

The Impact of School-Based Financial Education on High School Students and their Teachers*

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Abstract

Relying on a large-scale experiment, this paper studies the potential of school-based financial education for youth. Financial education lessons yield impressive effects on students' and teachers' financial skills (0.15SD and 0.32SD, respectively) and lead to sizable improvements on self-control and shopping habits among students. The intervention also raises teachers' likelihood to save by 10%, effect mostly driven by a 22%-increase in their probability to save formally. Longer-term credit bureau data fails to identify any effect on students' financial behavior but shows that teachers become 13% more likely to get a bank loan and 15% less likely to be delinquent.

Keywords: Financial Education, Youth, Self-Control, Consumption Behavior, Savings, Credit records, Treatment Effects

JEL Codes: D14, D91, J24, O16

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1 Introduction

Rapid financial development paired with financial inclusion initiatives have tremendously improved access to formal financial systems. However, take up and usage rates of financial products and services remain low [Karlan et al., 2016], especially in the developing world. Financial education has become a popular policy prescription to improve consumers' financial knowledge and capabilities as a way to bridge the gap between underserved population segments and financial institutions.

Despite initial disappointment [Fernades et al., 2014], recent evidence shows that financial education programs improve financial literacy and have a modest effect on financial behavior among adults [Miller et al., 2014; Kaiser and Menkhoff, 2017]. However, changing adults' financial habits is quite challenging, both due to low participation and attendance rates [Bruhn et al., 2013] and reduced malleability of their preferences and habits [Casey et al., 2005; Blakemore and Choudhury, 2006]. More recently, children and youth have been identified as a priority target for financial education efforts [OECD/INFE, 2015], which has fostered the development of school-based financial education programs and started the debate around the inclusion of this content in national curricula.

Relying on a large-scale randomized controlled trial (RCT) implemented in 300 public schools in Peru, this study measures the effects of a school-based financial education program on high school students and their teachers. Notably, this is the first experimental study that goes beyond survey data in the short run and relies on administrative records to estimate the medium-run impact of school-based financial education on financial outcomes. Further, I am able to measure the impact of youth financial education programs on students but also on teachers acting as facilitators. This is a novel and important contribution since this potential source of trickle-up effects has been ignored so far.

The experimental sample was stratified by region and, within each strata, schools were paired by their similarity in terms of observable characteristics, generating 150 matched pairs. The treatment was randomized at the school level, within each pair. Students in the sample were tested on their financial knowledge and surveyed both at baseline (May 2016) and at the end of the school year (December 2016). Survey data in both rounds included questions on personality traits related to financial behavior as well as on shopping and saving habits. Teachers' data come from an exit financial knowledge exam and an exit survey covering questions on financial attitudes and behavior. Access to administrative records allows me to observe students' cumulative grade point averages (GPAs) in three consecutive academic years: one year before the intervention, the year in which the intervention is launched, and the next year after the treatment is delivered. Furthermore, access to credit bureau data in

June 2018, 23 months after the intervention was implemented, allows me to evaluate changes in credit behavior of both students and teachers in the medium run.

The program had strong effects on the financial knowledge of young students. Relative to the control group, scores in the financial literacy exit exam go up by 0.15 SD. This effect is large, especially when compared to voluntary after-school programs [Berry et al., 2018; Jamison et al., 2014], but comparable to similar school-based interventions in other settings [Bruhn et al., 2016; Bover et al., 2018]. The impact of the intervention is equivalent to a 16-point improvement in the 2015 PISA financial literacy assessment of 15-year-old students. Such an improvement amounts to a 4% increase in the average score, which would close more than half the gap with Chile, the closest country ahead of Peru in the ranking.

The introduction of financial education lessons does not hinder performance in other courses and has no effect on grade progression in the short or the medium run. These results suggests that the time allocated away from other courses and into personal finances does not yield perverse effects in terms of academic achievement.

Although it was not designed with this objective in mind, the treatment develops greater levels of self-control among high school students, as measured by self-reported data using the Tangney et al. [2004]’s scale. The treatment also improves students’ shopping habits but it does not affect saving patterns. Despite these short-run effects, longer run impacts on credit behavior are absent both in terms of access to credit and delinquency rates. However, these results may respond to a low share of “at-risk” subjects since only 37% of the original sample of students become legal adults by June 2018.

The effect of the program on teachers’ knowledge and behavior is impressive. Imparting the financial education lessons improves teachers’ financial skills by 0.32 SD and leads to better savings habits among them: the probability that they save goes up by 10%. Interestingly, teachers disproportionately allocate savings to formal mechanisms, as is evident from the 22% increase in the probability to save formally. Administrative credit bureau records further allow me to identify an effect on teachers’ credit behavior: even though their credit access is unaffected on average, the probability that teachers in the treatment group obtain a bank loan goes up by 8 percentage points and their delinquency rates decrease by 10 percentage points. These effects are quite large and, relative to the control group, amount to 13% and 15% improvements, respectively. Heterogeneous treatment effects reveal that the impact on teachers is concentrated among those who covered a greater share of the curriculum. Though only suggestive, this result indicates that successfully and substantially improving adults’ financial skills and behavior may require learning strategies that incorporate constant reinforcement and repetition.

Rigorous evidence on the effects of financial literacy programs for youth is scarce but

the availability of experimental studies is on the rise. So far, the findings suggest that early financial education programs are quite promising. The evidence uncovers large and robust effect sizes under delivery models that incorporate personal finances material through a mandatory course requirement [Becchetti and Pisani, 2012; Bover et al., 2018; Bruhn et al., 2016; Batty et al., 2015, 2017] while revealing that voluntary after-school programs yield meager or null effects [Berry et al., 2018; Jamison et al., 2014].

However, the existing literature suffers from several limitations. First, despite greater malleability of preferences and personality traits at young ages, few studies on youth have paid attention to the impact on traits associated with financial habits such as time-preferences or self-control [Gathergood, 2012; Strömbäck et al., 2018].¹ A second limitation is that few studies focus on the potential spillover effects that financial education programs may have on academic outcomes. Looking at these outcomes is quite relevant as many argue that introducing financial literacy content in the curricula takes time away from other courses, potentially damaging performance. Another shortcoming of the current evidence is the lack of studies that look at the long-term impacts of financial education for the youth on financial behavior. Quasi-experimental studies take advantage of natural variations in graduation requirements in the US and show that young people who are exposed to state-mandated financial education courses while in school have relatively higher credit scores and lower delinquency rates than people without exposure to such requirements Brown et al. [2014]. However, experimental evidence has not been able to confirm this result as available studies either rely on phase-in designs or have very short follow-up periods.

Relying on a design similar to Bruhn et al. [2016]’s, this paper builds upon them and contributes to the empirical literature on financial education among youth on several fronts. First, this paper measures the impact of financial education on preferences and personality traits in addition to financial knowledge and behavior. Since teaching good financial habits usually requires touching upon topics such as intertemporal choices and trade-offs, I expect the program to influence individual intertemporal preferences and self-control levels, among others. Second, this study looks at the spillover effects of school-based financial education programs on academic outcomes such as grade promotion and performance. More importantly, this is the first experimental study that relies on administrative records to estimate the medium-run impact of school-based financial education on financial outcomes. Previous

¹Notable exceptions include Lührmann et al. [2015], who measure the impact of a financial education program on time inconsistency among German students. Jamison et al. [2014] look at 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 [2017] evaluate the impact of an intervention specifically designed to improve 3rd and 4th graders’ ability to picture their future selves and encourage forward-looking behavior.

experimental studies for the same age group have exclusively relied on survey data to measure short-term impacts on financial behavior [Becchetti and Pisani, 2012; Bruhn et al., 2016; Jamison et al., 2014] and/or used phase-in designs that ruled out the possibility to measure longer-term effects [Bover et al., 2018]. This is also the first study that measures the effect of school-based financial education on the *teachers* in charge of delivering the lessons, which can be informative for the design of adult financial education programs. Finally, the study provides a cost-effectiveness analysis, which is particularly relevant to guide policy efforts trying to move from evidence into action.

2 Experimental Design

2.1 The Intervention

In 2015, the Peruvian government launched the National Financial Inclusion Strategy [Comisión Multisectorial de Inclusion Financiera, 2015], which included as a high-priority goal the provision of school-based financial education to all primary 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 that provided financial education to high school students. Together, they developed student workbooks for each of the last three grades in high school (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 in 5 sessions, which included a training component on the financial literacy contents (4 sessions) as well as a pedagogical one (1 session).² MINEDU encouraged teachers to attend the training sessions conducted before the school year started. School principals were requested to facilitate teacher participation in the training and 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 described in Table A.1. In general, 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 while the curriculum for eleventh-graders covered the topics of responsible financial consumer 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

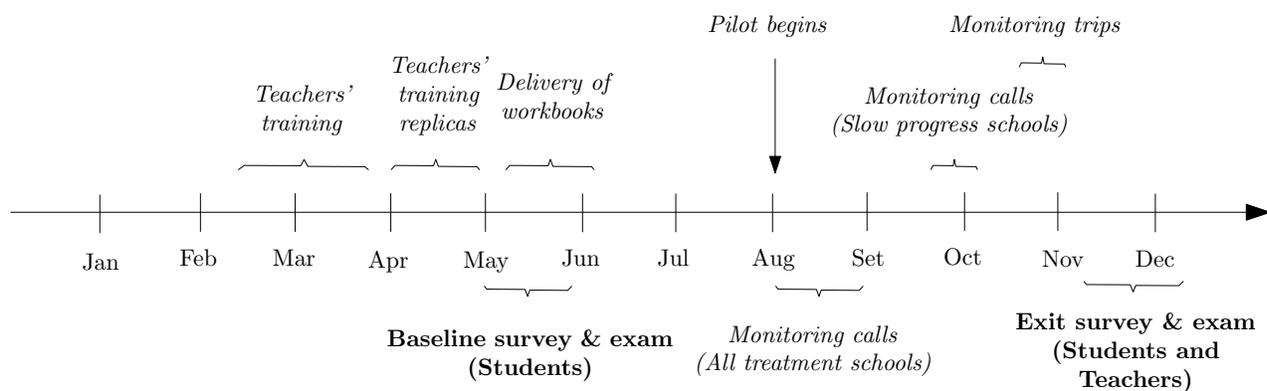
²The content of the pedagogical session included a review of the background of the program, including the launching of the National Financial Inclusion Strategy, as well as the use of teaching tools such as charts, figures, and case studies.

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 delivered became subject to performance evaluation and was considered high stakes by the students.

2.2 Study Timeline

Figure 1 organizes both the intervention activities (in italics) and the evaluation activities (in bold) that took place during the 2016 calendar year. Teachers’ training workshops were conducted between mid-February and March, before the beginning of the school year. Additional replica sessions conducted by trained teachers to reach untrained teachers were organized during the first month of classes. Distribution activities of students’ workbooks started in May and the delivery of the sessions in class took place during the second half of the 2016 school cycle, August through December. To make sure compliance levels were high, two monitoring efforts were conducted. First, MINEDU called all treatment schools to record progress by grade. Second, between the end of October and beginning of November, CEFI reached all schools that reported to be delayed during the first monitoring round. A few monitoring trips were conducted in late October by SBS and MINEDU to collect qualitative information on the opinions of the principals, teachers, and students on pilot activities.

Figure 1: Study Timeline



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 between the second week of November and the first week of December.³

³All data collection efforts were conducted once the Chesapeake Institutional Review Board (IRB) determined that the evaluation activities were exempt from IRB oversight.

2.3 Data and Measurement

The data used in the analysis of students' outcomes come from four sources: survey data collected before and after the pilot was implemented, exam scores collected at entry and exit, administrative records on student performance at the end of 2015, 2016, and 2017 academic years, and credit bureau records by the end of June 2018.

The 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. The questionnaire additionally gathers information on five personality constructs and preferences which may influence financial choices: conscientiousness, self-control, intertemporal preferences, impulsiveness and venturesomeness, and risk aversion.⁴ The survey also gathers information to measure student engagement in school⁵ and collects data on previous exposure to financial education programs and self-evaluation of math skills, financial knowledge, and pro-saving behavior [Mandell and Klein, 2009]. The baseline survey measured financial behavior on several fronts: formal savings, budgeting, participation in household financial decisions, consumption and saving habits, and financial autonomy, the latter measured as in Bruhn et al. [2016]. 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.

All exams were grade-specific and consisted of 15 questions. Four questions were drawn from the 2008 National Jump\$Start Coalition Survey of High School Seniors and College Students on the topics of risk, return and liquidity; intertemporal spending choices; budgeting to save; and the importance of investing in skills and education.⁶ The remaining questions tested students on the specific topics covered in each grade-specific workbook. Most questions were drawn from a teacher entry exam designed by the SBS and CEFI,⁷ but a few were

⁴Conscientiousness, 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]. Although preferences and personality traits are self-reported, they are measured relying on extensively tested scales that are specifically designed to be self-rated.

⁵The scale to measure student engagement comes from the Student Engagement in Schools Questionnaire (SESQ) and measures behavioral engagement: effort and persistence [Hart et al., 2011].

⁶See Mandell [2009]. The Jump\$Start Coalition for Personal Financial Literacy is a US non-profit coalition of 150 organizations that tries to promote financial literacy among students. Its target population includes students between pre-kindergarten and all the way through college. Jump\$Start 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 12th grade. Since 2000, Jump\$Start has administered the Survey of Personal Financial Literacy among high school students.

⁷SBS 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.

developed by the author to cover topics included in the workbooks but excluded from the teacher exam. The same grade-specific exam was administered at baseline and endline.

The Ministry of Education shared administrative records for all the students enrolled in high school in 2016 in any of the 300 schools of the experimental sample. The data records cumulative grades by course and grade progression at the end of three consecutive academic years, 2015 through 2017: one before and two after the intervention was launched.

Finally, all students in the sample were searched in the leading private credit bureau in the country, EQUIFAX, which concentrates loans data from almost all lenders in the Peruvian credit market.⁸ Data from EQUIFAX allows us to distinguish between loans from banks and other financial institutions supervised by the SBS from those that come from formal but non-regulated institutions such as microfinance NGOs and cooperatives. Students were searched in EQUIFAX's dataset by the end of June 2018, 23 months after the beginning of the intervention. Those matched were returned with records on their indebtedness level in regulated and non-regulated institutions as well as their delinquency status on loans and other bills such as department store credit cards, water and electricity services, personal taxes, etc.

Data for teachers come from an exit survey, an exit exam, as well as from credit bureau records. The endline questionnaire applied to teachers was very similar to the students' instrument but additional questions were added to capture teachers' professional background and experience, as well as to measure their monthly household income and formal and informal savings. The exit exam taken by teachers was developed by the author and included the four questions from the JumpStart questionnaire as well as questions from the students' exams for ninth grade (4), tenth grade (4), and eleventh grade (3). Teachers were also searched in EQUIFAX's database to gather information on their credit access and delinquency rates by the end of June 2018.

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 logistic reasons, the universe was further restricted to Local Education Management Units which were close to cities and with a high number of schools under its supervision. After imposing some additional restrictions (directly managed by the MINEDU, single-grade

⁸EQUIFAX concentrates 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 the credit records of their borrowers, EQUIFAX is the only firm in Peru that has been able to include over 90% of them in its records.

schools, and number of students by grade above the 5th percentile and below the 95th percentile), the final universe included 308 schools.⁹

The restricted universe was stratified by region. Following Bruhn and McKenzie [2009], I paired schools by their similarity within each of the six strata.¹⁰ This pairing procedure returns 150 matched pairs; within each pair, each school was randomly assigned to one of the treatment groups. Provided that non-compliance and non-response are orthogonal to the magnitude of the treatment impact, this design is robust to the exclusion of a pair of schools in which at least one school has incomplete records.

The total number of students in the experimental sample of 300 schools is 60,467. Within each school, one classroom from each grade was chosen at random to be interviewed and tested at baseline and endline. The baseline sample consists of 20,980 students (7,120, 6,955, and 6,905 in ninth, tenth, and eleventh grade, respectively), roughly a third of the universe of interest. On average, the initial sample size per classroom is 27 students. The exit survey and exam were applied to 19,750 students (6,720, 6,590, and 6,440 in ninth, tenth, and eleventh grade, respectively). Only 17,354 students were present in both data collection rounds. Although the attrition rate between baseline and endline is 17%, it is not differential by treatment status.

The sample of interest to evaluate the impact of the intervention are all students with records in the follow up survey and exam. If this sample is matched with performance records in 2016, the same academic year in which the intervention took place, the success rate is extremely high at 97.7%. The share of students that are matched with 2015 and 2016 records is still high, at 93%. Focusing on 9th and 10th graders who are still in school in 2017, the match rate with all three rounds of administrative performance records is 84%.

Matching students with EQUIFAX's credit bureau data naturally yields a much lower success rate. Since legal adulthood starts at age 18 in Peru, only students who have reached that age by the time they are searched for are at risk of being matched. By the end of June 2018, only 7,404 of the students in the follow up sample were legal adults. Conditional on being matched, only 23% of them had at least one credit record in the risk bureau. The data provided by EQUIFAX allows me to measure existence of credit records, access to credit,

⁹To 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, about 300 schools were required, 150 in each treatment arm.

¹⁰I minimize the Mahalanobis' distance 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; whether the school belongs to the original sample chosen by CEFI in 2015; and whether the school belongs to the experimental sample of any other ongoing pilot.

access to credit from regulated institutions (also referred to as bank credit), and delinquency.

Tables A.2 and A.3 in Appendix A provide basic descriptive statistics at the school and student level as well as balancing tests of the randomization. Consistent with the random treatment assignment, no significant differences are detected across groups.

2.5 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/teacher i in school j from pair p . The regressor y_{ijp}^{pre} , the baseline value of y_{ijp} , is included when evaluating students' outcomes that come from the survey as well as in the case of academic performance outcomes. The impact of the treatment is measured by β , the coefficient on the indicator of treatment status, T_{jp} . All regressions include additional students' or teachers' characteristics as controls, X_{ijp} , and a set of dummies, d_{jp} , identifying the pair of schools matched. In the case of survey outcomes, I exclude from the analysis all pairs with at least one school without available data at endline.¹¹

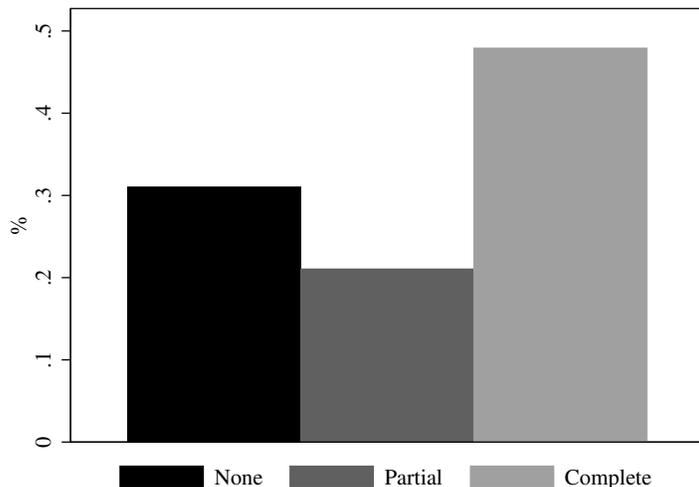
The intervention did not have perfect compliance levels within the treatment group (see sub-section 2.6) but no teacher in the control group attended the training workshops and no student from the control group received the lessons or the workbooks. For each grade, I define compliance at the school level by a dummy variable, Z_{jp} , that equals one if at least one of the teachers attended one or more of the training sessions. I rely on this measure of compliance for two reasons. First, it is an objective measure, based on the attendance records kept by the MINEDU and SBS. Teachers' self-report of the coverage of lessons in class (see Section 2.6) is expected to be biased as teachers may have an incentive to overreport their progress due to social desirability bias. Second, it is a primary measure of compliance since teachers need to be trained to deliver the content of the workbooks in class. Average treatment on the treatment (ATT) effects can then be obtained by instrumenting Z_{jp} with the random assignment of the treatment, T_{jp} .

¹¹Only one school refused to participate in the follow up, which reduces the sample size to 298 schools in 149 pairs in all estimations.

2.6 Treatment Intensity

About 73% of teachers in the treatment group attended at least one training session and only 43% attended all sessions. If we look at teachers' self-report of their progress teaching the lessons at follow up, the intensity of the treatment is similar. Figure 2 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. Still, a third of the teachers report that they had not taught a single lesson of the financial education material. In the sub-sample of treatment schools in which at least one teacher attended all sessions, 55% of the teachers covered all the lessons. However, even in this sample of high levels of training, a third of the teachers did not deliver any financial literacy content.

Figure 2: Coverage of Financial Education Content as Reported by Teachers, by Grade



Under the pilot activities, 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 replace content or accommodate their time usage for other topics in the course. Qualitative evidence indicates that teachers struggled to incorporate the materials. Survey data reveals that, on average, teachers incorporated 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.

Even though compliance levels were relatively high, a third of the teachers report not teaching the content of the workbooks at all. Consequently, the results obtained in the

pilot should be interpreted as a lower bound of those which could be obtained by offering more specific guidelines to incorporate the material and closer monitoring of the progress. The inclusion of financial education in the school curriculum could of course solve these implementation issues in subsequent rounds or scaling-up efforts.

3 Results

3.1 Treatment Impacts on Students

In general, the school-based financial education program implemented was extremely effective to improve high school students' financial knowledge. The first column in Table 1 reports that the treatment increases scores in the exit financial knowledge exam by 0.15 SD relative to the control group. These average gains are very much in line with the results of similar programs implemented among high school students in Brazil [Bruhn et al., 2016] and Spain [Bover et al., 2018]. 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 risk of substituting time and resources away from other courses, sacrificing student learning in other areas. The results from the pilot indicate that the opportunity cost of introducing the personal finance lessons is not high enough to hinder academic performance in other courses. Columns 2 and 4 in Table 1 show that the treatment has no significant effect on cumulative grades neither in the short nor in the medium run.¹² If anything, the personal finance lessons slightly boost language performance by 0.04 SD (see Table A.4).

The intervention does not yield unintended perverse effects on dropout as in Bjorvatn et al. [2015], where the curriculum covered encouraged entrepreneurship. The estimated treatment effects on grade progression are not significantly different from zero neither in 2016 nor in 2017, which could respond to the fact that passing rates are already very high: both the average and the median passing rate in the sample of schools is 0.94.

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.5 and A.6 in Appendix A). Moreover, it is accompanied by lack of changes in student effort, as measured by attendance or student engagement (see Table A.7). One may argue that the content of the financial education program may complement well some of the material taught in regular

¹²All grades are normalized by school quality to make them comparable across schools. See Appendix B for more details on the normalization implemented.

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

	Financial Literacy (1)	Academic Outcomes			
		2016		2017	
		GPA (2)	Pr(Promoted) (3)	GPA (4)	Pr(Promoted) (5)
Treatment	0.150*** [0.023]	-0.017 [0.014]	0.002 [0.009]	0.003 [0.015]	0.005 [0.009]
Number of Observations	19688	19249	18774	11614	11822
Number of schools	298	298	298	298	298
Mean in Control	-0.00	0.01	0.81	-0.00	0.80
R-squared	0.23	0.86	0.07	0.84	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 p-values 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, gender, currently working, score in literacy exam at baseline, 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.

courses or that the innovative content can boost other dimensions of student engagement such as the affective component (liking learning and/or school). However, these indirect effects were not triggered by the intervention: academic outcomes were left unaffected by the treatment.

Teaching financial skills entails covering constructs such as opportunity cost and intertemporal trade-offs, among others. This content may impact individual preferences or socioemotional skills. This is particularly important since we still know little about the malleability of these traits and the extent to which environment can shape them.

Table 2 presents the treatment impacts on conscientiousness, self control, and impulsiveness scales as well as on the probability of being a risk lover or having hyperbolic time preferences. The treatment successfully fosters the development of self-control skills with an average effect of 0.03 SD. Although this impact seems modest, it is still an important finding. The intervention was not directly designed to do so but it is still able to impact self-control, which is a socioemotional skill associated with better financial and health outcomes [Moffitt et al., 2011]. Developing lessons with more specific content to encourage the development of these traits may prove effective among youth.

Even though current financial choices of youth are quite limited, they still manage a budget and make shopping and saving decisions that can be shaped by the treatment. Indeed, Table 3 shows that the intervention has a sizable and significant impact on students' shopping

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

	Consciousness (1)	Self-Control (2)	Impulsiveness (3)	Risk averse (4)	Hyperbolic (5)
Treatment	-0.017 [0.015]	0.033** [0.016]	-0.000 [0.016]	0.008 [0.006]	-0.001 [0.005]
Number of Observations	15860	17405	13162	13480	13340
Number of schools	298	298	298	298	298
Mean in Control	-0.00	-0.00	-0.00	0.68	0.16
R-squared	0.20	0.21	0.18	0.08	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: grade, gender, currently working, score in literacy exam at baseline, 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.

habits. On average, shopping choices in the treatment group improve by 0.05SD in terms of an index that measures if they compare prices before shopping, save instead of borrow to buy something they cannot afford, and bargain before shopping. Despite this positive effect on shopping habits, the probability to save is not significantly affected.

Table 3: ITT Effects on Students' Consumption and Savings Habits

	Consumption			Savings
	Budgeting (1)	Shopping Habits Index (2)	Financial Autonomy (3)	Saves (4)
Treatment	0.005 [0.007]	0.057*** $\wedge\wedge\wedge$ [0.015]	0.011 [0.015]	0.010 [0.009]
Number of Observations	15861	16119	16896	14112
Number of schools	298	298	298	298
Mean in Control	0.64	0.00	-0.00	0.59
R-squared	0.07	0.05	0.16	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, gender, currently working, score in literacy exam at baseline, 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.

Unique data from EQUIFAX allows me to track students 23 months after the intervention and evaluate if the treatment affected their credit outcomes in the medium run. Unfortunately, Table 4 shows that the financial education program fails to improve access to credit or to reduce delinquency rates. Even though over a third of the sample (37%) becomes a legal adult by the time they are searched in the risk bureau’s database, it may still be too early in their adult lives to detect an effect. First, they may still be too young to demand credit as the median student who reaches legal adulthood by June 2018 in my sample is only 18.6 years old. Second, they may be excluded from formal financial systems as they have low labor participation rates and income and/or wealth levels. Indeed, in 2017 15% of people at age 15 and above in Peru borrowed money from a financial institution, but this percentage was only 8% in the age bracket 15 to 24 [Demirguc-Kunt et al., 2015].

In sum, the intervention was extremely successful at improving financial knowledge and academic performance. It also led to important changes in socioemotional traits and consumption habits in the short run. However, the treatment did not induce a longer term effect on credit access and delinquency among students. As expected, the ATT effects for financial literacy, preferences and socioemotional traits, and behavior are even larger than the ITT effects, but the general patterns and significance levels do not change much.¹³

Table 4: ITT Effects on Students’ Access to Credit and Delinquency

	Has Records (1)	Credit (2)	Bank Credit (3)	Default/ Arrears (4)
Treatment	-0.005 [0.010]	0.002 [0.004]	0.003 [0.004]	-0.005 [0.010]
Number of Observations	7404	7404	7404	7404
Number of schools	298	298	298	298
Mean in Control	0.24	0.04	0.03	0.22
R-squared	0.07	0.03	0.03	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, gender, currently working, score in literacy exam at baseline, 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.

¹³See Tables A.8-A.11 in Appendix A.

3.2 Treatment Impacts on Teachers

The training teachers received was quite intensive and practical and it followed a curriculum that built upon broad economic concepts to then move onto more specialized material. Teachers were trained on the content covered in all grades, irrespective of the grade(s) they were currently teaching, during a 20-hour workshop held over five days. Since the curriculum was designed with high school students as the target beneficiaries in mind, the workbooks developed used a very direct and simple language, with concrete examples and case studies that referred to everyday life. Teachers were also exposed to a great degree of repetition of the content as they had to teach it to their students. Thus, from the teachers' point of view, the intervention can be considered as an intensive adult financial education program with high exposure to the content through repetition, accessible materials, and a more general curriculum.

Table 5: ITT Effects on Teachers' Financial Knowledge and Teaching Efficacy

	Financial Literacy (1)	Teaching Efficacy (2)
Treatment	0.320*** [0.100]	0.012 [0.097]
Number of Observations	417	318
Number of schools	250	206
Mean in Control	0.03	-0.01
R-squared	0.37	0.42

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. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: gender, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

On average, the treatment generates important knowledge gains of about 0.32SD among HGE teachers (see Table 5). These are impressive learning gains, particularly when compared to the 0.21SD effect size on financial literacy estimated in the meta-analysis by Kaiser and Menkhoff [2017] for financial education interventions implemented outside the school setting.

In terms of their performance in the classroom, it seems that the treatment does not generate significant changes in teaching practices. Column 2 in Table 5 shows that teachers do not exhibit any improvements in their perceived levels of teaching efficacy. This suggests that neither the financial literacy component nor the pedagogical component of the training had spillover effects on teaching practices in other courses.

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

	Consumption			Savings		
	Budgeting	Shopping Habits Index	Financial Autonomy	Saves	Saves Formally	Saves Informally
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.013 [0.025]	0.122 [0.119]	0.130 [0.095]	0.087** ^{^^^} [0.035]	0.140*** ^{^^^} [0.048]	0.080* [0.042]
Number of Observations	331	280	347	334	376	334
Number of schools	212	184	214	214	232	214
Mean in Control	0.93	0.01	0.05	0.84	0.64	0.75
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: gender, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

Table 6 shows that the intervention does not change teachers' shopping habits but is able to generate important effects on their saving habits. Teachers in the treatment group are 9 percentage points more likely to save. Behind this aggregate effect, there is a 14-percentage-point 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 impacts on the probability to save and to save formally are quite high, especially if we compare them to the effect sizes estimated in studies that measure the impact of financial education interventions for adults. For instance, Seshan and Yang [2012] find that exposure to a financial literacy workshop is not able to 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 urban unbanked 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 urban poor households in India exposed to classroom-based financial education.

The treatment is also able to generate large and significant changes on teachers' credit behavior in the medium-run, 23 months after the intervention was launched. Table 7 shows that, even though credit access does not change, the probability that teachers in the treatment group obtain a loan from a bank and other SBS supervised financial institutions goes up by 8 percentage 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.

All in all, the treatment leads to significant changes on teachers' savings and credit outcomes. More importantly, the treatment seems to foster greater access of teachers to *formal* financial services, both through a higher preference to save in the financial system and to obtain loans from banks and other regulated institutions in the market.

A crucial difference between a teacher and any other adult receiving financial education is that the former has to teach the content over and over again. The exercise of simplifying the concepts and repeating them to their students in different ways may be an important channel that explains the gap in learning when compared to the available evidence on the impact of financial literacy programs among adults. By relying on the measure of compliance that captures teachers' self-reported coverage of the lessons in class, I explore the heterogeneous impact on teachers' outcomes by the degree of repetition of the content. I define three groups of teachers: no repetition (i.e., never taught a lesson), low repetition (i.e., taught less than

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

	Has records (1)	Credit (2)	Bank Credit (3)	Default/ Arrears (4)
Treatment	0.005 [0.012]	0.043 [0.043]	0.078* [0.043]	-0.103** [0.045]
Number of Observations	417	417	417	417
Number of schools	250	250	250	250
Mean in Control	0.98	0.65	0.58	0.67
R-squared	0.31	0.41	0.42	0.38

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 p-values 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: gender, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

Table 8: ITT Effects on Teachers' Financial Knowledge and Teaching Efficacy, by Compliance Level

	Financial Literacy (1)	Teaching Efficacy (2)
Treatment X None	0.091 [0.153]	-0.004 [0.203]
Treatment X Low	0.383** [0.159]	-0.195 [0.156]
Treatment X High	0.468*** [0.144]	0.308* [0.170]
Number of Observations	417	318
Number of schools	250	206
Mean in Control	0.03	-0.01
R-squared	0.37	0.44

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. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: gender, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

half of the lessons), and high repetition (i.e., taught most of the lessons).

Indeed, the greater the exposure to repetition of the content, the larger are the financial literacy improvements among teachers. Table 8 shows that those who covered more than

half of the sessions in class reap gains close to 0.50 SD. These teachers in the high repetition group also report greater perceived teaching efficacy (0.31 SD). Along the same lines, the impact on savings is increasing in the level of repetition and, except for the likelihood to save formally, only significant for the high repetition group (see Table 9). Notably, the probability to save in the financial system goes up by over 20 percentage points among teachers in the treatment group who covered most of the lessons.

Greater levels of coverage are also related to larger effects in terms of credit outcomes. Teachers with high levels of repetition see their access to credit improve by almost 12 percentage points as opposed to the lack of impact estimated 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 in the treatment group who cover most of the material see their chances to obtain a bank loan significantly increased by 15 percentage points.

This exercise is only informative as 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, among other observable and unobservable characteristics. However, when checking how ex ante observables vary across different repetition levels, no important differences emerge (see Table A.12). Although I cannot rule out that selection into these three groups is guided by unobservables or initial levels of financial literacy, the similarity in observables is somehow reassuring.

In sum, this evidence suggests that repetition by teaching new concepts to someone else seems to enlarge 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 repeatedly reinforce the concepts taught.

Table 9: ITT Effects on Teachers' Savings and Credit Behavior, by Compliance Level

	Savings			Credit			
	Saves	Saves Formally	Saves Informally	Has Records	Credit	Bank Credit	Default/Arrears
	(1)	(2)	(3)	(4)	(5)	(6)	
Treatment X None	0.091 [0.068]	0.053 [0.102]	0.129 [0.084]	0.013 [0.015]	0.014 [0.073]	0.040 [0.073]	-0.054 [0.073]
Treatment X Low	0.063 [0.053]	0.143** [0.072]	0.027 [0.063]	0.014 [0.012]	0.010 [0.066]	0.054 [0.067]	-0.138* [0.071]
Treatment X High	0.112** [0.052]	0.213*** $\wedge \wedge \wedge$ [0.067]	0.105* [0.058]	-0.014 [0.028]	0.115* [0.069]	0.147** [0.072]	-0.107 [0.083]
Number of Observations	334	376	334	417	417	417	417
Number of schools	214	232	214	250	250	250	250
Mean in Control	0.84	0.64	0.75	0.98	0.65	0.58	0.67
R-squared	0.41	0.32	0.43	0.31	0.42	0.42	0.38

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 p-values 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. All specifications include a set of dummy variables that correspond to the matched-pair of schools and the following set of controls: gender, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

3.3 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 urgent 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 8 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 attending the meetings.

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) equal US\$ 4.8 per student. Even though these costs are slightly higher than the ones reported by Berry et al. [2018], the large 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, I estimate that 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 large impact on teachers' knowledge and behavior, the cost-effectiveness ratio can only become lower.

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 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; indeed, the average cost per student to improve learning by one standard deviation within this sample of cost-effective interventions is as high as US\$ 448.

4 Conclusion

In the last decade, numerous countries have given financial education a central role in their efforts to promote financial inclusion. Until recently, financial education was conceived as corrective or compensatory and aimed at an adult population at risk of making inadequate financial decisions. Available evidence to date, which comes largely from interventions focused on adults, indicates that financial education programs have positive but modest effects on financial knowledge and behavior.

Instead, the development of financial education programs aimed at children and adolescents offers great potential. On the one hand, the provision of financial education within the school offers the possibility of reaching a captive audience, which solves the problems of participation and attendance faced when working with adults. On the other hand, children and young people are more malleable in terms of their habits because they are still developing them; providing timely guidance could be crucial to have a significant and lasting impact on their financial behavior during adulthood.

When working with children and youth, the policy goal is to invest early to change future financial behavior. But even if healthier financial habits are promoted in the present, these may not permeate into more complex decisions once out of school. In the short term, it is thus crucial to evaluate the impact of school-based financial education programs on intrinsic preferences and socioemotional skills that will shape financial choices and habits during adulthood. At the same, more efforts are needed to assess the longer term impact of these programs on behavior.

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. Through the use of a rigorous design, large sample size, and rich survey and administrative data sources that allow me to measure outcomes in the short and the medium-run, this study contributes to the scarce literature on the effect of financial education on young people on several fronts. First, it analyzes the spillover effects of school-based financial education programs on academic outcomes such as grade promotion and performance. Moreover, I measure the impact of financial education on youth's preferences and personality traits that are related to financial behavior, which are more malleable during childhood and adolescence. Notably, this is the first study to provide experimental evidence of the impact of financial education for youth on medium-run financial behavior, both among students and teachers in charge of the delivery of the lessons. Finally, the study also portrays a cost-effectiveness analysis that is helpful to guide policy efforts.

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' and teachers' behavior, with differential effects across sub-samples in the short and the long run. Among students, the treatment led to sizable improvements on self-control and shopping habits 5 months after the intervention was implemented. However, longer-term credit bureau data fails to identify any effect on students' financial behavior. Among teachers, I identify a strong effect on the demand for formal financial services. The intervention raised teachers' likelihood to save by 10%, effect mostly driven by a 22%-increase in their probability to save formally. Almost 2 years after the intervention, teachers become 13% more likely to get a bank loan and 15% less likely to be delinquent.

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 under more intensive programs, with more accessible materials, and that cover a broad curriculum. 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 intervention was extremely cost-effective, with a cost per student of US\$ 4.8. Assuming constant returns to scale, I estimate that each additional dollar spent in the program yields a 3.3-point improvement in the PISA financial literacy assessment.

Keeping in mind that a third of the teachers never taught a single lesson and that only 43% of them attended all training sessions, the results obtained are even more promising. The intention to treat effects are impressive even with modest levels of treatment intensity, and they represent a lower bound of the effect that including financial education content in the secondary school curriculum could have. The official inclusion of the content will improve compliance levels because teachers' attendance of training workshops could be better enforced. It would also solve the coordination problems between teachers and principals to incorporate the materials and would help teachers plan ahead to reserve time for this particular content. Nevertheless, one has to keep in mind the introductory nature of these programs. Current evidence does not let us predict if the large impacts on knowledge on students and teachers would persist once financial literacy lessons become a regular course in the high school curriculum.

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A Additional Figures and Tables

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

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

Table A.2: Balance check: School characteristics

Variable	Control mean	T-C	N
Number of teachers	22.347 [12.497]	0.213 [1.415]	300
Number of students	362.973 [222.866]	-3.107 [25.412]	300
Number of classrooms	13.887 [7.516]	-0.073 [0.841]	300
Number of students, 9th grade	72.060 [42.631]	0.747 [4.952]	300
Number of students, 10th grade	68.880 [41.459]	-0.500 [4.767]	300
Number of students, 11th grade	64.933 [42.250]	-0.207 [4.692]	300
Number of classrooms, 9th grade	2.773 [1.542]	0.020 [0.171]	300
Number of classrooms, 10th grade	2.660 [1.423]	-0.007 [0.164]	300
Number of classrooms, 11th grade	2.553 [1.412]	-0.020 [0.161]	300
Passing rate, 9th grade	0.702 [0.141]	-0.005 [0.016]	300
Passing rate, 10th grade	0.742 [0.128]	-0.010 [0.015]	300
Passing rate, 11th grade	0.801 [0.122]	0.006 [0.015]	300
Dropout rate, 9th grade	0.032 [0.036]	-0.003 [0.004]	300
Dropout rate, 10th grade	0.031 [0.038]	-0.002 [0.004]	300
Dropout rate, 11th grade	0.018 [0.023]	0.000 [0.003]	300
GPA 2015, 9th grade	13.367 [0.618]	0.005 [0.067]	300
GPA 2015, 10th grade	13.414 [0.526]	0.032 [0.061]	300
GPA 2015, 11th grade	13.660 [0.602]	-0.033 [0.068]	300

Note: Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation with robust standard errors. Standard errors(deviations) of coefficients(control means) are in brackets.

Table A.3: Balance check: Student characteristics

Variable	Control mean	T-C	N
Sex	1.501 [0.500]	-0.008 [0.016]	20958
Age	15.159 [1.219]	0.010 [0.032]	16820
Ratio of household members to bedrooms	1.852 [0.993]	0.006 [0.023]	20138
Mother's education: Primary or less	0.429 [0.495]	0.012 [0.019]	19506
Mother's education: Secondary	0.419 [0.493]	0.004 [0.013]	19506
Mother's education: More than secondary	0.152 [0.359]	-0.016 [0.014]	19506
Father's education: Primary or less	0.429 [0.495]	0.012 [0.019]	19506
Father's education: Secondary	0.419 [0.493]	0.004 [0.013]	19506
Father's education: More than secondary	0.152 [0.359]	-0.016 [0.014]	19506
Lives with both parents	0.590 [0.492]	0.004 [0.012]	20382
Asset index	-0.000 [1.000]	-0.036 [0.047]	20525
High level of parental supervision	0.755 [0.430]	0.007 [0.009]	19453
Has dinner with parents 7 days a week	0.321 [0.467]	-0.001 [0.010]	20580
Truancy in the past 2 weeks	0.058 [0.234]	-0.006 [0.004]	20599
Student engagement	-0.000 [1.000]	0.013 [0.022]	18460
Expects to become a professional	0.804 [0.397]	-0.013 [0.010]	19373
Expects to obtain at least tertiary education	0.509 [0.500]	-0.008 [0.015]	20260
Impulsiveness	-0.000 [1.000]	0.024 [0.019]	17547
Conscientiousness	-0.000 [1.000]	0.008 [0.022]	15807
Self-control	0.000 [1.000]	0.001 [0.021]	16984
Hyperbolic preferences	0.126 [0.332]	-0.006 [0.005]	18368
Risk lover	0.077 [0.267]	-0.000 [0.004]	19280
No previous exposure to financial education	0.368 [0.482]	-0.016 [0.013]	19281
Financial literacy raw score	8.059 [2.947]	0.114 [0.109]	20765
GPA 2015	13.751 [1.483]	-0.025 [0.057]	17869

Continued on next page

Variable	Control mean	T-C	N
Financial autonomy (1-75)	40.863 [12.926]	0.421 [0.262]	19646
Has a savings account	0.137 [0.343]	0.003 [0.007]	19316
Budgeting	0.566 [0.496]	-0.010 [0.009]	18464
Compares prices	0.043 [0.203]	-0.005 [0.005]	18466
Bargaining	0.939 [0.239]	0.007 [0.006]	18466
Talks to parents/tutors about family finance	0.708 [0.454]	-0.002 [0.008]	18745
Helps family with budgeting	0.679 [0.467]	0.008 [0.010]	18692

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: ITT Effects on Grades by Courses

	Math (1)	Language (2)	Excluding HGE (3)	HGE (4)
Treatment	-0.011 [0.019]	0.035* [0.018]	-0.001 [0.020]	-0.018 [0.014]
Number of Observations	19249	19249	19249	19249
Number of schools	298	298	298	298
Mean in Control	0.01	0.01	0.01	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: gender, currently working, score in literacy exam at baseline, 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.5: ITT Effects on Academic Outcomes, All Students in the School

	GPA (1)	Pr(Pass Grade) (2)	GPA (3)	Pr(Pass Grade) (4)
Treatment	-0.002 [0.011]	-0.010 [0.009]	0.013 [0.014]	0.005 [0.009]
Number of Observations	54414	60427	33010	38721
Number of schools	300	300	300	300
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, gender, and the value of dependent variable at baseline.

Table A.6: ITT Effects on Grades by Courses, All Students in the School

	Math (1)	Language (2)	Excluding HGE (3)	HGE (4)
Treatment	0.000 [0.016]	0.023 [0.015]	-0.000 [0.020]	-0.002 [0.011]
Number of Observations	54414	54414	54414	54414
Number of schools	300	300	300	300
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: gender, currently working, score in literacy exam at baseline, 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.7: ITT Effects on Students' Effort and Engagement

	Truancy (1)	Engagement (2)
Treatment	-0.006 [0.006]	0.006 [0.014]
Number of Observations	18895	17607
Number of schools	298	298
Mean in Control	0.21	-0.00
R-squared	0.12	0.31

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. 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: gender, currently working, score in literacy exam at baseline, 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.8: ATT Effects on Students' Financial Knowledge and Academic Outcomes

	Financial Literacy (1)	Academic Outcomes			
		2016		2017	
		GPA (2)	Pr(Promoted) (3)	GPA (4)	Pr(Promoted) (5)
Treatment	0.189*** [0.029]	-0.022 [0.017]	0.003 [0.011]	0.004 [0.019]	0.006 [0.012]
Number of Observations	19688	19249	18774	11614	11822
Number of schools	298	298	298	298	298
Mean in Control	-0.00	0.01	0.81	-0.00	0.80
R-squared	0.22	0.86	0.07	0.84	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 p-values 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, gender, currently working, score in literacy exam at baseline, 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.9: ATT Effects on Students' Socioemotional Skills and Preferences

	Consciousness (1)	Self-Control (2)	Impulsiveness (3)	Risk averse (4)	Hyperbolic (5)
Treatment	-0.022 [0.019]	0.042** [0.020]	0.000 [0.020]	0.011 [0.008]	-0.001 [0.007]
Number of Observations	15860	17405	13162	13480	13340
Number of schools	298	298	298	298	298
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 p-values 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: gender, currently working, score in literacy exam at baseline, 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.10: ATT Effects on Students' Consumption and Savings Habits

	Consumption			Savings
	Budgeting	Shopping Habits Index	Financial Autonomy	Saves
	(1)	(2)	(3)	(4)
Treatment	0.005 [0.009]	0.074*** $\wedge\wedge\wedge$ [0.020]	0.014 [0.019]	0.013 [0.012]
Number of Observations	15861	16119	16896	14112
Number of schools	298	298	298	298
Mean in Control	0.64	0.00	-0.00	0.59
R-squared	0.06	0.05	0.16	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: gender, currently working, score in literacy exam at baseline, 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.11: ATT Effects on Students' Access to Credit and Delinquency

	Has Records	Credit	Formal Credit	Default/ Arrears
	(1)	(2)	(3)	(4)
Treatment	-0.006 [0.013]	0.003 [0.005]	0.004 [0.004]	-0.006 [0.012]
Number of Observations	7404	7404	7404	7404
Number of schools	298	298	298	298
Mean in Control	0.24	0.04	0.03	0.22
R-squared	0.07	0.03	0.03	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: gender, currently working, score in literacy exam at baseline, 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.

Table A.12: Differences in Observables Across Teachers that Incur in Heterogenous Degrees of Repetition

Variable	No exposure	Low exposure	High exposure	N
Sex	0.481 [0.503]	0.039 [0.076]	-0.022 [0.088]	240
Staff contract	0.622 [0.488]	0.108 [0.067]	-0.038 [0.086]	234
Teaching burden (more than 25h)	0.723 [0.451]	0.086 [0.067]	-0.015 [0.086]	202
High experience (greater than median)	0.471 [0.503]	0.132 [0.078]*	-0.082 [0.093]	215
Degree in social sciences	0.632 [0.486]	0.068 [0.073]	0.047 [0.085]	211
Postgraduate studies	0.425 [0.498]	-0.027 [0.081]	-0.120 [0.085]	230

Note: Significance levels (* 10%; ** 5%; *** 1%). Differences between low and high repetition are obtained relative to the no repetition category and are obtained from regressing each variable on the two former repetition categories. Standard errors in brackets.

B Normalization of GPAs

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} :

$$A_{igs} = \frac{\overline{\text{GPA}}_{igs}}{\overline{\text{Exam Score}}_{gs}} \div \frac{\overline{\text{GPA}}_{ig}}{\overline{\text{Exam Score}}_g} \quad (\text{B.1})$$

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} .