0.009]
Number of Observations	19487	19054	18574	11498	11703
Number of schools	296	296	296	296	296
Mean in Control	-0.01	-0.01	0.81	-0.02	0.80
R-squared	0.23	0.86	0.08	0.83	0.07

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for academic outcomes, GPA and Pr(Promoted). Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents each day of the week. In the case of financial literacy and GPAs, the value of the dependent variable at baseline is also included as a control.

student effort, measured either as attendance or student engagement, was not affected by the treatment (see Table A.11).

Teaching financial skills entails covering constructs such as opportunity cost and intertemporal trade-offs. This content may impact individual time preferences or patience-related attributes. This is particularly important since we still know little about the malleability of soft-skills and preferences and the extent to which environment can shape them.

Columns 1 and 2 in Table 2 present the treatment impacts on self control and the probability of having hyperbolic time preferences, respectively. The treatment successfully fosters the development of self-control skills with an average effect of 0.03 SD. Although this impact seems modest and does not survive multiple hypothesis testing, it is still an important finding. The intervention was not specifically designed to target this outcome, but it was still able to impact self-control, which is a soft skill linked to better financial and health outcomes [Moffitt et al., 2011; Gathergood, 2012; Strömbäck et al., 2018]. Developing lessons with more specific content to encourage the development of these traits may prove effective among youth. Additional results for conscientiousness, impulsiveness, as well as on the probability of being risk averse, are presented in Table A.12 in Appendix A. The program does not lead to significant effects on any of these traits.

Even though current financial choices of youth are quite limited, they still manage a

Table 2: ITT Effects on Students' Socioemotional Skills, Preferences, and Consumption Habits

	Soft Skills & Preferences		Consumption		
	Self-Control	Pr(Hyperbolic)	Pr(Budgeting)	Shopping Habits Index	Financial Autonomy
	(1)	(2)	(3)	(4)	(5)
Treatment	0.032**	-0.000	0.006	0.060***	0.016
	[0.016]	[0.005]	[0.007]	[0.016]	[0.015]
Number of Observations	17220	13193	15672	15928	16696
Number of schools	296	296	296	296	296
Mean in Control	0.00	0.16	0.64	0.00	-0.01
R-squared	0.21	0.02	0.06	0.05	0.16

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for socioemotional skills and preferences (including outcomes in this table and the ones in columns 1-3 in Table A.12) and shopping habits, separately. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, has dinner with parents all days of the week, and the value of dependent variable at baseline.

budget and make shopping decisions that can be shaped by the treatment. Indeed, column 4 in Table 2 shows that the intervention has a sizable and significant impact on students' shopping habits. On average, shopping behavior in the treatment group improves by 0.06SD in terms of an index that measures if they compare prices before shopping, save instead of borrowing to buy something they cannot afford, and bargain before shopping. Column 4 in Table A.12 in Appendix A reports that the probability to have a savings account is not significantly affected by the treatment.

Despite increasing evidence confirming the positive effects of school-based financial education on financial literacy and habits in the short-term (see [Kaiser and Menkhoff, 2019] for a meta-analysis and Frisancho [2019] for a survey on experimental studies), critics of these programs often argue that these effects may not be sustained along the life cycle. Learning depreciates and financial systems quickly change, which may render the effects of financial education irrelevant once youth reach adulthood and start to make financial choices [Willis, 2011]. Recent studies in the United States take advantage of non-experimental changes in graduation requirements to complete financial education courses [Brown et al., 2016] or variation across states in the enactment of these requirements [Urban et al., 2018] to evaluate the impact of financial education on credit behavior after graduating from high school. Relying

on consumer credit reports from Equifax in the United States, these studies identify a positive impact of financial education on credit behavior, both in terms of improved repayment and a reduced reliance on non-student debt.

These are well-executed and promising contributions, but they rely on aggregate sources of data and non-exogenous variation in the provision of financial education. To date, no previous experimental study is able to examine longitudinal credit bureau records to measure causal long-term effects of school-based financial education on financial behavior. This study relies on EQUIFAX's credit registry to study the outcomes of high school graduates up to three years after the delivery of personal finance lessons. Specifically, credit behavior is evaluated by looking at the probabilities of having a credit history; have an active loan, further distinguishing between bank loans and microloans; and have negative records in the credit bureau. The distinction between bank loans and microloans is useful to evaluate preferences for formal banks, which tend to be more demanding in terms of requirements to get a loan (e.g., collateral), but offer lower interest rates than microfinance institutions.[18]

Panel A in Table 3 reports the impact of the program two years after the intervention. By June 2018, a third of the sample (41%) become legal adults, but it may still be too early in their adult lives to detect an effect. Indeed, the descriptive statistics at the bottom of the table show that, conditional on being over 18 years old, the match rate with EQUIFAX credit records is 23% in the control group. In most cases, the existence of a credit record is due to negative signals as opposed to outstanding loan balances. In the control group, 21% of the students are matched due to unpaid or delinquent bills, credit card statements, or past loans, while only 3% are matched due to an active loan. Not surprisingly, the results show that the treatment fails to improve students' access to credit or repayment behavior.

Three years after the intervention, 71% of the students originally in the sample became legal adults. Column 1 in Panel B in Table 3 shows that the match rate with EQUIFAX records in the control group increases to 30%; once more, the probability to be matched is mostly driven by the probability to have negative records (see column 5), but the probability to have an outstanding loan is now 7% in the control. With a higher share of the graduates becoming legal adults, some significant and long-lasting effects start to manifest: the treatment reduces delinquency rates by 6%. This improvement in repayment behavior corresponds one-to-one to a reduced probability of being matched with EQUIFAX records (column 1), indicating that treated students become less likely to start their credit histories with a negative signal.

This finding is extremely novel and powerful. Youth are often excluded from formal financial systems due to low labor participation rates and income and/or wealth levels.

[18]Section 2.3 provides a description of EQUIFAX's data and the construction of the outcome variables.

Since interactions of young adults within the financial system begin late, youth's needs end up being met by inadequate products and services. In Peru, for example, their lack of access to tailored financial services as well as their inexperience and low financial literacy levels lead to high levels of over-indebtedness through the use of credit cards. The provision of financial education during high school proves to be effective in reducing their likelihood to start a credit history with delinquent records.

Table 3: ITT Effects on Students' Access to Credit and Delinquency

	Pr(Records)	Pr(Credit)	Pr(Bank Loan)	Pr(Microcredit)	Pr(Default/ Arrears)
	(1)	(2)	(3)	(4)	(5)
Panel A: Two years after the intervention (June 2018)					
Treatment	-0.010	0.001	0.002	-0.001	-0.009
	[0.010]	[0.003]	[0.003]	[0.001]	[0.010]
Number of Observations	7850	7850	7850	7850	7850
Number of schools	296	296	296	296	296
Mean in Control	0.23	0.03	0.03	0.00	0.21
R-squared	0.07	0.03	0.03	0.03	0.07
Panel B: Three years after the intervention (June 2019)					
Treatment	-0.019**	-0.004	-0.005	0.000	-0.016*
	[0.009]	[0.004]	[0.004]	[0.001]	[0.009]
Number of Observations	13714	13714	13714	13714	13714
Number of schools	296	296	296	296	296
Mean in Control	0.30	0.07	0.07	0.00	0.27
R-squared	0.07	0.04	0.04	0.02	0.07

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents each day of the week. EQUIFAX records correspond to June 2018 and June 2019, two and three years after the intervention.

As expected, the ATT effects for all students' outcomes are even larger when compared to the ITT effects, but the general patterns and significance levels do not change dramatically.[19] Additional detailed results by grade are presented in Appendix A (Tables A.17 - A.21). Effect

[19]See Tables A.13-A.16 in Appendix A.

sizes on financial skills were relatively similar across ninth and eleventh grades, but smaller in tenth grade. Moreover, the average treatment effect estimated for the probability to pass a grade seems to hide a positive (negative) and significant effect among ninth (tenth) graders in the medium-run. However, both effect sizes are quite small. The impact on self-control seems to be driven mostly by tenth graders who also experience a reduction in the probability to be a hyperbolic discounter (Table A.18). This could be explained by a greater emphasis on forward-looking behavior and investment in the curriculum for this grade. Table A.19 indicates that the improvement in consumption habits comes from older students, who are more likely to manage money and expenses. Finally, Tables A.20 and A.21 examine the impact of the treatment on credit behavior by grade. However, these results should be analyzed with caution due to the reduced sample size in each sub-sample.

Even though parents were not directly targeted throughout the program, some of the workbooks' sections had short assignments for students to complete at home, either with or without the explicit request for parents' help. Even when students failed to do homework with parents, they could still be influenced by just interacting with the students within the household; this leads to potential spillover effects on their own financial knowledge and behavior.

Table 4: ITT Effects on Parents' Access to Credit and Delinquency

	Pr(Records)	Pr(Credit)	Pr(Bank Loan)	Pr(Microcredit)	Pr(Default/ Arrears)
	(1)	(2)	(3)	(4)	(5)
Treatment	0.020**	0.001	0.004	-0.005**	0.011
	[0.008]	[0.008]	[0.008]	[0.002]	[0.008]
Number of Observations	14709	14709	14709	14709	14709
Number of schools	296	296	296	296	296
Mean in Control	0.75	0.34	0.33	0.03	0.61
R-squared	0.04	0.04	0.04	0.03	0.03

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls corresponding to his child: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents each day of the week. EQUIFAX records correspond to June 2018, two years after the intervention.

Fortunately, the baseline survey instrument asked students to report the first and last name and cell phone number of their father or guardian. Ex-post, these identifiers were used

to obtain data on credit behavior almost two years after the intervention. Table 4 presents the results obtained from this snapshot of parental behavior. Relative to the control group, parents of students in the treatment group were two percentage points more likely to be reported in the bureau, i.e., to have a credit history. However, as opposed to the case of students, this higher match rate does not entirely respond to negative records. Moreover, parents seem to move away from microcredit loans (column 4), which tend to be more accessible, but at the cost of higher interest rates. The increase in the probability to have a bank loan is not strong enough to yield significant treatment effects, but the direction of the impact suggests a substitution effect toward these sources of credit.

In sum, the intervention was extremely successful at improving financial knowledge and academic performance. It also led to important changes in self-control and consumption habits in the short term. Notably, the treatment induced long-lasting effects on delinquency rates among graduates almost three years after the intervention. The intervention also had modest spillover effects on parental credit behavior, an outcome not directly targeted. Interaction within the household with their teenage children seems to ease access to knowledge, facilitating the transmission of personal finance knowledge. Two years after the intervention, parents in the treatment group exhibit positive changes in credit behavior, which can only improve cost-effectiveness calculations of the program.

4.2 Treatment Impacts on Teachers

Teachers were trained on the content covered in all grades, irrespective of the grade(s) they taught, during a 20-hour workshop held over five days. This is an important difference with respect to similar school-based interventions: in most previous studies, teachers only received guidance and were not trained in the concepts to be taught.

The curriculum was designed with high school students as the target beneficiaries in mind. Thus, the workbooks used very direct and simple language, with concrete examples and case studies that referred to everyday life. Because teachers delivered the content, they were continuously exposed to these materials. From the teachers' standpoint, participation in the treatment was not mandatory, but encouraged by the MINEDU and the school principals. Teachers not only have less access to financial knowledge than parents–they also face a more stringent requirement to participate. In addition, teachers are continuously exposed to the content through repetition when delivering the lessons.

Column 1 in Table 5 presents initial evidence on the first-hand effect of the financial education program on teachers' financial literacy. On average, the treatment generates important knowledge gains of 0.32SD. This is impressive, both when compared to previous

meta-analysis on the effects of financial education [Fernandes et al., 2014; Miller et al., 2014] as well as more recent and favorable ones [Kaiser and Menkhoff, 2017].

Table 5: ITT Effects on Teachers' Financial Knowledge

	Financial Literacy	
	Global	By repetition
	Global	level
	(1)	(2)
Treatment	0.320***	
	[0.100]	
Treatment X None		0.091
		[0.153]
Treatment X Low		0.383**
		[0.159]
Treatment X High		0.468***
		[0.144]
Number of Observations	417	417
Number of schools	250	250
Mean in Control	0.03	0.03
R-squared	0.37	0.37

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. Repetition level, based on self-report, is defined as: "None" if the teacher has not taught any lesson, "Low" if he/she covered under half of them, and "High" if the full material was covered. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

The financial literacy gains accrued by teachers do not translate into significant changes in shopping habits, but they do influence teachers' savings behavior. Table 6 shows that teachers in the treatment group are nine percent more likely to save. Behind this aggregate effect, there is a 14-percent increase in the share of those who save through formal channels; almost twice as large as the impact identified on the share of informal savers.

The estimated impact on the probability to save formally is quite large when compared to studies that measure the impact of financial education for adults on savings. For instance, Seshan and Yang [2012] find that exposure to a financial literacy workshop cannot affect the probability to save among Indian migrants in Qatar while Cole et al. [2011] identify no effect of a financial education program on the probability to open a savings account among

Table 6: ITT Effects on Teachers' Consumption and Saving Habits

	Consumption			Savings		
	Pr(Budgeting)	Shopping Habits Index	Financial Autonomy	Pr(Save)	Pr(Save Formally)	Pr(Save Informally)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.013	0.122	0.130	0.087**∧∧	0.140***∧∧	0.080*
	[0.025]	[0.119]	[0.095]	[0.035]	[0.048]	[0.042]
Number of Observations	331	280	347	334	376	334
Number of schools	212	184	214	214	232	214
Mean in Control	0.92	0.03	0.02	0.84	0.64	0.77
R-squared	0.43	0.32	0.33	0.41	0.31	0.42

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. ∧ significant at 10%; ∧∧ significant at 5%; ∧∧∧ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for shopping and saving habits, separately. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

unbanked urban households in Indonesia. The results on the likelihood to save identified among teachers are more in line with those obtained by Drexler et al. [2014], who report that the delivery of a heuristic financial training program led to an 8 percentage point increase in the probability to save among microfinance clients in the Dominican Republic. Similarly, the sizable increase in teachers' probability to save formally almost doubles the 7.5 percentage point increase identified by Carpena et al. [2015] among poor urban households in India exposed to classroom-based financial education. The sizeable impact on teachers' probability to save formally is particularly impressive when compared to recent effect sizes obtained from successful interventions *explicitly and exclusively* promoting higher levels of formal savings [Karlan et al., 2014; Dupas and Robinson, 2013; Flory, 2018; Breza and Chandrasekhar, 2019].

Savings data is self-reported and only measured in the short-term. However, the high-stakes long-term credit bureau data confirm that the treatment had an important impact on financial behavior among teachers.[20] Column 3 in Panel A in Table 7 shows that, two years after the intervention, the probability that teachers in the treatment group obtain a loan from a bank and other supervised financial institutions increases by eight percentage

[20]Note that virtually all teachers have a credit history in EQUIFAX, while this share is only three quarters among parents. This could be explained by higher levels of formality among teachers due to their public sector jobs. By default, teachers get their monthly wages deposited into a debit account, which may contribute to their relatively higher financial inclusion levels.

points. Moreover, delinquency rates among teachers in the treatment group decrease by 10 percentage points. These effects are quite large and, relative to the control group, amount to 13% and 15% improvements, respectively. Although no longer significant, these effects are sustained by the third year after the intervention (see Panel B) and they are not statistically different from those identified after the second year.[21]

Table 7: ITT Effects on Teachers' Access to Credit and Delinquency

	Pr(Records)	Pr(Credit)	Pr(Bank Loan)	Pr(Microcredit)	Pr(Default/ Arrears)
	(1)	(2)	(3)	(4)	(5)
Panel A: Two years after the intervention (June 2018)					
Treatment	0.005	0.043	0.078*	0.006	-0.101**
	[0.012]	[0.043]	[0.043]	[0.036]	[0.044]
Number of Observations	417	417	417	417	417
Number of schools	250	250	250	250	250
Mean in Control	0.98	0.65	0.58	0.27	0.69
R-squared	0.31	0.41	0.42	0.38	0.38
Panel B: Three years after the intervention (June 2019)					
Treatment	-0.008	0.003	0.035	0.034	-0.066
	[0.007]	[0.039]	[0.041]	[0.036]	[0.041]
Number of Observations	417	417	417	417	417
Number of schools	250	250	250	250	250
Mean in Control	1.00	0.72	0.67	0.28	0.78
R-squared	0.36	0.41	0.43	0.41	0.40

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

All things considered, the treatment led to significant changes in teachers' savings and credit outcomes. In general, the treatment seems to foster greater access to *formal* financial services among teachers, both through a higher preference to save in the financial system and a greater likelihood to obtain loans from banks and other regulated institutions in the market. As predicted by the model in Sub-Section 3.1, teachers experience much larger

[21]For all outcomes, a t-test rejects that the treatment coefficients are statistically different across June 2018 and June 2019.

impacts on their credit behavior than parents. This was expected since the treatment only tangentially affects parental behavior by reducing access barriers to financial education. Teachers, instead, are treated directly, minimizing the role of participation constraints, which enhances the effect of a reduction in the price of financial education.

Teachers are treated both directly through the training they receive as well as indirectly when delivering the lessons. Intensity of the treatment they experience will thus depend on their own choice to teach the lessons. A crucial difference between a teacher and another adult receiving financial education is that the former has to continuously teach the content. The exercise of simplifying the concepts and repeating them to their students in different ways may enhance learning. Thus, it is worth exploring the heterogeneous impact on teachers' outcomes by the degree of repetition of the content. Teachers who did not teach any of the lessons in the classroom would be thus comparable to adults in voluntary financial education programs, while those with at least partial compliance through teaching are benefiting from repetition.

Relying on self-reported records on coverage of the lessons from the endline survey, teachers are classified into three groups: no repetition (i.e., never taught a lesson), low repetition (i.e., taught less than half of the lessons), and high repetition (i.e., taught most of the lessons). Column 2 in Table 5 shows that greater repetition of the content is correlated with more financial literacy improvements among teachers. Teachers who covered more than half of the sessions in class reap gains close to 0.50 SD in terms of improved financial skills. Teachers in the high repetition group also report larger impacts on the probability to save, either formally or informally (see Table A.22). Notably, the probability to save in the financial system increases over 20 percent among teachers who covered most of the lessons.

Greater levels of coverage are also related to larger and more sustained effects in terms of credit outcomes. Two years after the intervention, teachers with high levels of repetition improved their access to credit by almost 12 percentage points (see column 2 in Panel A in Table 8), as opposed to the lack of impact estimated for this outcome in the full sample. Moreover, while the probability to obtain a loan from a regulated financial institution does not change among teachers with low levels of coverage, teachers who cover most of the material increase their chances to obtain a bank loan by 15 percentage points. Similarly, delinquency rates fall by 13 percentage points among teachers in the highest exposure group. Notably, the impact on credit access is sustained among teachers in the high repetition group even three years after the intervention (see columns 2 and 3 in Panel B).

Since the number of sessions taught is not exogenous and instead may depend on the motivation of the teachers and their initial levels of financial knowledge, this exercise is

Table 8: ITT Effects on Teachers' Access to Credit and Delinquency, by Repetition Level

	Pr(Records)	Pr(Credit)	Pr(Bank Loan)	Pr(Microcredit)	Pr(Default/ Arrears)
	(1)	(2)	(3)	(4)	(5)
Panel A: Two years after the intervention (June 2018)					
Treatment X None	0.013	0.014	0.040	-0.062	-0.047
	[0.015]	[0.073]	[0.073]	[0.064]	[0.074]
Treatment X Low	0.014	0.010	0.054	0.026	-0.118*
	[0.012]	[0.066]	[0.067]	[0.057]	[0.070]
Treatment X High	-0.014	0.115*	0.147**	0.050	-0.133*
	[0.028]	[0.069]	[0.072]	[0.072]	[0.078]
Number of Observations	417	417	417	417	417
Number of schools	250	250	250	250	250
Mean in Control	0.98	0.65	0.58	0.27	0.69
R-squared	0.31	0.42	0.42	0.39	0.38
Panel A: Three years after the intervention (June 2019)					
Treatment X None	-0.002	-0.001	0.005	-0.014	-0.056
	[0.002]	[0.067]	[0.069]	[0.066]	[0.067]
Treatment X Low	-0.003	-0.074	-0.041	0.028	-0.103*
	[0.003]	[0.061]	[0.065]	[0.059]	[0.061]
Treatment X High	-0.020	0.107*	0.163**	0.090	-0.029
	[0.017]	[0.064]	[0.064]	[0.074]	[0.072]
Number of Observations	417	417	417	417	417
Number of schools	250	250	250	250	250
Mean in Control	1.00	0.72	0.67	0.28	0.78
R-squared	0.37	0.42	0.44	0.41	0.40

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for saving and credit outcomes, separately. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. Repetition level, based on self-report, is defined as: "None" if the teacher has not taught any lesson, "Low" if he/she covered under half of them, and "High" if the full material was covered. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

only informative and should not be regarded as one yielding causal effects.[22] However, this

[22]Although selection into these three groups based on unobservables or initial levels of financial literacy cannot be ruled out, no important pattern emerges when checking how ex ante teachers' and students'

evidence suggests that repetition by teaching new concepts to someone else seems to increase the effects of the treatment among teachers. Although this analysis is only suggestive, it provides a rationale for the limited impact of one-shot programs that are usually preferred to maximize attendance among adults. Helping adults learn and change their habits may entail the use of strategies that make participation a high-stakes issue and repeatedly reinforce the concepts taught.

4.3 Cost Analysis

Existing evidence on financial education interventions does not provide much information on implementation costs. These data are extremely important, especially since these programs have become a common tool in financial inclusion efforts supported by national governments. Moreover, as an increasing number of countries are running school-based pilots with the hopes of scaling up these interventions, it becomes even more critical to collect and share information on their cost-effectiveness.

Among studies focusing on financial education for youth, Berry et al. [2018] is the only one that provides cost estimates that incorporate the marginal costs of training, monitoring, and materials for an after-school financial education program. The program, with a duration of eight weeks, had very low costs of US$ 0.62 per student enrolled in the experimental sample of schools. However, since attendance was voluntary, the actual cost amounted to US$ 4.15 per student.

In the Peruvian case, excluding the fixed cost of developing the workbooks, which amounted to US$ 56,100, marginal implementation costs of the school-based financial education program in 150 schools (31,000 high school students) amount to US$ 4.8 per student. Even though these costs are slightly higher than the ones reported by Berry et al. [2018], the significant impact of the Peruvian intervention on financial skills yields a very low cost-to-effectiveness ratio: the cost per student to improve average financial skills by one standard deviation amounts to US$ 31.5. Assuming constant returns to scale, each additional dollar spent in the program yields a 3.3-point improvement in the PISA financial literacy assessment. Notice that the effectiveness of the intervention is only measured in terms of students' learning. Given the measured impact on students' long-term behavior, teachers' knowledge and behavior, and parental behavior, the cost-effectiveness ratio can only decrease.

The returns to the financial education intervention are particularly high when compared to cost-effective interventions that seek to improve academic performance. For instance, Busso et al. [2017] identify a sample of 21 cost-effective interventions aimed at improving

observables affect the probability to teach the lessons in the classroom (see Table A.23).

learning in primary school (see chapter 7). In this sample, all but one intervention greatly surpass the ratio of cost to effect size calculated for the financial literacy program.

5 Conclusion

In the last decade, numerous countries have given financial education a central role in their efforts to promote financial inclusion. National financial inclusion strategies often have a strong financial education component, with an emphasis on children and youth. As an increasing number of governments debate the inclusion of financial education in the official school curriculum and as more resources are allocated to the development and implementation of school-based financial education programs, it is critical to evaluate the effectiveness of such efforts.

Relying on a large-scale experiment implemented in 300 public schools in Peru, this study measures the effects of a school-based financial education program for high school students. This study uses a rigorous design, large sample size, and rich survey and administrative data that allow for the measurement of outcomes in the short-term and long-term. In doing so, this study contributes to the scarce amount of literature on the effect of financial education on young people on two fronts. First, it relies on high-stakes data to measure the impact of financial education on financial behavior, both in the short-term and the long-term. This paper complements self-reported data with individual-level administrative academic and credit bureau records to measure the program's impact on academic outcomes and its long-lasting effects on financial behavior. Notably, this is the first paper to experimentally evaluate the long-term effects of school-based financial education on financial behavior. Second, this study analyzes the spillover effects of school-based financial education programs on adults interacting with the students at home and in the classroom. Indeed, no other study estimates treatment impacts among the instructors delivering the lessons. This is also the first paper that looks at the spillover effects of school-based financial education on parents when they are not directly targeted.

Overall, the financial education program implemented among high school students in Peru was extremely effective in improving students' and teachers' financial knowledge. The average gains among students are equivalent to an improvement of 16 points in Peru's performance in the 2015 PISA financial literacy assessment, which would be more than enough to halve the gap in performance with the next country in the ranking–Chile. Importantly, the effects on financial literacy are not generated to the detriment of academic performance.

The program also led to important changes in students', parents', and teachers' behavior. Among students, the treatment led to sizable improvements on self-control and shopping

habits five months after the intervention. Moreover, longer-term credit bureau data identifies a reduction in delinquency rates three years after the intervention. This effect is quite powerful, as it provides youth a better chance at starting their credit histories in good shape.

The study also identifies positive spillover effects of the program on parents, even though they were not directly targeted. Parents in the treatment group become more likely to have records in the credit registry and their likelihood of undertaking credit from microfinance institutions declines. Moreover, the impact on teachers' financial knowledge and behavior is impressive. Getting trained and imparting the financial education lessons improved teachers' financial skills by 0.32 SD. Teachers in the treatment group recorded a 10% increase in the probability to save, with a disproportionate preference for formal over informal mechanisms. Long-term effects on credit behavior also show a marked preference for formal instruments: two years after the intervention, the probability that teachers in the treatment group obtain formal bank loans increases by 13%, relative to their counterparts in the control group. Teachers also experience improved repayment behavior, reflected in a 15% decrease in their delinquency rates.

Although the results for teachers are not exactly comparable with existing evidence for adults, they are still encouraging as they show that some adults may learn and significantly change their financial behavior with a compulsory, or at least high-stakes, requirement to participate. Additionally, the heterogeneous impact on teachers by repetition levels suggests that adults may need learning strategies that incorporate constant reinforcement to successfully improve their financial literacy and behavior.

The results are even more promising when considering that a third of the teachers never taught a lesson and that only 43% of them attended all training sessions. The ITT effects are impressive even with modest levels of treatment intensity and they constitute a lower bound of the effect that the inclusion of financial education in the secondary school curriculum could have. The official inclusion of the content would improve compliance levels as teachers' attendance to the training workshops could be better enforced. It would also solve coordination problems between teachers and principals to incorporate the materials and would help teachers plan ahead to introduce this content.

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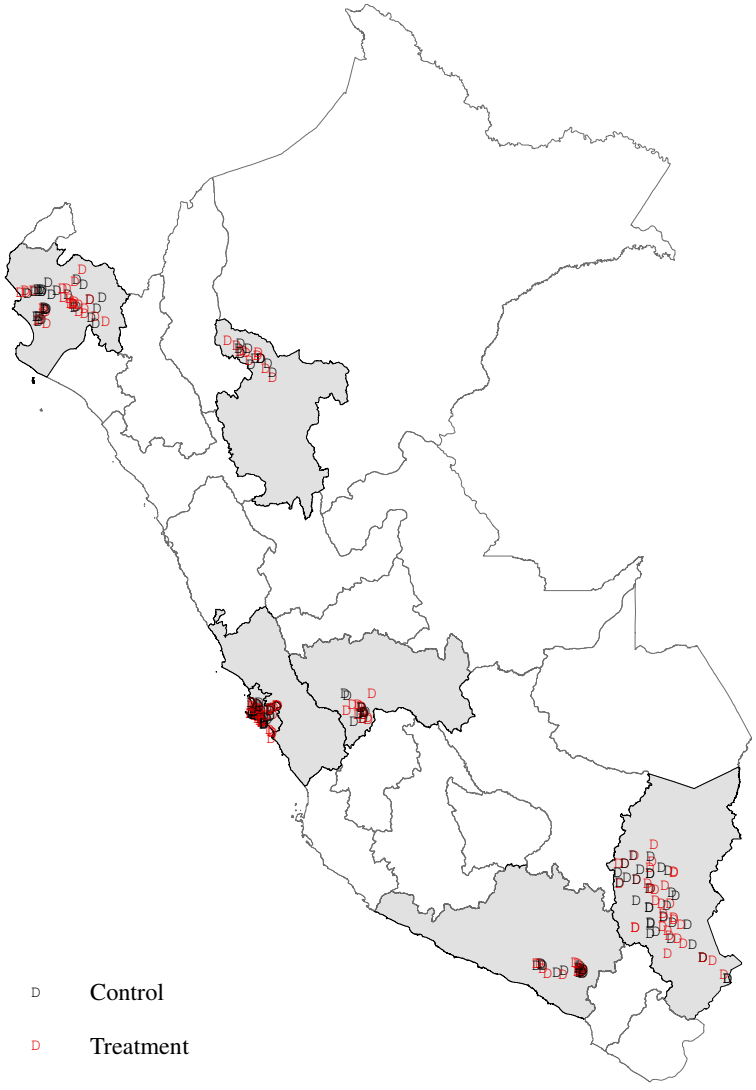
A Additional Figures and Tables (For Online Publication)

Figure A.1: Spatial Distribution of Control and Treatment Schools



NOTE: Intervention regions are highlighted grey.

Table A.1: Financial Literacy Lessons in Student Workbooks by Grade

3rd grade	4th grade	5th grade
1. Needs and resources	1. Financial products and services	1. Responsible financial consumer
1.1. Wants vs. needs	1.1. Financial system	1.1. Capacity to pay
1.2. Opportunity cost	1.2. Saving vs. Investment	1.2. Overindebtness
1.3. Savings vs. credit, expenditure vs. investment	1.3. Assets and liabilities	1.3. Financial consumer's rights
1.4. Economic agents	1.4. Financial future and capacity to pay	1.4. Protection of consumer rights
2. Budgeting	1.5. Adequate usage of financial products and services	1.5. The State and financial stability
2.1. Financial plan		2. Information
2.2. Income and expenses		2.1. Transparency in financial contracts
2.3. Budgeting		2.2. Consumers' responsibilities
2.4. Usefulness of budgets		

Table A.2: Psychometric Properties of the Students' Financial Literacy Exam

Item	Difficulty	Discriminatory	Difficulty	Discriminatory	Difficulty	Discriminatory
1	-2.526***	1.106***	-2.404***	1.406***	-2.404***	1.406***
2	-0.639***	0.863***	-1.025***	0.907***	-1.025***	0.907***
3	-0.360***	1.360***	-0.678***	1.174***	-0.678***	1.174***
4	-0.761***	0.905***	-1.039***	0.914***	-1.039***	0.914***
5	0.567***	0.704***	-0.080***	1.063***	-0.080***	1.063***
6	0.597***	1.555***	0.773***	0.492***	0.773***	0.492***
7	-6.114***	-0.285***	-0.782***	1.606***	-0.782***	1.606***
8	-0.229***	0.951***	-0.990***	1.719***	-0.990***	1.719***
9	4.933***	0.224***	0.117***	0.737***	0.117***	0.737***
10	0.147***	1.468***	2.085***	0.338***	2.085***	0.338***
11	0.070	0.913***	-0.068	0.731***	-0.068	0.731***
12	-0.079***	1.014***	-0.863***	1.773***	-0.863***	1.773***
13	0.501***	1.061***	-0.560***	1.614***	-0.560***	1.614***
14	-0.138***	1.161***	0.121***	1.099***	0.121***	1.099***
15	4.919***	0.278***	1.138***	0.492***	1.138***	0.492***

NOTE: Item-response theory estimates using a two-parameter model with students' baseline data. Significance levels * 10%, ** 5%, *** 1%.

Table A.3: Balance check: Student characteristics

Variable	Control mean	T-C	N
Sex	1.502	-0.010	20817
	[0.500]	[0.013]	
Age	15.160	0.005	16721
	[1.219]	[0.021]	
Ratio of household members to bedrooms	1.852	0.008	20002
	[0.996]	[0.016]	
Mother's education: Primary or less	0.429	0.006	19371
	[0.495]	[0.012]	
Mother's education: Secondary	0.419	0.010	19371
	[0.493]	[0.009]	
Mother's education: More than secondary	0.152	-0.015	19371
	[0.359]	[0.009]*	
Father's education: Primary or less	0.429	0.006	19371
	[0.495]	[0.012]	
Father's education: Secondary	0.419	0.010	19371
	[0.493]	[0.009]	
Father's education: More than secondary	0.152	-0.015	19371
	[0.359]	[0.009]*	
Lives with both parents	0.589	0.003	20245
	[0.492]	[0.009]	
Asset index	-0.000	-0.025	20388
	[1.000]	[0.029]	
High level of parental supervision	0.755	0.008	19330
	[0.430]	[0.006]	
Has dinner with parents 7 days a week	0.321	-0.002	20444
	[0.467]	[0.007]	
Truancy in the past 2 weeks	0.058	-0.006	20461
	[0.234]	[0.003]	
Student engagement	0.000	0.015	18346
	[1.000]	[0.016]	
Expects to become a professional	0.805	-0.011	19241
	[0.396]	[0.007]	
Expects to obtain at least tertiary education	0.510	-0.006	20123
	[0.500]	[0.009]	
Impulsiveness	-0.000	0.025	17435
	[1.000]	[0.014]*	
Conscientiousness	-0.000	0.004	15724
	[1.000]	[0.016]	
Self-control	0.000	0.002	16893
	[1.000]	[0.016]	
Hyperbolic preferences	0.126	-0.006	18262
	[0.332]	[0.004]*	
Risk lover	0.077	-0.001	19161
	[0.267]	[0.003]	
No previous exposure to financial education	0.367	-0.013	19162
	[0.482]	[0.009]	
Financial literacy raw score	8.060	0.110	20625
	[2.943]	[0.078]	
GPA 2015	13.727	-0.028	18382
	[1.484]	[0.042]	

Continued on next page

Variable	Control mean	T-C	N
Financial autonomy (1-75)	40.848	0.384	19520
	[12.911]	[0.189]**	
Has a savings account	0.137	0.003	19197
	[0.344]	[0.005]	
Budgeting	0.566	-0.012	18347
	[0.496]	[0.007]*	
Compares prices	0.043	-0.003	18357
	[0.204]	[0.004]	
Bargaining	0.939	0.005	18357
	[0.240]	[0.005]	
Talks to parents/tutors about family finance	0.709	-0.003	18632
	[0.454]	[0.006]	
Helps family with budgeting	0.679	0.008	18580
	[0.467]	[0.007]	

NOTE: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors(deviations) of coefficients(control means) are in brackets.

Table A.4: Balance check: Teacher characteristics

Variable	Control mean	T-C	N
Sex	0.577	-0.108	452
	[0.495]	[0.041]***	
Age	46.755	-0.924	431
	[11.028]	[0.958]	
Undefined contract teacher	0.637	-0.006	434
	[0.482]	[0.038]	
Workload (hours)	0.797	-0.050	378
	[0.404]	[0.041]	
Years of teaching experience	17.177	-0.649	400
	[10.217]	[1.004]	
Degree in Social Sciences	0.632	0.014	392
	[0.484]	[0.052]	
Higher education	0.332	0.055	425
	[0.472]	[0.046]	
Teaches in 9th grade	0.531	0.060	452
	[0.500]	[0.038]	
Teaches in 10th grade	0.526	0.037	452
	[0.501]	[0.037]	
Teaches in 11th grade	0.488	0.030	452
	[0.501]	[0.036]	

NOTE: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors(deviations) of coefficients(control means) are in brackets.

Table A.5: Balance check: Response rates

	All	9th	10th	11th
	(1)	(2)	(3)	(4)
Treatment	0.008	0.016	-0.005	0.013
	[0.009]	[0.016]	[0.017]	[0.016]
Number of Observations	888	296	296	296
Number of schools	296	296	296	296
Mean in Control	0.87	0.88	0.88	0.86
R-squared	0.30	0.51	0.49	0.49

NOTE: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors(deviations) of coefficients(control means) are in brackets.

Table A.6: Share of Missing Data by Construct and Survey Round

	Financial Literacy	Consciousness	Self-Control	Impulsiveness	Pr(Risk averse)	Pr(Hyperbolic)	Pr(Budgeting)	Shopping Habits Index	Financial Autonomy	Pr(Savings Account)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Baseline Survey										
Treatment	-0.010	-0.007	-0.006	-0.011	-0.006	-0.015	-0.009	-0.011	-0.014	-0.007
	[0.012]	[0.010]	[0.011]	[0.011]	[0.012]	[0.012]	[0.012]	[0.013]	[0.013]	[0.013]
Number of Observations	19487	19487	19487	19487	19487	19487	19487	19487	19487	19487
Number of schools	296	296	296	296	296	296	296	296	296	296
Mean in Control	0.13	0.33	0.28	0.26	0.19	0.23	0.22	0.24	0.18	0.19
R-squared	0.08	0.04	0.04	0.06	0.07	0.06	0.07	0.06	0.09	0.08
Panel B: Endline Survey										
Treatment	0.000	0.007	0.009	0.021*	0.018	0.007	-0.006	0.009	0.016	0.005
	[.]	[0.007]	[0.008]	[0.012]	[0.013]	[0.011]	[0.012]	[0.012]	[0.013]	[0.012]
Number of Observations	19487	19487	19487	19487	19487	19487	19487	19487	19487	19487
Number of schools	296	296	296	296	296	296	296	296	296	296
Mean in Control	0.00	0.19	0.11	0.32	0.30	0.32	0.20	0.18	0.13	0.15
R-squared	.	0.04	0.04	0.13	0.14	0.09	0.07	0.09	0.10	0.09

NOTE: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors(deviations) of coefficients(control means) are in brackets.

Table A.7: Balance check: Attrition Rates between Baseline and Endline Survey

	All	9th	10th	11th
	(1)	(2)	(3)	(4)
Treatment	-0.001	-0.001	-0.000	-0.003
	[0.001]	[0.001]	[0.000]	[0.002]
Number of Observations	17226	5921	5699	5606
Number of Schools	296	293	290	291
Mean in Control	0.16	0.17	0.16	0.16
R-squared	0.08	0.06	0.02	0.16

NOTE: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors(deviations) of coefficients(control means) are in brackets.

Table A.8: ITT Effects on Grades by Course

	Math	Language	Average w/o HGE	HGE
	(1)	(2)	(3)	(4)
Treatment	-0.007	0.033*	0.001	-0.016
	[0.019]	[0.018]	[0.020]	[0.014]
Number of Observations	19054	19054	19054	19054
Number of schools	296	296	296	296
Mean in Control	-0.01	-0.01	0.00	-0.01
R-squared	0.68	0.70	0.68	0.86

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents all days of the week, and the value of dependent variable at baseline.

Table A.9: ITT Effects on Academic Outcomes, Universe of Students

	2016		2017	
	GPA	Pr(Promoted)	GPA	Pr(Promoted)
	(1)	(2)	(3)	(4)
Treatment	-0.002	-0.010	0.013	0.004
	[0.011]	[0.009]	[0.014]	[0.009]
Number of Observations	54077	60074	32827	38528
Number of schools	298	298	298	298
Mean in Control	0.00	0.74	0.00	0.78
R-squared	0.88	0.03	0.86	0.03

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, and the value of dependent variable at baseline.

Table A.10: ITT Effects on Grades by Course, Universe of Students

	Math	Language	Average w/o HGE	HGE
	(1)	(2)	(3)	(4)
Treatment	0.001	0.022	0.003	-0.002
	[0.017]	[0.016]	[0.020]	[0.011]
Number of Observations	54077	54077	54077	54077
Number of schools	298	298	298	298
Mean in Control	0.00	0.00	0.00	0.00
R-squared	0.68	0.69	0.66	0.88

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents all days of the week, and the value of dependent variable at baseline.

Table A.11: ITT Effects on Students' Effort and Engagement

	Truancy	Engagement
	(1)	(2)
Treatment	-0.008	0.008
	[0.006]	[0.014]
Number of Observations	18695	17427
Number of schools	296	296
Mean in Control	0.21	0.00
R-squared	0.12	0.31

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. ∧ significant at 10%; ∧∧ significant at 5%; ∧∧∧ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for academic outcomes. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents all days of the week, and the value of the dependent variable at baseline.

Table A.12: ITT Effects on Other Self-Reported Student Outcomes

	Soft Skills & Preferences			Savings
	Consciousness	Impulsiveness	Pr(Risk averse)	Pr(Account)
	(1)	(2)	(3)	(4)
Treatment	-0.013	-0.000	0.009	-0.004
	[0.015]	[0.016]	[0.007]	[0.005]
Number of Observations	15705	13015	13324	16584
Number of schools	296	296	296	296
Mean in Control	0.00	0.00	0.68	0.16
R-squared	0.20	0.18	0.08	0.11

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. ∧ significant at 10%; ∧∧ significant at 5%; ∧∧∧ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for socioemotional skills and preferences (including outcomes in this table and the ones in columns 1-2 in Table 2). Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents all days of the week, and the value of the dependent variable at baseline.

Table A.13: ATT Effects on Students' Financial Knowledge and Academic Outcomes

		Academic Outcomes			
	Financial	2016		2017	
	Literacy	GPA	Pr(Promoted)	GPA	Pr(Promoted)
	(1)	(2)	(3)	(4)	(5)
Treatment	0.197***	-0.019	0.003	0.007	0.006
	[0.029]	[0.017]	[0.011]	[0.019]	[0.012]
Number of Observations	19487	19054	18574	11498	11703
Number of schools	296	296	296	296	296
Mean in Control	-0.01	-0.01	0.81	-0.02	0.80
R-squared	0.22	0.86	0.08	0.83	0.07

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for academic outcomes, GPA and Pr(Promoted). Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents all days of the week. In the case of financial literacy and GPAs, the value of the dependent variable at baseline is also included as a control.

Table A.14: ATT Effects on Students' Socioemotional Skills and Preferences

	Consciousness	Self-Control	Impulsiveness	Pr(Risk averse)	Pr(Hyperbolic)
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.016	0.041**	-0.000	0.013	-0.000
	[0.019]	[0.020]	[0.020]	[0.008]	[0.007]
Number of Observations	15705	17220	13015	13324	13193
Number of schools	296	296	296	296	296
Mean in Control	0.00	0.00	0.00	0.68	0.16
R-squared	0.20	0.21	0.18	0.07	0.02

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, has dinner with parents all days of the week, and the value of dependent variable at baseline.

Table A.15: ATT Effects on Students' Consumption and Savings Habits

	Consumption			Savings
	Pr(Budgeting)	Shopping Habits Index	Financial Autonomy	Pr(Account)
	(1)	(2)	(3)	(4)
Treatment	0.006	0.077***∧∧	0.021	-0.005
	[0.009]	[0.020]	[0.019]	[0.007]
Number of Observations	15672	15928	16696	16584
Number of schools	296	296	296	296
Mean in Control	0.64	0.00	-0.01	0.16
R-squared	0.06	0.05	0.16	0.11

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. ∧ significant at 10%; ∧∧ significant at 5%; ∧ ∧ ∧ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, has dinner with parents all days of the week, and the value of dependent variable at baseline.

Table A.16: ATT Effects on Students' Access to Credit and Delinquency

	Pr(Records)	Pr(Credit)	Pr(Bank Loan)	Pr(Microcredit)	Pr(Default/ Arrears)
	(1)	(2)	(3)	(4)	(5)
Panel A: Two years after the intervention (June 2018)					
Treatment	-0.012	0.002	0.002	-0.001	-0.011
	[0.012]	[0.004]	[0.004]	[0.001]	[0.012]
Number of Observations	7850	7850	7850	7850	7850
Number of Schools	296	296	296	296	296
Mean in Control	0.23	0.03	0.03	0.00	0.21
R-squared	0.07	0.03	0.03	0.03	0.07
Panel A: Three years after the intervention (June 2019)					
Treatment	-0.025**	-0.006	-0.006	0.000	-0.021*
	[0.012]	[0.005]	[0.005]	[0.001]	[0.012]
Number of Observations	13714	13714	13714	13714	13714
Number of Schools	296	296	296	296	296
Mean in Control	0.30	0.07	0.07	0.00	0.27
R-squared	0.07	0.04	0.04	0.02	0.07

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents all days of the week. EQUIFAX records correspond to June 2018 and June 2019, two and three years after the intervention.

Table A.17: ITT Effects on Students' Financial Knowledge and Academic Outcomes, by Grade

	Financial Literacy	Academic Outcomes			
		2016		2017	
		GPA	Pr(Promoted)	GPA	Pr(Promoted)
	(1)	(2)	(3)	(4)	(5)
		Panel A: 9th Grade			
Treatment	0.169***	-0.024	0.013	0.014	0.037***
	[0.032]	[0.018]	[0.012]	[0.020]	[0.012]
Number of Observations	6634	6476	6240	5632	5743
Number of Schools	296	296	296	296	296
Mean in Control	-0.01	0.01	0.78	-0.01	0.77
R-squared	0.30	0.86	0.09	0.84	0.09
		Panel B: 10th Grade			
Treatment	0.113***	-0.036**	-0.017	0.003	-0.023**
	[0.033]	[0.017]	[0.012]	[0.019]	[0.011]
Number of Observations	6496	6355	6201	5828	5917
Number of Schools	296	296	296	296	296
Mean in Control	-0.01	-0.02	0.81	-0.03	0.84
R-squared	0.28	0.87	0.10	0.85	0.09
		Panel C: 11th Grade			
Treatment	0.200***	0.026	0.013		
	[0.032]	[0.020]	[0.010]		
Number of Observations	6357	6223	6133		
Number of Schools	296	296	296		
Mean in Control	0.00	-0.01	0.85		
R-squared	0.23	0.88	0.10		

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for academic outcomes, GPA and Pr(Promoted). Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents each day of the week. In the case of financial literacy and GPAs, the value of the dependent variable at baseline is also included as a control.

Table A.18: ITT Effects on Students' Socioemotional Skills and Preferences, by Grade

	Consciousness	Self-Control	Impulsiveness	Pr(Risk averse)	Pr(Hyperbolic)
	(1)	(2)	(3)	(4)	(5)
			Panel A: 9th Grade		
Treatment	-0.021	0.018	0.031	-0.027**	0.014
	[0.023]	[0.024]	[0.026]	[0.011]	[0.010]
Number of Observations	5242	5786	4201	4315	4421
Number of Schools	296	296	294	296	294
Mean in Control	-0.01	0.00	-0.01	0.71	0.13
R-squared	0.21	0.19	0.19	0.09	0.06
			Panel B: 10th Grade		
Treatment	-0.036	0.065***	-0.022	0.018*	-0.016*
	[0.022]	[0.023]	[0.026]	[0.011]	[0.009]
Number of Observations	5223	5784	4462	4558	4487
Number of Schools	296	296	294	294	295
Mean in Control	0.00	0.00	0.00	0.67	0.17
R-squared	0.21	0.23	0.20	0.11	0.06
			Panel C: 11th Grade		
Treatment	0.019	0.018	-0.011	0.038***	0.006
	[0.024]	[0.024]	[0.027]	[0.012]	[0.009]
Number of Observations	5240	5650	4352	4451	4285
Number of Schools	296	296	295	295	296
Mean in Control	0.00	0.00	0.00	0.66	0.16
R-squared	0.24	0.27	0.23	0.12	0.05

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, has dinner with parents each day of the week, and the value of dependent variable at baseline.

Table A.19: ITT Effects on Students' Consumption and Savings Habits, by Grade

	Consumption			Savings
	Pr(Budgeting)	Shopping Habits Index	Financial Autonomy	Pr(Account)
	(1)	(2)	(3)	(4)
Panel A: 9th Grade				
Treatment	0.016	0.035	-0.022	0.009
	[0.012]	[0.024]	[0.027]	[0.010]
Number of Observations	5146	5263	5541	5521
Number of Schools	295	296	296	295
Mean in Control	0.63	-0.03	-0.01	0.19
R-squared	0.09	0.08	0.17	0.13
Panel B: 10th Grade				
Treatment	-0.006	0.055**	-0.004	-0.005
	[0.011]	[0.024]	[0.024]	[0.009]
Number of Observations	5301	5371	5644	5584
Number of Schools	295	294	294	295
Mean in Control	0.64	0.01	0.00	0.15
R-squared	0.08	0.09	0.18	0.14
Panel C: 11th Grade				
Treatment	0.003	0.122***	0.083***	-0.011
	[0.012]	[0.025]	[0.026]	[0.008]
Number of Observations	5225	5294	5511	5479
Number of schools	296	295	296	296
Mean in Control	0.65	0.02	-0.01	0.15
R-squared	0.10	0.08	0.20	0.13

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, has dinner with parents each day of the week, and the value of dependent variable at baseline.

Table A.20: ITT Effects on Students' Access to Credit and Delinquency Two Years After the Intervention (June 2018), by Grade

	Pr(Records)	Pr(Credit)	Pr(Bank Loan)	Pr(Microcredit)	Pr(Default/ Arrears)
	(1)	(2)	(3)	(4)	(5)
			Panel A: 9th Grade		
Treatment	-0.017	-0.020*	-0.020*	0.000	-0.003
	[0.031]	[0.012]	[0.012]	[.]	[0.030]
Number of Observations	610	610	610	610	610
Number of schools	225	225	225	225	225
Mean in Control	0.13	0.02	0.02	0.00	0.12
R-squared	0.30	0.26	0.26	.	0.28
			Panel B: 10th Grade		
Treatment	-0.029*	-0.008	-0.006	-0.002*	-0.024
	[0.016]	[0.006]	[0.006]	[0.001]	[0.016]
Number of Observations	1887	1887	1887	1887	1887
Number of Schools	281	281	281	281	281
Mean in Control	0.18	0.03	0.03	0.00	0.16
R-squared	0.11	0.10	0.10	0.07	0.11
			Panel C: 11th Grade		
Treatment	-0.001	0.006	0.007	-0.001	-0.002
	[0.011]	[0.004]	[0.004]	[0.001]	[0.011]
Number of Observations	5353	5353	5353	5353	5353
Number of Schools	296	296	296	296	296
Mean in Control	0.27	0.04	0.04	0.00	0.24
R-squared	0.06	0.04	0.04	0.04	0.07

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents each day of the week. EQUIFAX records correspond to June 2018 and June 2019, two and three years after the intervention.

Table A.21: ITT Effects on Students' Access to Credit and Delinquency Three Years After the Intervention (June 2019), by Grade

	Pr(Records)	Pr(Credit)	Pr(Bank Loan)	Pr(Microcredit)	Pr(Default/ Arrears)
	(1)	(2)	(3)	(4)	(5)
			Panel A: 9th Grade		
Treatment	-0.077***	-0.016*	-0.018**	0.001	-0.003
	[0.019]	[0.009]	[0.009]	[0.002]	[0.030]
Number of Observations	1875	1875	1875	1875	610
Number of Schools	283	283	283	283	225
Mean in Control	0.22	0.05	0.05	0.00	0.12
R-squared	0.12	0.10	0.10	0.08	0.28
			Panel B: 10th Grade		
Treatment	-0.012	-0.004	-0.002	-0.001	-0.012
	[0.012]	[0.005]	[0.005]	[0.001]	[0.011]
Number of Observations	5546	5546	5546	5546	5546
Number of Schools	296	296	296	296	296
Mean in Control	0.23	0.05	0.05	0.00	0.21
R-squared	0.05	0.04	0.04	0.03	0.05
			Panel C: 11th Grade		
Treatment	-0.008	-0.002	-0.004	0.002	-0.003
	[0.013]	[0.007]	[0.007]	[0.001]	[0.012]
Number of Observations	6293	6293	6293	6293	6293
Number of Schools	296	296	296	296	296
Mean in Control	0.38	0.10	0.10	0.00	0.35
R-squared	0.06	0.06	0.06	0.04	0.06

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. $\wedge$ significant at 10%; $\wedge\wedge$ significant at 5%; $\wedge\wedge\wedge$ significant at 1% after correcting pvalues for multiple testing. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: grade, sex, currently working, received financial education lessons in the past, ratio of household members to bedrooms, asset index, high level of parental supervision, lives with both parents, and has dinner with parents each day of the week. EQUIFAX records correspond to June 2018 and June 2019, two and three years after the intervention.

Table A.22: ITT Effects on Teachers' Savings Behavior, by Repetition Level

	Pr(Save)	Pr(Save Formally)	Pr(Save Informally)
	(1)	(2)	(3)
Treatment X None	0.091	0.053	0.129
	[0.068]	[0.102]	[0.084]
Treatment X Low	0.063	0.143**	0.027
	[0.053]	[0.072]	[0.063]
Treatment X High	0.112**	0.213***∧∧∧	0.105*
	[0.052]	[0.067]	[0.058]
Number of Observations	334	376	334
Number of Schools	214	232	214
Mean in Control	0.84	0.64	0.77
R-squared	0.41	0.32	0.43

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. ∧ significant at 10%; ∧∧ significant at 5%; ∧∧∧ significant at 1% after correcting pvalues for multiple testing. Correction for multiple testing implemented for saving outcomes. Standard errors clustered at the school level. OLS estimates, standard errors clustered at the school level are reported in brackets. Repetition level, based on self-report, is defined as: "None" if the teacher has not taught any lesson, "Low" if he/she covered under half of them, and "High" if the full material was covered. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: sex, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

Table A.23: Determinants of the Probability to Teach the Financial Education Lessons

	Pr(cover most lessons)	Pr(cover some lessons)
	(1)	(2)
Sex	0.002	0.034
	[0.059]	[0.063]
Age	-0.002	0.001
	[0.002]	[0.003]
Staff contract	0.050	0.055
	[0.084]	[0.087]
Teaching burden (more than 25h)	-0.076	0.003
	[0.072]	[0.072]
Experienced teacher	-0.109	0.004
	[0.069]	[0.086]
Degree in social sciences	-0.042	0.014
	[0.057]	[0.068]
Average students' initial GPA	0.054	-0.086
	[0.181]	[0.206]
Average students' initial financial literacy score	0.031	0.103
	[0.311]	[0.349]
Arequipa	-0.082	-0.092
	[0.090]	[0.131]
Junin	0.367***	0.326***
	[0.104]	[0.084]
Piura	0.098	0.177**
	[0.090]	[0.088]
Puno	-0.035	-0.153
	[0.085]	[0.122]
San Martin	0.194	0.369***
	[0.136]	[0.071]
miss_contr	0.043	0.007
	[0.082]	[0.075]
Number of Observations	240	240
Number of schools	144	144
R-squared	0.11	0.14

Note: Significance levels (* 10%; ** 5%; *** 1%). OLS estimates, standard errors clustered at the school level are reported in brackets. Sample of teachers in the treatment group. Based on teachers' self report, covering most lessons implies covering at least 50% of the material in the workbooks, while covering some lessons implies covering at least one lesson of the curriculum.

B Normalization of GPAs (For Online Publication)

Using raw GPAs as a performance measure poses several problems since they are not comparable across schools due to differential school quality, grade inflation, grading criteria, among other reasons. To deal with this issue, we construct *school quality normalized* GPAs [Frisancho et al., 2016]. For each subject i in grade g and school s, we define the adjustment factor, A_{igs}:

$$A_{igs} = \frac{\overline{\text{GPA}}_{igs}}{\overline{\text{Exam Score}}_{gs}} \div \frac{\overline{\text{GPA}}_{ig}}{\overline{\text{Exam Score}}_{g}} \tag{B.1}$$

where $\overline{\text{GPA}}_{igs}$ is the average GPA for subject i in grade g and school s. Similarly, $\overline{\text{Exam Score}}_{gs}$ is the average score in the baseline financial literacy exam for grade g in school s. $\overline{\text{GPA}}_{ig}$ and $\overline{\text{Exam Score}}_{g}$ are the average GPA for subject j and exam scores for all students in the same grade, irrespective of the school.

The ratio in the numerator in (B.1) should go up if the school is inflating grades relative to its true quality, for example. If the average GPA in math at grade g and school s is 8/10 but the average exam score for these students is only 5/10, grade g in school s is worse than the raw GPAs suggest. After all, since all students in the same grade take the same baseline financial literacy exam and are graded with the same objective criteria, $\overline{\text{Exam Score}}_{gs}$ should be a good proxy for the quality of the school on a unique scale. The ratio in the denominator in (B.1) is just a constant for all the students in the same grade and it takes the adjustment factor by subject to a common scale.

Define the school quality normalized GPA in subject i for student n in grade g and school s as:

$$\text{GPAnorm}_{nigs} = 100 \left(\frac{\widetilde{\text{GPA}}_{nigs}}{\widetilde{\text{GPA}}_{ig}^{\max}} \right)$$

where:

$$\widetilde{\text{GPA}}_{nigs} = \left(\frac{\text{GPA}_{nigs}}{A_{igs}} \right)$$

and $\widetilde{\text{GPA}}_{ig}^{\max}$ is just the maximum $\widetilde{\text{GPA}}_{nigs}$ in a given grade. Notice that this normalization penalizes grade inflation through a higher A_{igs}.