IS SCHOOL-BASED FINANCIAL EDUCATION EFFECTIVE? IMMEDIATE AND LONG-LASTING IMPACTS ON HIGH SCHOOL STUDENTS

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Abstract

Relying on a large-scale experiment in Peru, this study evaluates the effects of an in-class intervention on financial literacy and financial behavior. As soon as the program is over, treated students record significant financial literacy gains that do not hinder their academic performance. The program also leads to immediate changes in downstream financial behavior as measured by financial autonomy and financial savviness. Credit bureau records gathered three years later show that early improvements in financial literacy translate into limited, but positive long-lasting changes in financial behavior. The treatment did not affect students' credit or repayment behavior on the extensive margin, but, among those few with outstanding loans, it reduced arrears by 20%.

Keywords: Financial Education, Youth, Financial Literacy, Credit records, Treatment Effects, Long-lasting impacts

JEL Codes: C93, D14, G53, O16

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1 Introduction

Financial education tends to trigger heated debates in academic and policy forums. Despite the evidence supporting a link between financial literacy and economic outcomes [Behrman et al., 2012; Lusardi and Mitchell, 2014; Lusardi et al., 2017; Bianchi, 2018; van Rooij et al., 2012], many are skeptical that financial education programs can effectively improve financial skills, let alone lead to sustained changes in financial choices and behavior. The increased availability of experimental studies targeting children and youth supports significant immediate impacts of school-based financial education on financial literacy [Kaiser and Menkhoff, 2019; Frisancho, 2019], but the ability of these programs to yield robust and long-lasting effects on financial behavior is still under scrutiny.

This paper exploits experimental variation in the delivery of mandatory personal finance lessons in Peru to study the immediate and potential long-lasting impacts of school-based financial education. Combining survey and credit bureau records on nearly 20,000 students, this paper shows that financial education in the school is effective to improve youth's financial literacy and downstream financial behavior in the short run and to generate limited, but sustained effects on credit behavior a few years out of school. Previous studies on youth that have relied on experimental variation to asses the impact of financial education have measured financial outcomes using self-reported survey records and within a short time span of at most 16 months [Bruhn et al., 2016; Bover et al., 2018; Jamison et al., 2014; Luhrmann et al., 2018].[1] A number of studies assess the sustainability of financial education's effects over longer-term horizons in the US using administrative records such as Census records, credit bureau records, or financial aid data, but these rely on differences-in-differences models, synthetic control strategies, or event studies that exploit non-experimental variation in course requirements during high school [Bernheim et al., 2001; Brown et al., 2016; Cole et al., 2016; Urban et al., 2020; Stoddard and Urban, 2020]. This is the first study that relies on credible exogenous variation in exposure to financial education and complements survey records with rich administrative data that measure actual financial behavior several years after the treatment.

This study relies on data from a large-scale randomized controlled trial implemented in 300 public high schools in six regions in Peru, targeting grades nine through eleven. The treatment was randomized at the school level and consisted of the delivery of financial education lessons during the school day, between August and December 2016. The curricula imparted varied across grades: while 9th graders received lessons on the differences between needs and resources and budgeting, 10th graders learnt about financial products and services and forward-looking choices, and 11th graders received material on responsible financial consumers and access to information in financial markets. The instructors in charge of the lessons were school teachers who were trained in the

[1] Alan and Ertac [2018] is a notable exception among the experimental studies as it moves beyond survey records and relies on incentivized time preference elicitation tasks both in the short-run as well as three years after the delivery of the treatment. However, the authors are not able to look at actual financial behavior as their sample corresponds to elementary school children.

materials. Therefore, teachers are treated both directly through the training they receive as well as indirectly when delivering the lessons in the classroom.

Measurement of the impact of the treatment relies on both survey and administrative data sources. Students in the treatment and control groups were tested on their financial knowledge and surveyed both before and after the delivery of the lessons. The content of the financial literacy exam varied by grade, depending on the curricula. Survey data in both rounds included questions on financial behavior such as financial autonomy, budgeting, and shopping and saving habits. Teachers in treated and control schools completed a financial knowledge exam and an exit survey. Access to school administrative records provides information on students' cumulative grade point averages (GPAs) in two consecutive academic years, pre and post treatment. Furthermore, credit bureau data provides information on access to credit and delinquency for students and teachers three years after the intervention was launched.

The program led to significant financial literacy gains among treated students: relative to the control group, scores in the financial literacy exit exam increased by 0.16 SD in the treatment group. 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 introduction of financial education lessons did not hinder performance in other courses and had no effect on grade progression, which shows that the time diverted away from other courses and into personal finances did not jeopardize academic achievement. The provision of financial education also led to modest immediate changes in financial autonomy and financial savviness, the latter measured as budgeting usage and having healthy shopping habits.

Credit bureau records from the experimental sample show that early improvements in financial literacy translate into limited, but positive long-lasting changes in financial behavior among high schools students. Three years after the launch of the intervention, the treatment did not lead to significant changes in students' credit or repayment behavior on the extensive margin: the likelihood to hold debt and the probability of being delinquent do not change due to the financial education program. However, among those with outstanding loans, the treatment reduced arrears by 20%. The magnitude of this contraction is economically important, especially when benchmarked against Kaiser et al. [2022], a recent meta-analysis on the effectiveness of financial education focusing on randomized experiments. When measured in SD, the treatment effect on arrears in the Peruvian pilot amounts to a 0.147 SD drop in the balance of delinquent debt, above the average effect sizes measured in Kaiser et al. [2022] for financial behavior outcomes in general (0.100 SD) and credit outcomes in particular (0.042 SD).

While the effect on arrears survives multiple hypothesis testing, it is not robust to an alternative specification of the debt variables in dollars as opposed to log-transformed amounts. This suggests that a few large debt balances drive the results in levels. The fragility of the results on arrears is also partially explained by power issues as only a very small share of the experimental sample holds outstanding debt by the time in which EQUIFAX records are observed. Even three years after the intervention, individuals in the experimental sample are still very young: their average ages are

between 18 and 20, depending on the cohort, and only 5% of the students in the control group have an outstanding loan. Focusing on this age group is relevant as it constitutes a key transition period when early mistakes can be costly and hard to amend, but one should keep in mind that the effect estimated on delinquent debt in the Peruvian context is present for a very small sub-sample. Previous studies that rely on course-requirement variations across US states and EQUIFAX data to measure credit management outcomes have focused either on older individuals or samples of similar age, but with denser credit histories.[2]

A side contribution of the study is the focus on the financial literacy and behavior outcomes of the instructors delivering the training. While some papers have looked at the evolution of teaching skills while teaching, far fewer have studied if instructors become more knowledgeable on a specific subject while teaching. Getting trained and imparting the financial education lessons improved teachers' financial skills by 0.32 SD, an impact twice as large as that identified among students. Teachers in the treatment group also recorded a 10% increase in the probability of saving as well as a 66% increase in their reported savings balances. The treatment did not affect their credit or repayment outcomes on the extensive margin. While the program led to a sizeable reduction in arrears among teachers with outstanding debt, this effect is not statistically significant.

As an increasing number of countries are running school-based pilots with the hopes of scaling up financial education programs in the school setting, it becomes critical to collect and share information on their effectiveness as well as their cost. At a cost per student of US$4.8, the Peruvian program yields a very low cost-effectiveness ratio in terms of students' financial literacy: the cost per student to improve average financial skills by one standard deviation amounts to US$30.7.

A growing body of research shows that financial knowledge is positively associated with financial outcomes. van Rooij et al. [2012] provide evidence of a strong positive association between financial literacy and net worth, while Bianchi [2018] shows that the most financially literate households make portfolio choices that yield 0.4% higher yearly returns, relative to an average return of 4.3%. The main channel put forward to rationalize these findings is that financial knowledge reduces the costs of gathering and processing information, improving financial choices, and expanding potential investment portfolio choices. In fact, van Rooij et al. [2012] claim that, in their setting, financial knowledge reduces information gaps, decreasing barriers to invest in the stock market.

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 [Jappelli and Padula, 2013; Lusardi et al., 2017]. The provision of school-based financial education reduces both pecuniary and opportunity costs since classes are imparted during regular school hours. Indeed, several papers have shown that school-based financial

[2]Cole et al. [2016]'s sample covers the age range 35–54 while Brown et al. [2016] focuses on individuals aged 19 through 29. Urban et al. [2020] focus on subjects aged between 18 and 20, but 35% of 18-to 19-year-olds in their sample have a credit history.

education programs have robust effects on children and youth's financial literacy [Frisancho, 2019; Kaiser and Menkhoff, 2019], particularly when they have a mandatory nature and incorporate the content during regular classes.

This paper contributes to the literature on the effectiveness of financial literacy programs for youth in at least two ways. First, it relies on high-stakes data to measure the long-lasting effects of financial education on financial behavior in an experimental setting. By complementing survey self-reported data with individual-level credit bureau records, this paper provides evidence on the impact of early investments in financial education on credit behavior three years after the intervention, posing an advantage over closely related studies such as Bruhn et al. [2016], Bover et al. [2018], Jamison et al. [2014], and Luhrmann et al. [2018]. While there are a handful of randomized experiments that have measured the effects of financial education on financial behavior after 24 months [Kaiser et al., 2022], these have all focused on adults. This study also contributes to the non-experimental literature that provides mixed evidence on the sustained impact of financial education programs on credit management using EQUIFAX data for the US. Cole et al. [2016] find that personal finance lessons have no effect on financial outcomes. In turn, Urban et al. [2020] show that financial education requirements are associated with fewer defaults and higher credit scores among individuals and Brown et al. [2016] find a reduction in the share of delinquent debt relative to total outstanding balance. This study is closer to Urban et al. [2020] due to its focus on a similar age range and it is aligned with their results on repayment behavior. Second, this paper tackles a recurring argument against the introduction of financial education lessons in the school setting: the substitution of time and resources away from other courses. Access to administrative academic records provides an opportunity to measure the program's opportunity cost in terms of individual grades and passing rates.

The remainder of this article is organized into five Sections. The following Section presents the experimental design and describes the data sources. Section 3 defines the outcomes variables and presents the estimation strategy. Section 4 presents the results, focusing on the immediate effects on financial literacy and financial behavior as well as on the medium-term effects on credit and repayment behavior. Section 5 concludes.

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 primary and secondary students by 2021. In this context, the Ministry of Education (MINEDU), the Superintendency of Banks and Insurance (SBS), and the Center of Studies (CEFI) of the Peruvian Association of Banks jointly developed a pilot to provide financial education to high school students. The implementation partners developed student workbooks for each of the last three high school grades (equivalent to 9th, 10th, and 11th grades in the United States) as well as a teacher's guide.

The partners safeguarded that the lessons were aligned both with the basic education curriculum and the 2015 Peruvian national strategy of financial education (PLANEF, for its name in Spanish, *Plan Nacional de Educacion Financiera*).[3] The lessons developed for the pilot were adapted to the specific content of the national curriculum for each grade but, in general, they focused on two main goals: fostering economic citizenship and providing knowledge about individual rights and duties to fully exercise citizenship.

The implementation partners 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). MINEDU encouraged teachers to attend the training sessions and school principals were requested to facilitate teacher participation in the sessions. Participants received both a transport subsidy (mostly in kind) and a full meal during the workshop. Teachers were also provided with a completion certificate that counted towards the evaluation of their performance as an investment in professional development.

The content of the workbooks varies by grade and it is fully detailed in Table A.1. The lessons provided to 9th graders focused on the differences between needs and resources as well as on budgeting. The lessons imparted to 10th graders focused on financial products and services and forward-looking choices. The curriculum for 11th graders covered topics on becoming a responsible financial consumer as well as access to, and use of, personal information in financial markets.

The sessions were delivered during the regular classes of the course "History, Geography, and Economics" (HGE). The workbooks and teachers' guide supported teachers in the delivery of the lessons using a mix of case analysis, exercises, group activities, and homework. The MINEDU instructed HGE teachers to incorporate the material in the Economics portion of the course and monitored their engagement with the program. Even though teachers were left to decide how to implement the sessions during the HGE course, they were provided some guidelines about the duration of the sessions covered in each workbook. The suggested number of hours required to cover all the lessons in the workbooks varied by grade, ranging from 16 (9th grade) to 24 (8th grade) to 32 (7th grade).[4] Since the content of the lessons was not incorporated as a stand-alone course in the official curriculum, MINEDU could not enforce full compliance of the teachers in the classroom. Nevertheless, once a teacher delivered the personal finance lessons within the HGE regular course, the content became subject to performance evaluation and was considered high-stakes from the students' point of view.

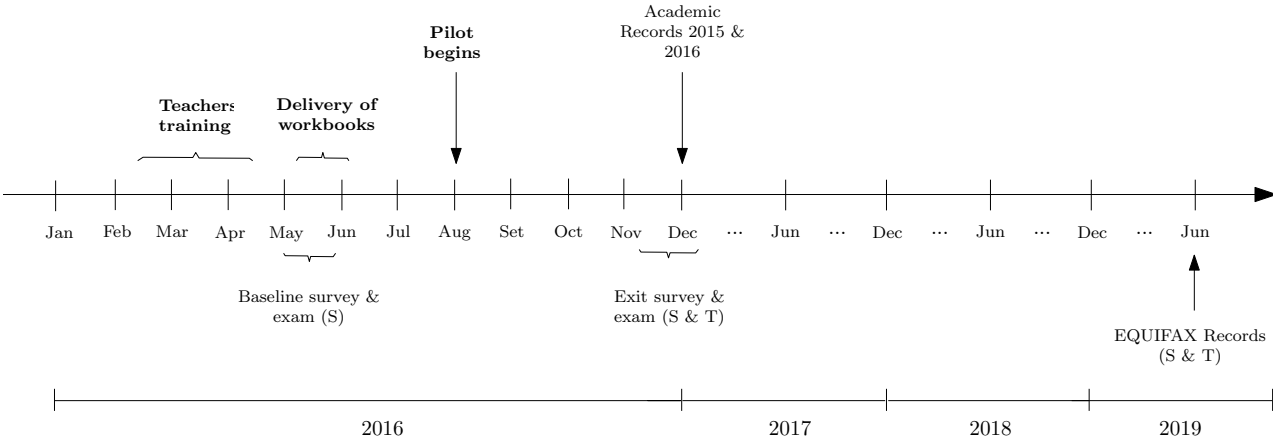
[3]Relative to its neighbors, Peru has been a pioneer in promoting the development of financial skills in school. It was the first country in the Latin American and Caribbean region to include financial education in the national curriculum as early as 2009. Under the curriculum, financial skills are to be developed to fulfill one of the 29 competencies that basic education seeks to provide: "responsibly manages economic resources". These efforts were further consolidated with the PLANEF, which was jointly developed by the SBS and the MINEDU. The strategy focused on 5 basic action areas: payments, savings, borrowing, insurance, and consumer protection.

[4]See Table A.1 in the Appendix. Compared to other school-based interventions targeting youth, the pilot in Peru provides a very high intensity treatment in terms of hours of exposure, surpassed only by the program studied in [Bruhn et al., 2016] which is a clear outlier with an average of 108 hours of teaching required to deliver all the material included in the program's textbooks. In comparison, the Peruvian program was more compact, but the number of hours of exposure surpassed most of the other programs targeting youth that have been experimentally evaluated. See Table A.2 in the Appendix.

The treatment was only fully implemented in all grades and regions during 2016. During 2017, the implementation partners had no resources to fund all activities, but the workbooks were still printed and distributed to the treatment schools. The MINEDU did not provide specific instructions to continue with the delivery of the lessons nor did it continue to offer teacher training sessions. Still, teachers in treatment schools may have continued to teach the material during the HGE classes even if no specific guidelines, monitoring, nor incentives were provided. Unfortunately, there are no administrative or evaluation records available to check teachers' engagement with the personal finance material after 2016.

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 by the SBS and the MINEDU 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 and was completed successfully in all treated schools by July. The delivery of the sessions in class began during the second half of the 2016 school year; August through December. To ensure that compliance levels were high, regular monitoring phone calls took place September through November.

Figure 1: Study Timeline



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

Treated and control schools were visited twice in 2016 to collect survey data and measure the financial skills of both students and teachers. Self-administered baseline surveys and financial literacy entry exams for students were simultaneously collected during May. Exit surveys and exams for students and teachers were conducted toward the end of the 2016 academic year. Individual-level data on grades and passing rates for the 2015 and 2016 academic years were provided by the MINEDU for all the schools in our sample. Credit bureau data on students and teachers were obtained from EQUIFAX, the leading private credit bureau in Peru. Students and teachers in our

survey sample were searched in the bureau's records in June 2019.

2.2 Sample Selection and Randomization

The implementation partners decided to focus on 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 and implementation constraints, the sampling frame was limited depending on schools' proximity to cities and a few 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), yielding a restricted universe of 308 eligible schools.[5]

The sample of eligible schools was stratified by region. Following Bruhn and McKenzie [2009] and Bruhn et al. [2016], schools were paired by their similarity within each of the six strata.[6] This procedure returned 150 matched pairs, yielding a final 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-A.5 provide basic descriptive statistics at the student and teacher level, as well as balancing tests of the randomization (both at endline and baseline, in the case of students). Consistent with the random treatment assignment, very few significant differences are detected across groups. In any case, the estimation of treatment impacts considers the effect of background controls and, whenever available, initial levels of the dependent variable.

2.3 Data and Measurement

(a) Survey and Exam Data. Survey and exam data were collected for students and teachers in the 300 schools of the experimental sample. Within each school, one classroom from each targeted grade was chosen at random to conduct the surveys and apply the exams. The main study sample comprises about 20,000 students (from 900 classrooms) and 453 teachers.

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 survey also measures students' school engagement [Hart et al., 2011] and collects data on previous exposure to financial education programs. Financial behavior is measured in the survey through several constructs: holding savings, budgeting, consumption and saving habits, and financial autonomy [Bruhn et al., 2016]. The survey also measured monthly cash flows derived from different income sources including allowances, gifts from family and friends, and labor. Despite their young age, Table A.4 shows that 40% of the students at baseline performed

[5]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 0.1 SD, 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.

[6]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 9th, 10th, and 11th grades; dropout rate; passing rate; and whether the school belongs to the experimental sample of any other ongoing pilot.

paid work activities. These students record an average (median) monthly income of US$102.6 (US$33.2), with a third of their earnings coming from labor. Even among those who do not claim to work, average (median) monthly income amounts to US$88.6 (US$29.9). 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.

The survey questionnaire applied to teachers at endline was very similar to the students' instrument, but additional questions were added to capture their professional background and experience, as well as their savings balances. To make room for these additional questions, questions on income were dropped. Teachers in the treatment group completed an additional survey module that inquired 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$tart Coalition Survey of High School Seniors and College Students [Mandell, 2009]. The remaining questions tested students on the topics covered in each grade-specific workbook. Most questions were drawn from an entry exam designed by the implementing partners and administered to teachers in the treatment group who attended at least one of the training sessions. A few questions were developed by the author to cover all topics included in the workbooks. The same grade-specific exam was administered at baseline and endline. The exit exam taken by teachers was developed by the author and included the four questions from the Jump$tart questionnaire as well as questions from the students' exams for 9th grade (4), 10th grade (4), and 11th grade (3). Teachers had no access to the students' exam questionnaires at baseline and the exit exam was applied to teachers and students during the same school visit. This ensures that teachers could not teach to the exam during the school year. The psychometric properties of the exam based on students' baseline data are presented in Table B.1.

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 survey records. Indeed, two pairs of schools from the original experimental sample are excluded from the analysis due to non-response either in the baseline or the endline survey. The main analysis sample thus consists of 296 schools, with a total population of approximately 60,000 students. Baseline survey records are available for 20,622 students (7,003; 6,841; and 6,788 in 9th, 10th, and 11th grade, respectively), roughly a third of the targeted population. The exit survey and exam were applied to 19,462 students (6,627; 6,489; and 6,346 in 9th, 10th, and 11th grade, respectively) and 453 teachers. The attrition rate between baseline and endline among students is 17%, but it is not differential by treatment status (see Table A.6 in the Appendix). The sample of interest to evaluate the impact of the intervention includes all students with records in the follow-up survey and exam, as they have data on the outcome variables after exposure to the intervention.[7]

[7]Table A.3 presents the balance check for the endline sample. 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.7, the share of missing records varies depending on the construct and the survey round; however, it is not

(b) School Academic Records. MINEDU's academic records provide data for all high school students enrolled in any of the 300 schools of the experimental sample. These data contain individual-level information on cumulative grades by course and grade progression for two consecutive academic years, 2015 and 2016. The success rate when matching the exit survey and exam data with performance records from 2015 and 2016 is extremely high at 91% and 98%, respectively.

(c) Credit Bureau Records. Credit outcomes three years after the intervention were provided by EQUIFAX, a private credit bureau that concentrates credit data from almost all lenders in the Peruvian credit market as well as non-credit information that may be relevant to determine a person's ability to repay a loan. EQUIFAX collects credit information from all banks and most microfinance institutions. These records are very similar to those obtained by Urban et al. [2020], who relied on credit report data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel (CCP) to track young individuals.

EQUIFAX's data contains records on all individuals in Peru who have reached legal age, irrespective of previous access to credit from financial institutions or other creditors. The National Identification and Civil Status Registry provides the bureau with monthly updates of the roster of people who are over age 18 in the country. EQUIFAX's records capture an individual's credit standing at the time in which she is searched.

The author provided EQUIFAX with individual identifiers (names and national IDs) collected in the survey and the bureau used these to match students and teachers with their records in June 2019. This snapshot provides information on loan balances by repayment status of the loan (i.e., current and past due debt), source of the funds (i.e., type of lender), and type of loan according to intended purpose (i.e., productive loans funding micro-enterprise and small business and non-productive loans including consumption loans, credit card debt, mortgages, and auto financing). Although EQUIFAX produces individual credit scores that they share with their clients for a fee, this information was not shared with the author. In addition to loan balances, the credit bureau's data also capture negative records corresponding to delinquency on non-credit related bills (e.g. cellphone, water, electricity, gas, etc.), taxes, or credit cards balances. Negative signals from non-credit bills stay active in the bureau's database until the pending balance has been paid off or until five years have passed since the service provider has reported a late or missed payment. By law, EQUIFAX has to stop disclosing negative records after this exposure period expires, even if the debt has not been collected.[8] As opposed to the records on loans, which provide a snapshot of individual credit balances, the negative signals from non-credit bills provide retrospective information, although left censored to five years in the past.

Focusing on the endline sample, the match rate between survey and EQUIFAX records is

significantly different by treatment arm.

[8]Lenders observe negative records in non credit-related bills when they search for potential borrowers in the database. They directly incorporate this information and the records on late/missed payments of loans in their own risk assessment models. In fact, several financial institutions, particularly microcredit institutions, require lack of negative records to be eligible as a client. Negative signals originating from credit card debt, non credit-related bills, and arrears/default in loans are all incorporated in the credit scoring models developed by EQUIFAX.

98% among students and 96% among teachers. The discrepancy between the survey and the administrative data may be due to mistakes in the identifiers collected in the survey and/or problems in the update of the national registry. By June 2019, 5% of the students in the control group had an outstanding loan. This number is in line with data from FINDEX that shows that, in 2017, only 8% of the people in the age bracket 15 to 24 borrowed money from a financial institution in Peru [Demirguc-Kunt et al., 2018]. Virtually all the loans contracted by the students in the sample come from regulated banking institutions such as banks and microfinance lenders (i.e., Municipal Savings and Loans Associations, Rural Savings and Loans Associations, and other entities that promote the development of small businesses). In turn, by June 2019 63% of the teachers in the control group had an outstanding loan with a banking institution. Among those with debts, 92% of them had loans with regulated lenders and 39% had debts with microfinance NGOs and cooperatives.

2.4 Teachers' Compliance with the Treatment

Teachers were encouraged to attend the training sessions and to deliver the material in the classroom. Even though the MINEDU could not impose either of these activities as mandatory, teachers' engagement with the pilot was high. About 73% of the teachers in the treatment group attended at least one training session and 43% had perfect attendance. 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 they had taught all the lessons and 21% had covered part of the material by the end of the school year. A third of the teachers reported not teaching the workbook lessons at all.

Several factors explain the relatively high compliance levels of teachers with the treatment. On one hand, these were teachers who were already teaching Economics as part of the content of the HGE course. It is thus very likely that they had greater interest in these topics relative to the rest of the school staff. Second, the MINEDU designed an incentive scheme that motivated teachers' participation in the training by providing a completion certificate. This was valuable for the teachers as the educational system in Peru promotes them based on merit and one of the criteria to evaluate their performance relates to their investments in professional development through refresher courses, training, certifications, and graduate studies. Third, the teacher training's content was in itself attractive for the participants. Professional facilitators knowledgeable on personal finances and with several years of experience in delivering trainings led the sessions and delivered the content in a very interactive and clear format. Teachers received lessons on the content of the three grades all together, which allowed them to benefit from the progressive building of financial knowledge while covering the three curricula. Finally, the materials developed to deliver the classroom lessons were kept simple in the exposition of concepts and provided several opportunities to promote active learning both during the teacher training and the delivery of the lessons to students.

Survey data reveals that, on average, HGE 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. Unfortunately, grade effects by subtopic cannot be estimated using administrative records as teachers allocate a single grade for the HGE course at the end of each academic year.

3 Empirical Analysis

3.1 Outcome Variables

(a) Students. Scores in the financial literacy exams are measured at endline and standardized at the grade level, using the distribution of the control group in the baseline exam as a benchmark. Immediate changes in financial behavior are also captured through three outcomes measured in the endline survey: financial autonomy; the probability of saving; and financial savviness. Unfortunately, the students' survey did not collect information on saving balances. The financial autonomy index [Bruhn et al., 2016] captures individual responses to questions aiming at measuring whether students felt empowered, confident, and capable of making independent financial choices and influencing their households' financial decisions. Financial savviness is also measured as an index that aggregates four binary outcomes: keeps a budget, saves before buying something that cannot be afforded, compares prices, and bargains before shopping.[9] Both indexes are computed as an equally weighted average of the z-scores of each component. These z-scores are obtained by subtracting the control group mean and dividing by the control group standard deviation.

Relying on administrative records on students' credit behavior, six main outcomes are constructed. First, three outcomes measure impacts on the extensive margin of credit and repayment outcomes: the probability of having an outstanding loan, either current or past due; the probability of having a loan in arrears; and the probability of having arrears in a non-credit bill or a credit card statement. To capture the effects of the treatment on the intensive margin, three additional outcomes are measured: current and past due debt, conditional on having an outstanding loan; and past due debt in non-credit bills, conditional on having arrears in a non-credit bill or a credit card statement. All debt variables are log-transformed. The logarithmic transformation is convenient due to the skewness of the debt variables (long right tails), even after conditioning on having outstanding loans. Brown et al. [2016] perform the same transformation for mortgage loans in their sample. Taking logs also makes much more sense to think about changes in debt in a multiplicative scale rather than an arithmetic scale.

The effect of the intervention on academic performance is assessed by focusing on GPAs, the probability of passing a grade (or graduate, in the case of 11th graders), and aspirations to get a university degree. Grades and grade progression are constructed using school administrative records, while the latter comes from survey data. Cumulative grades and grades by course (math, verbal,

[9] While "comparing prices" and "bargaining before shopping" are indicators of potential improved consumer welfare due to a greater likelihood to pay better final prices, "saving before buying something that cannot be afforded", may not always improve financial well-being, particularly if there are investment opportunities that can be missed. However, given the young age of the students at baseline, it is not very likely that they face such situations. Survey data indicates that students' expenditures are mostly allocated to clothing, school supplies, and household staples.

and HGE) are observed at the end of the intervention year and standardized at the grade/course level, using the distribution of the control group in 2015 as a benchmark. Grades are also normalized by school quality to make them comparable across schools (see Appendix B.4). Aspirations are measured as a dichotomous variable that is equal to one if the highest expected degree is university.

(b) Teachers. Financial literacy is measured using exit exam scores, which are standardized using the distribution of the control group as a benchmark. Teachers' financial behavior is measured relying on both survey and credit bureau data. Survey-based behavior outcomes include those used for students as well as savings balances. Teachers' credit and repayment behavior is evaluated using the same the outcome variables defined for students in the EQUIFAX database.

3.2 Estimation Strategy

The impact of the financial education program on different outcomes is measured as the difference across treatment arms, captured from an intention-to-treat (ITT), 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 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' financial literacy, academic performance, and self-reported outcomes gathered from the survey data. Implementation of an analysis of covariance (ANCOVA) to estimate the treatment effects leads to large improvements in power compared to a difference-in-difference specification [McKenzie, 2012].

The impact of the treatment is measured by β, the coefficient on the indicator of treatment status, T_{jp}, which is equal to one whenever the school was randomized into the treatment group and zero otherwise. 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. Note that students' and teachers' results are not sensitive to the exclusion of controls (results available upon request). The Romano-Wolf correction is implemented for each family of outcomes to deal with potential issues of simultaneous inference [Romano and Wolf, 2005].

The main specification corresponds to ITT effects to keep the results for the Peruvian pilot comparable to those presented in similar studies [Bover et al., 2018; Berry et al., 2018; Bruhn et al., 2016; Batty et al., 2020; Jamison et al., 2014; Luhrmann et al., 2015]. ITT effects also provide a more conservative estimate of the effects on the beneficiaries, while taking into account issues of non-compliance in the field. This is particularly important in the estimation of the treatment effects on outcomes measured after the endline. When looking at financial literacy and behavior at endline, there are no differences in exposure across grades. However, when assessing the effects of the treatment on credit behavior three years later, potential variation in the years of exposure to the program arises. Since neither teacher nor school-level records of compliance are available due to lack of administrative or survey records between 2017 and 2018, ITT effects are more suitable to

measure the impact on outcomes measured after 2016. This approach is feasible as the treatment assignment at the school level was respected throughout the analysis period (between 2016 and 2019).

The intervention did not have perfect compliance levels within the treatment group (see subsection 2.4). 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. Despite the potential variation in the intensity of exposure after 2016, average treatment on the treated (ATT) effects can still be estimated relying on self-reported data on teachers' coverage of the lessons in the classroom. Compliance is thus defined at the school level by Z_{jp}, which is equal to one if at least one teacher in the grade self-reports to have partially or fully covered the curriculum in the classroom. ATT effects can then be obtained from estimating β^{TOT} by instrumenting Z_{jp} with the random assignment of the treatment:

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

Unfortunately, it is impossible to link teacher training attendance records to grades or classes as these administrative data do not include such identifiers. Note that teacher attendance defines effective treatment only during the first year of the intervention. Therefore, ATT effects will only be estimated for outcomes measured during that same year, 2016.

4 Results

4.1 Treatment Impacts on Students

(a) Immediate Effects on Financial Literacy and Financial Behavior (by Endline, 2016). Column 1 in Table 1 presents the effects of the treatment on financial literacy at endline. Overall, the program improved high school students' scores in the exit financial literacy exam by 0.16 SD. These average gains are closely aligned with the experimental evidence available to date on school-based programs. Figure 2 presents the results from a meta-analysis conducted with 10 experimental studies targeting children and youth and confirms that the impact of the Peruvian high school program on financial knowledge is very close to the average effect size in the literature, estimated at 0.18 SD and significant at the 95% confidence interval. The results are particularly comparable to those reported by similar programs targeting high school students in Brazil [Bruhn et al., 2016] and Spain [Bover et al., 2018].

Financial knowledge results may be driven by teachers teaching to the test, especially since some of the questions on the students' exam were based on the entry exam that teachers in treatment schools took at the beginning of the training workshops (see Sub-Section 2.3). Nevertheless, average treatment effects are quite robust (coeff=0.12 SD, p-value=0) even when financial skills are measured using only the questions developed either by Jump$tart or by the author.

One recurring argument against the introduction of financial education lessons in the school

Table 1: Effect on Students' Financial Literacy and Financial Behavior

	Financial Literacy (1)	Financial Autonomy (2)	Pr(Saving) (3)	Financial Savviness (4)
Treatment	0.156***† † †	0.024**†	0.013	0.030***† † †
	(0.023)	(0.010)	(0.009)	(0.008)
Number of Observations	19462	16675	13917	17116
Number of Schools	296	296	296	296
Mean in Control	–0.005	–0.004	0.593	–0.009

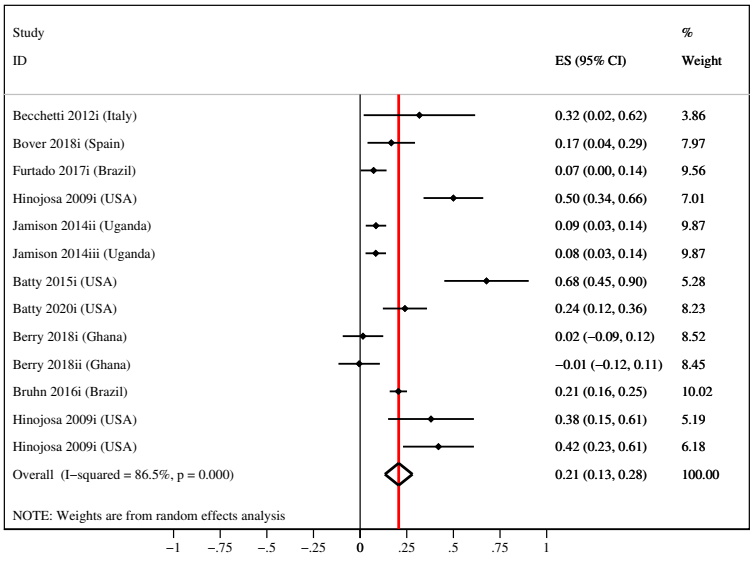
Note: All outcomes are measured at the end of the 2016 academic year. Scores in the financial literacy exams are standardized at the grade level, using the distribution of the control group in the baseline exam as a benchmark. The financial autonomy index aggregates fifteen binary variables capturing whether students felt empowered, confident, and capable of making independent financial choices and influencing their households' financial decisions. The probability of saving captures both formal and informal savings decisions. The financial savviness index aggregates four binary variables measuring if the student keeps a budget, saves before buying something that cannot be afforded, compares prices, and bargains before shopping. The financial autonomy and financial savviness indexes are computed as an equally weighted average of the z-scores of each of its components. These z-scores are obtained by subtracting the control group mean and dividing by the control group standard deviation. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for financial literacy, financial autonomy, probability of saving, and financial savviness. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs 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. The value of the dependent variable at baseline is also included as a control.

setting is the substitution of time and resources away from other courses, potentially sacrificing student learning in other areas. Columns 1-4 in Table A.8 suggest that the opportunity cost of introducing personal finance content is not high enough to hinder academic performance in terms of grades. At the end of the 2016 academic year, the treatment has no significant effect neither on cumulative grades nor on specific course grades. If anything, the personal finance lessons slightly boost language performance, without any deterioration of HGE grades or math grades.

A few studies on youth have shown that financial education may influence adolescents' intertemporal choices. For instance, Luhrmann et al. [2018] show that the provision of school-based financial education in German high schools led treated students to make more time-consistent choices, increasing the quality of intertemporal decision-making. Exposure to financial education may even foster young people's investment in future schooling if financial education lessons impact students' perceptions and valuations of alternative future trajectories. However, Column 5 in Table A.8 shows that the intervention in Peru does not yield any significant effects on grade promotion. Column 6 shows that students' aspirations to pursue tertiary education also remain unchanged, which could respond to already high baseline levels (83% of the students in the control group expect to obtain a university degree).

Since the endline survey takes place at the end of the 2016 academic year, there is a limited range of students' self-reported financial behaviors that can be measured by then as they are still underage and have limited financial services available to them. However, students still manage a budget and

Figure 2: The Impact of Financial Education on Financial Literacy: Average Effect Sizes of Programs Targeting Youth



NOTE: Author's own elaboration. Estimates come from a random effects meta-analysis regression for a sample of recent experimental studies focusing on youth: Batty et al. [2015, 2020]; Becchetti and Pisani [2012]; Bover et al. [2018]; Bruhn et al. [2016]; Furtado et al. [2017]; Hinojosa et al. [2009]; Berry et al. [2018]; Jamison et al. [2014].

make shopping decisions that can be shaped by the treatment.[10] The immediate impacts of the treatment on financial behavior are measured through three survey outcomes that aim at capturing changes in students' daily habits and financial behavior at their young age. Columns 2-4 in Table 1 show that the gains measured in terms of students' financial literacy have modest trickle down effects on short-run financial behavior. The treatment does not lead to significant changes in the probability of saving, but it does seem to have modest impacts on financial autonomy and financial savviness, the latter capturing students' budgeting and shopping habits. Relative to the control, the treatment group records 0.02 SD and 0.03 SD gains in their levels of financial autonomy and savviness, respectively.

Appendix A presents ATT effects for students' outcomes measured during 2016, where effective compliance is defined at the school level based on teachers' self-reporting of the coverage of lessons. As expected, these are even larger than the estimated ITT effects. Additionally, all the outcomes measuring financial behavior in the short-run (i.e., financial autonomy, financial savviness, and the

[10] As mentioned in Subsection 2.3, students in the sample perceive non-negligible incomes, even when they are not working. Detailed high frequency data from a financial diaries study with a sub-sample of the experimental sample in Piura shows that youth have active and modestly sophisticated financial lives. Over a period of 6 months, the average youth records an average of 14 monthly financial transactions. While transactions related to expenses represent over half of the total recorded transactions, income flows and financial tools (savings and loans operations) represent 38.1 and 9.7 percent of the recorded transactions, respectively. In terms of magnitude, income flows represent the largest share of youth's budget, with 46.2 percent of the total transactional value recorded over the entire six-month period [Frisancho et al., 2021].

probability of saving) become significant and survive multiple hypothesis testing (see Table A.9).

At a cost per student of US$4.8, the program yields a very low cost-effectiveness ratio in terms of students' financial literacy: the cost per student to improve average financial skills by one standard deviation is US$30.7 (see Appendix B.3 for more details).

(b) Medium-Term Effects on Financial Behavior (by June 2019). Financial education is expected to increase financial literacy by reducing the costs of gathering and processing information when making financial choices, which can then translate into actual changes in financial behavior. While the previous results showed modest changes in that respect, several concerns arise in relation to survey-based outcomes. First, they are measured early during the students' life cycle as economic agents. Even though youth in the experimental sample actively engage in financial transactions while at school, the volume and diversity of these is still limited. Second, survey outcomes are self-reported and subject to misreporting biases, particularly social-desirability biases in the treatment group. Third, survey-based outcomes are measured as soon as the intervention concludes and are thus unable to capture long-lasting effects of the financial education program on financial behavior.

These potential issues are overcome with administrative records on students' *actual* credit behavior. These data reflect credit and repayment choices that the students made three years after the intervention, giving a more accurate measure of their financial behavior over time. Table 2 presents the estimated treatment impacts on credit and repayment behavior by June 2019. Three years after the launch of the intervention, treated students do not significantly differ from control group students in terms of their likelihood to hold outstanding debt or their likelihood to have loans in arrears (see columns 1 and 2). Similarly, the treatment does not affect the probability of having arrears in non-credit bills or credit card debt (column 3).

Still, the treatment may yield an impact on students' debt portfolios and repayment outcomes. If students were becoming indebted without taking into account their repayment capacity or fully understanding the conditions that they were being offered, the provision of financial education can actually reduce their demand for credit on the intensive margin. The treatment may also affect the type of loans students take as they become more familiar with concepts such as returns, investments, and interest rates. For instance, Stoddard and Urban [2020], shows that personal finance graduation requirements increase students' likelihood to use federal aid and reduce private loan balances when making choices about college funding. Their results also show that students exposed to financial literacy mandates tend to shy away from higher cost borrowing (i.e., credit card loans).

The results in columns 1-3 show that the treatment does not affect the probability of having a loan or having arrears. Since there is no sample selection into borrowing, columns 4 and 5 present the treatment impacts on current debt and loan arrears when conditioning on the sub-sample of students who had an outstanding loan in June 2019. Similarly, column 6 presents the treatment effect on arrears corresponding to non-credit bills and credit card debt, conditional on having such arrears by June 2019. While the program fails to impact the size of the amount in arrears from

non-credit bills, it significantly reduces loan arrears by almost 20%. Importantly, the treatment does not impact actual usage of current debt, which indicates that credit usage is not hindered.

The impact on arrears is aligned with Brown et al. [2016], who rely on event studies and find that exposure to financial education during high school reduced the proportion of debt balance that is delinquent. While their estimates only yield a 2% reduction, their baseline model is not directly comparable as it includes all individuals in the sample, even those without debts. The improvement in repayment outcomes among active borrowers in Peru is also in line with Urban et al. [2020], who implement a synthetic control strategy and find that financial education course requirements in high school reduced 30-day delinquency rates by 40% in Texas and 50% in Georgia. In contrast, Cole et al. [2016] find that state-mandated personal finance high-school courses do not influence credit behavior during adulthood (ages 35-54). Their analysis focuses on policy changes that took place much earlier than those analyzed in Brown et al. [2016] or Urban et al. [2020] and thus focus on older adults. Thus, their results do not necessarily imply that there are no sustained effects of financial education targeting youth, but that these effects may dissipate over longer time-horizons. Nevertheless, it is hard to benchmark the magnitude of the results on repayment for the Peruvian study against these non-experimental papers focusing on the US case. On one hand, differences in the estimated impacts may be related to a focus on a sample coming from a developed country where agents from the same age are heavily reliant on debt. On the other hand, the variation exploited by these studies is not exogenous and raises several concerns.

Table 2: Effects on Students' Credit and Delinquency Outcomes

	Pr(Debt)	Pr(Arrears)		Log(Current	Log(Debt Arrears)	
		Loans	Other bills	Debt)	Loans	Other bills
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	–0.002	–0.001	0.002	0.161	–0.226**†	–0.038
	(0.003)	(0.001)	(0.002)	(0.107)	(0.106)	(0.117)
Number of Observations	19113	19113	19113	902	902	658
Number of Schools	296	296	296	249	249	251
Mean in Control	0.049	0.006	0.034	5.203	0.571	3.960

Note: Students' credit and default outcomes measured in June 2019. Current debt and debt in arrears are measured in US dollars and log-transformed. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for two families of outcomes: (i) probability of having debt, probability of having loan arrears, and probability of having arrears from other bills and (ii) current debt and debt arrears in loans. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs 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.

(c) Discussion. In sum, the school-based financial education program effectively improved high school students' financial literacy levels with low opportunity costs in terms of academic outcomes. These knowledge gains had modest trickle down effects on short-run financial behavior. Financial

education lessons do not lead to selection into borrowing or into being delinquent, but, among those with debt, they significantly reduce students' arrears by 20%. This effect reflects better repayment behavior on the intensive margin, which is aligned with the positive impacts of the intervention on financial autonomy and financial savviness in the short-run.

The effect on arrears survives multiple hypothesis testing, it is not robust to an alternative specification of the dependent variable in dollars as opposed to log-transformed amounts. The estimated treatment effects become very noisy when using the debt variables in levels, regardless of the use of controls or the inclusion of non-borrowers as zeros (results available upon request). Still, all specifications in levels yield a sizable (though non-significant) reduction of arrears. This suggests that a few large debt balances drive the results when arrears are not log-transformed. Part of the problem is related to power issues as only a very small share of the experimental sample holds outstanding debt by the time in which the EQUIFAX records are observed.

Note that, even three years after the intervention, individuals in the experimental sample are still very young: their average ages are between 18 and 20, depending on the cohort, and only 5% of the students in the control group have an outstanding loan. Thus, the effect found on delinquent debt is present for a very small sub-sample.

The effect on arrears is economically meaningful in size, amounting to a 0.147 SD drop in the balance of delinquent debt. It is particularly large when benchmarked against Kaiser et al. [2022], a recent meta-analysis on the causal impact of financial education programs. The authors estimate an average effect size of 0.042 SD on credit outcomes, the smallest when compared to other downstream financial behaviors such as budgeting (0.147 SD), saving (0.097 SD), insurance (0.059 SD), and remittances (0.047 SD). Even though the meta-analysis is not specific to young adults (in fact, 72.4% of their sampled studies focus on adults above age 25) and includes studies with different time-horizons and data sources to measure financial outcomes, it still serves as a nice reference point for the treatment effects obtained when using credit bureau data.

The reduction in arrears may have important implications on youth's future access to credit and borrowing conditions. Young adults in the 18-25 age bracket have limited access to credit from financial institutions due to their low levels of income and asset accumulation patterns at early stages of their life cycle. Lack of access to tailored financial services as well as their inexperience and low financial literacy levels can lead to usage of expensive sources of credit and high delinquency rates relative to other age groups. The treatment does not improve their engagement with the credit market and has no impact on their delinquency rates. However, it is effective to improve repayment behavior of those few who are active borrowers at an early age, fostering better performance at the onset of their credit histories.

A recurrent concern in the economics of education literature is that interventions that aim at fostering learning end up widening initial inequalities due to heterogeneous effects of the treatment. This same concern applies to financial education programs, but very few studies focusing on youth estimate heterogeneous treatment impacts. A notable exception is Stoddard and Urban [2020], who focus on heterogeneity by family background including disposable income and race or

ethnicity. Kaiser and Menkhoff [2019] show results for sub-samples of studies in their meta-analysis to test for differential effects by country per capita income, delay in measurement, intensity, and class size. However, none of the experiments or quasi-experiments included in their review presents heterogeneous effects by individual characteristics. Appendix B.2 presents the analysis of heterogeneous effects by sex, socioeconomic status, and baseline financial literacy scores. In general, the impact of school-based financial education on financial literacy and short-run financial behavior seems to be very inclusive: treatment effects are quite similar by sex or baseline score. However, students from households with a higher asset index benefit relatively more from the financial literacy lessons: a one standard deviation increase in the asset index significantly raises the treatment effect on financial literacy by 0.05 SD. In the medium run, a higher asset index is also associated with a significant effect on the probability of having debt. While the average treatment effect on the extensive margin is null, a one standard deviation increase in the asset index yields a 0.007 increase in the probability of having outstanding debt, a 14% increase relative to the control. Since this effect is not accompanied by an increase in the probability of having arrears, this is potentially a welfare-improving effect. Data from the control group indicates that students from households with higher asset indexes have a relatively lower probability of having an outstanding loan. Thus, this differential effect on does not lead to increased inequality by socioeconomic status in terms of access to credit.

4.2 Treatment Impacts on Teachers

(a) Immediate Effects on Financial Literacy and Financial Behavior (by Endline, 2016). Column 1 in Table 3 presents initial evidence on the first-hand effect of the financial education program on teachers' financial literacy. On average, the treatment generates knowledge gains of 0.32 SD. This is a sizeable effect, both when compared to previous meta-analysis on the effects of financial education on adults [Fernandes et al., 2014; Miller et al., 2014] as well as more recent and favorable ones [Kaiser and Menkhoff, 2017; Kaiser et al., 2022].

The financial literacy gains accrued by teachers translate into important changes in their savings behavior. Column 3 in Table 3 shows that teachers in the treatment group become 8.7 percentage points more likely to save. The program also led to higher savings balances: relative to the control group, teachers in the treatment group increase their savings balances by two thirds (see column 4). The treatment did not translate into significant changes in financial autonomy (see column 2), nor did it lead to changes in financial savviness (see column 5).

Savings behavior is a self-reported measure which may be influenced by social desirability bias, especially after being exposed to the financial education material. Unfortunately, this cannot be directly tested as administrative records on savings behavior do not exist. Nevertheless, the treatment effect on the probability of saving is larger than similar estimates from studies evaluating the impact of financial education for adults on savings, which suggests that it cannot be fully explained by measurement error. For instance, Seshan and Yang [2012] find that exposure to a financial literacy workshop does not affect the probability of saving among Indian migrants in Qatar

Table 3: Effect on Teachers' Financial Literacy and Financial Behavior

	Financial Literacy (1)	Financial Autonomy (2)	Pr(Saving) (3)	Savings Balance (4)	Financial Savviness
Treatment	0.320***††	0.087	0.087**††	516.7*	0.009
	(0.100)	(0.068)	(0.035)	(268.4)	(0.044)
Number of Observations	417	347	334	334	335
Number of Schools	250	214	214	214	214
Mean in Control	0.025	–0.006	0.839	789.1	0.011

Note: All outcomes are measured at the end of the 2016 academic year. Scores in the financial literacy exams are standardized at the grade level, using the distribution of the control group in the baseline exam as a benchmark. The financial autonomy index aggregates fifteen binary variables capturing whether students felt empowered, confident, and capable of making independent financial choices and influencing their households' financial decisions. The probability of saving captures both formal and informal savings decisions. Savings balances are expressed in US dollars. The financial savviness index aggregates four binary variables measuring if the student keeps a budget, saves before buying something that cannot be afforded, compares prices, and bargains before shopping. The financial autonomy and financial savviness indexes are computed as an equally weighted average of the z-scores of each of its components. These z-scores are obtained by subtracting the control group mean and dividing by the control group standard deviation. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for financial literacy, financial autonomy, probability of saving, savings balance, and financial savviness. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs of schools and the following set of controls: sex, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

while Cole et al. [2011] identify no effect of a financial education program on the probability of opening a savings account among 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 of saving among microfinance clients in the Dominican Republic.

As mentioned before, 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. Table A.10 presents the ATT effects on teachers' outcomes by the degree of repetition of the content, confirming that the more teachers deliver the content, the greater the impact on their levels of financial literacy and the improvement in their financial downstream financial behavior.

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 only informative and should not be regarded as one yielding causal effects. However, even though selection into teaching 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' observables affect the probability of teaching the lessons in the classroom (see Table A.11).

(b) Medium-Term Effects on Financial Behavior (by June 2019). Table 4 presents the treatment impacts on teachers' credit behavior three years after the launch of the intervention. First, notice that teachers have far more access to credit than the average Peruvian: more than half of them have outstanding debt balances, while only one in three adults borrow from formal credit sources in Peru. This high level of bancarization among teachers may be explained by the quality and formality of their jobs. As contract teachers, public servants receive their wages into a bank account in the national bank, which may enable them to access credit from other lenders in the market.

The treatment did not affect teachers' likelihood to hold an outstanding loan in June 2019 (see column 1) or the probability of having arrears (see columns 2 and 3). However, it led to a decrease in debt arrears among those with outstanding loans (column 5). This seems to be a sizeable effect when compared to the baseline levels in the control, but it is not statistically significant due to the reduced size of the sample of teachers.

Overall, the treatment does not have an effect on teachers' credit management outcomes. Since treated teachers increased their probability of saving and their savings balances, one could expect to see a drop in their debt levels. The lack of an impact on credit outcomes either on the extensive nor on the intensive margin may indicate that teachers face credit constrains and that they resort to savings to deal with them.

Table 4: Effects on Teachers' Credit and Delinquency Outcomes

	Pr(Debt)	Pr(Arrears)		Log(Current	Log(Debt Arrears)	
		Loans	Other bills	Debt)	Loans	Other bills
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.022	-0.012	0.001	-0.010	-0.212	1.035
	(0.040)	(0.013)	(0.039)	(0.246)	(0.132)	(0.826)
Number of Observations	414	414	414	257	257	101
Number of schools	249	249	249	188	188	88
Mean in Control	0.605	0.029	0.254	8.213	0.281	4.542

Note: Teachers' credit and default outcomes measured in June 2019. Current debt and debt in arrears are measured in US dollars and log-transformed. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for two families of outcomes: (i) probability of having debt, probability of having loan arrears, and probability of having arrears from other bills and (ii) current debt and debt arrears in loans. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs of schools and the following set of controls: sex, type of contract, total hours teaching, experience, degree in social sciences, and postgraduate studies.

(c) Discussion. All in all, the program led to significant and sizeable treatment effects on financial literacy and savings behavior among teachers. However, these effects do not lead to sustained changes in credit and repayment behavior.

While the treatment impacts on financial literacy are aligned with those identified among students, the effects on financial behavior differ in the short and the medium run. As adults, teachers have had more time than students to invest in financial literacy during their life cycle. Exposure

to financial education offers them the opportunity to access additional knowledge and information, but it will only be effective to change their financial behavior along the dimensions in which they face the largest pre-treatment knowledge gaps. The results for teachers thus suggest that they faced relatively larger knowledge gaps on topics such as the importance of savings and mechanisms to save, and less so in the case of healthy budgeting, shopping practices, or credit management.

The results on teachers contribute to answer a more general and often over-looked question in the education and human capital accumulation literature, which is whether someone can learn a skill or change their own behavior by teaching. Some specialized papers study learning about teaching during the initial formation period of an educator and later on while teaching, showing that instructors' teaching skills tend to improve through teaching [Grudnoff and Tuck, 2003].[11] However, far fewer studies focus on the hypothesis that teachers can become more knowledgeable on a specific subject while delivering the content to their students. A notable exception comes from Bakhtiar et al. [2021]. They provide formal business training to experienced female micro-entrepreneurs in Ethiopia, who then become mentors of other female-led businesses. While mentees do not experience significant effects on profits, the bundled effect of receiving training and providing mentoring leads to an important increase in profits among mentors.

The results on teachers are also indicative of the quality of the program delivered in Peru. The sizeable treatment impacts on financial literacy that they experience confirm that the trainers were knowledgeable on the content of the lessons imparted in the classroom.

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 about 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. The study combines survey and credit bureau records on nearly 20,000 students and measures the immediate and sustained impacts of school-based financial education on financial behavior. Previous studies on the effectiveness of financial education on youth have relied on experimental variation and self-reported survey records within a short time span. Another strand of the literature has tried to assess the sustainability of financial education's effects over longer-term horizons using

[11] For instance, Barber and Turner [2007] shows that newly-qualified teachers working in primary schools experience an increase in confidence in relation to special educational needs and report feeling more skilled in this area by the end of their first year of teaching. Moreover, Perkins et al. [2015] study the effects that teaching other adults can have on instructors' skills in the context of a beginner program delivering music lessons in the UK. The authors show that the teachers reformulated the ways in which they thought about teaching music to adult learners and developed teaching skills relevant to a wide-range of teaching contexts.

high-stakes data to measure financial behavior, but relying on non-experimental variation in course requirements during high school or college in the US. This is the first study that relies on credible exogenous variation in exposure to financial education and complements survey records with rich administrative data, alleviating self-reporting biases and providing a measure of actual financial behavior several years after the treatment.

This study supports the immediate effectiveness of school-based financial education programs. It also finds limited, but sustained effects on financial behavior when students are aged between 18 and 20, a key period in which young adults are becoming economically independent. The treatment did not affect students' credit or repayment behavior on the extensive margin, but, among those few with outstanding loans, it reduced loan portfolios in arrears by 20%.

Credit and repayment outcomes are especially relevant for this population as early mistakes can be costly and hard to amend. By reducing the average size of the portfolio in arrears, the treatment fosters better performance at the onset of their credit histories. This effect is economically meaningful in size: it is equivalent to a 0.147 SD drop in the balance of delinquent debt, well above the average effect size estimated for credit outcomes (0.042 SD) in [Kaiser et al., 2022]. The effects on repayment in other non-experimental papers focusing on the US case go in the same direction, but it is hard to benchmark the impact on arrears against them. On one hand, the magnitude of the effects may differ due to their focus on a country where youth is much more reliant on debt. On the other hand, the variation exploited raises several endogeneity concerns.

The results presented here support the formal inclusion of financial education content in the school curricula, especially since it does not seem to hinder academic performance. One of the strengths of the pilot implemented in Peru is the high level of compliance with the training and the delivery of the lessons among teachers. However, the inclusion of financial education content in the curricula could further improve teachers' compliance and solve coordination problems between teachers and principals to incorporate the materials. The ITT effects thus constitute a lower bound of the effect that these programs could have if they were to be included as a mandatory course or course portion, subject to regular evaluation.

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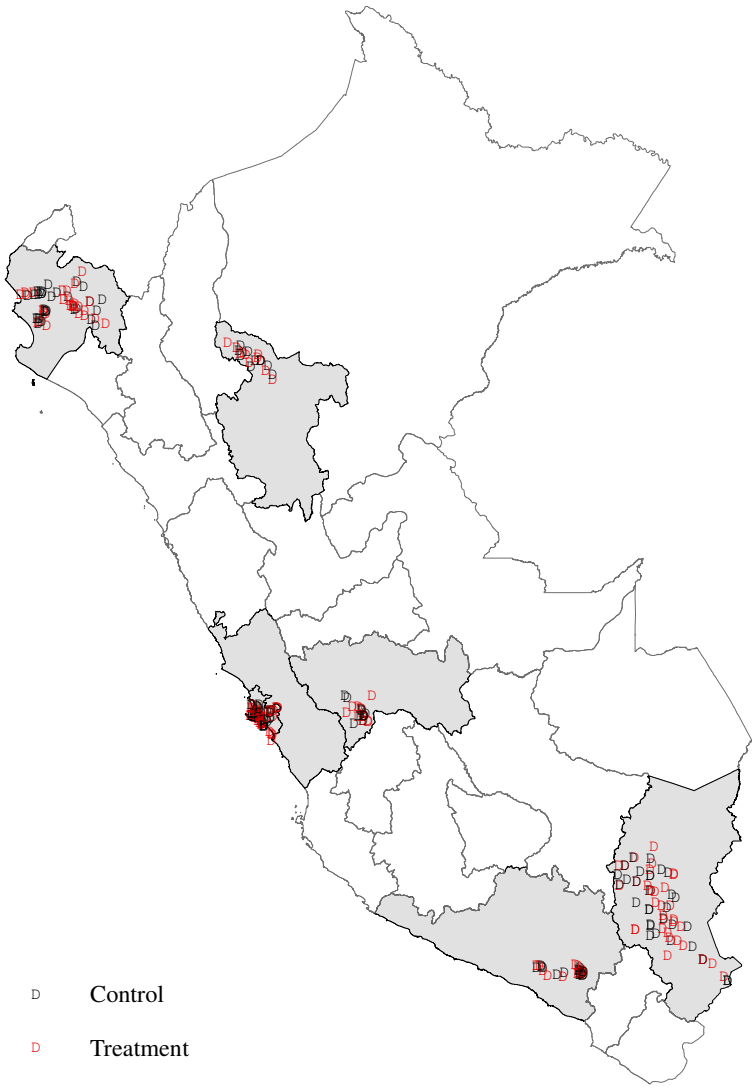
A Appendix

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



NOTE: Intervention regions are highlighted grey.

Table A.1: Financial Literacy Lessons and Intensity of Exposure by Grade

	9th grade	10th grade	11th grade
A. Lessons	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 2. Budgeting 2.1. Financial plan 2.2. Income and expenses 2.3. Budgeting 2.4. Usefulness of budgets	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. Information 2.1. Transparency in financial contracts 2.2. Consumers' responsibilities
B. Exposure	8 sessions 16 weeks 32 hours	5 sessions 12 weeks 24 hours	7 sessions 8 weeks 16 hours

NOTE: Exposure information reflects the suggested guidelines provided by the MINEDU to all HGE teachers in treatment schools during the first year of the pilot, 2016.

Table A.2: Hours of Exposure to Financial Education in School-Based Programs Targeting Children and Youth

Paper	Grade at baseline	Age at baseline	Time (hours)
Bruhn et al. (2016)	**11**	**16**	**72-144**
Frisancho (2021)	**9-11**	**15**	**16-32**
Batty et al. (2020)	4	10	20
Becchetti et al. (2013)	**12**	**17**	**16**
Becchetti and Pisani (2012)	**12**	**17**	**16**
Supanantaroek et al. (2016)	5-6	12	16
Alan and Ertac (2018)	3	9	16
Hinojosa et al. (2009)	**4-10**	**13**	**15**
Bover et al. (2018)	**9**	**15**	**10**
Luhrmann et al. (2018)	**7-8**	**13-15**	**4.5**
Batty et al. (2015)	4-5	9	4

NOTE: Experimental studies targeting high school students are highlighted in **bold**.

Table A.3: Balance check in the Endline Sample: Student characteristics

Variable	Control mean	T-C	N
Male	0.499	0.009	19462
	(0.500)	(0.013)	
Age	15.656	0.015	18666
	[1.222]	[0.022]	
Works	0.402	–0.004	16778
	(0.490)	(0.011)	
Ratio of household members to bedrooms	1.849	–0.000	16567
	(0.996)	(0.018)	
Lives with both parents	0.598	0.007	16756
	(0.490)	(0.010)	
Asset index	–0.025	–0.033	16851
	(0.994)	(0.030)	
High level of parental supervision	0.760	0.013	15983
	(0.427)	(0.007)*	
Has dinner with parents 7 days a week	0.328	–0.004	16896
	(0.469)	(0.008)	
Truancy in the past 2 weeks	0.140	0.001	16431
	(0.347)	(0.006)	
Student engagement (scale) - Baseline	0.026	0.007	15224
	(0.881)	(0.015)	
Impulsiveness	0.012	0.027	14466
	(0.878)	(0.013)**	
Conscientiousness	0.018	–0.006	13108
	(0.884)	(0.015)	
Self-control	0.012	0.002	14037
	(0.879)	(0.015)	
Time inconsistency: hyperbolic	0.125	–0.004	15127
	(0.331)	(0.004)	
Risk averse	0.708	0.006	15867
	(0.455)	(0.006)	
No previous exposure to financial education	0.373	–0.018	15867
	(0.484)	(0.010)*	
Financial literacy raw score (0–15)	8.070	0.091	17038
	(2.919)	(0.082)	
GPA 2015 (0–20)	13.741	–0.043	17706
	(1.471)	(0.041)	
Financial autonomy (0–100)	40.873	0.483	16151
	(12.963)	(0.207)**	
Has a savings account	0.136	0.002	15883
	(0.343)	(0.005)	
Prepares a personal budget	0.564	–0.014	15202
	(0.496)	(0.007)*	
Compares prices before shopping	0.043	0.002	15195
	(0.203)	(0.004)	
Bargains	0.938	–0.000	15195
	(0.242)	(0.005)	
Talks to parents/tutors about family finance	0.711	–0.001	15425
	(0.453)	(0.006)	
Helps family with budgeting	0.683	0.006	15389
	(0.465)	(0.007)	

NOTE: Data from the baseline survey and exam for the sample of students present at the exit survey and exam. Test for joint covariates orthogonality $p - value = 0.5269$. Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors (deviations) of coefficients (control means) are in parentheses.

Table A.4: Balance check in the Baseline Sample: Student characteristics

Variable	Control mean	T-C	N
Male	0.498	0.010	20600
	(0.500)	(0.013)	
Age	15.642	0.014	16504
	[1.212]	[0.022]	
Works	0.401	–0.008	20079
	(0.490)	(0.011)	
Ratio of household members to bedrooms	1.853	0.006	19794
	(0.999)	(0.017)	
Lives with both parents	0.589	0.003	20039
	(0.492)	(0.009)	
Asset index	–0.000	–0.023	20173
	(1.000)	(0.029)	
High level of parental supervision	0.755	0.007	19123
	(0.430)	(0.006)	
Has dinner with parents 7 days a week	0.321	–0.001	20227
	(0.467)	(0.007)	
Truancy in the past 2 weeks	0.156	–0.001	19630
	(0.363)	(0.006)	
Student engagement (scale) - Baseline	–0.001	0.015	18155
	(0.883)	(0.014)	
Impulsiveness	–0.003	0.026	17246
	(0.879)	(0.012)**	
Conscientiousness	–0.002	0.006	15577
	(0.883)	(0.014)	
Self-control	–0.001	–0.000	16725
	(0.887)	(0.014)	
Time inconsistency: hyperbolic	0.127	–0.006	18066
	(0.333)	(0.004)*	
Risk averse	0.706	0.009	18956
	(0.456)	(0.006)*	
No previous exposure to financial education	0.371	–0.018	18956
	(0.483)	(0.009)**	
Financial literacy raw score (0–15)	8.028	0.130	20409
	(2.929)	(0.077)*	
GPA 2015 (0–20)	13.727	–0.034	18216
	(1.483)	(0.042)	
Financial autonomy (0–100)	40.787	0.425	19314
	(12.903)	(0.189)**	
Has a savings account	0.137	0.004	18990
	(0.344)	(0.005)	
Prepares a personal budget	0.565	–0.011	18150
	(0.496)	(0.007)*	
Compares prices before shopping	0.044	–0.003	18156
	(0.205)	(0.004)	
Bargains	0.938	0.005	18156
	(0.242)	(0.005)	
Talks to parents/tutors about family finance	0.709	–0.002	18429
	(0.454)	(0.006)	
Helps family with budgeting	0.679	0.006	18377
	(0.467)	(0.007)	

NOTE: Data from the baseline survey and exam for the sample of students present at baseline. Test for joint covariates orthogonality $p - value = 0.5144$. Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors (deviations) of coefficients (control means) are in parentheses.

Table A.5: Balance check: Teacher characteristics

Variable	Control mean	T-C	N
Male	0.577	–0.086	453
	(0.495)	(0.048)	
Age	46.755	–0.176	432
	(11.028)	(1.077)	
Undefined contract teacher	0.637	0.021	435
	(0.482)	(0.046)	
Workload (hours)	0.797	–0.039	379
	(0.404)	(0.043)	
Years of teaching experience	17.177	0.297	401
	(10.217)	(1.100)	
Degree in Social Sciences	0.632	0.041	393
	(0.484)	(0.050)	
Higher education	0.332	0.051	426
	(0.472)	(0.046)	
Teaches in 9th grade	0.531	0.053	453
	(0.500)	(0.037)	
Teaches in 10th grade	0.526	0.008	453
	(0.501)	(0.036)	
Teaches in 11th grade	0.488	0.016	453
	(0.501)	(0.035)	

NOTE: Data comes from the exit survey and exam. Test for joint covariates orthogonality $p-value = 0.5628$. Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors (deviations) of coefficients (control means) are in parentheses.

Table A.6: Determinants of Attrition between Baseline and Endline Survey

	All	9th	10th	11th
	(1)	(2)	(3)	(4)
10th grade	-0.007			
	(0.016)			
11th grade	-0.019			
	(0.017)			
Male	0.005	-0.012	-0.008	0.026*
	(0.009)	(0.015)	(0.015)	(0.016)
Works	0.013	-0.004	0.033**	0.016
	(0.009)	(0.014)	(0.017)	(0.016)
No previous exposure to financial education	-0.007	-0.003	-0.011	-0.011
	(0.010)	(0.014)	(0.015)	(0.017)
Ratio of household members to bedrooms	0.004	-0.007	0.013	0.004
	(0.004)	(0.008)	(0.008)	(0.008)
Asset index	0.018***	0.005	0.021**	0.026**
	(0.006)	(0.010)	(0.008)	(0.011)
High level of parental supervision	-0.013	-0.015	-0.025	0.005
	(0.010)	(0.017)	(0.016)	(0.018)
Lives with both parents	-0.033***	-0.034**	-0.022	-0.043***
	(0.008)	(0.015)	(0.015)	(0.015)
Has dinner with parents 7 days a week	-0.011	-0.007	-0.013	-0.006
	(0.008)	(0.013)	(0.015)	(0.017)
Baseline financial literacy score	-0.010**	-0.006	-0.014*	-0.004
	(0.005)	(0.008)	(0.008)	(0.008)
Treatment	-0.008	-0.082**	0.039	0.113***
	(0.026)	(0.036)	(0.038)	(0.038)
T×10th grade	0.035			
	(0.024)			
T×11th grade	0.062**			
	(0.028)			
T×Male	0.007	0.031	0.021	-0.027
	(0.013)	(0.023)	(0.021)	(0.021)
T×Works	0.006	0.017	-0.004	0.003
	(0.012)	(0.022)	(0.021)	(0.022)
T×No previous exposure to financial education	0.006	0.005	0.026	-0.013
	(0.013)	(0.020)	(0.021)	(0.022)
T×Ratio of household members to bedrooms	0.001	0.015	-0.001	-0.008
	(0.006)	(0.010)	(0.012)	(0.011)
T×Asset index	-0.016**	0.000	-0.024**	-0.022*
	(0.008)	(0.015)	(0.010)	(0.013)
T×High level of parental supervision	-0.031**	-0.015	-0.025	-0.058**
	(0.014)	(0.024)	(0.024)	(0.022)
T×Lives with both parents	-0.014	0.012	-0.045**	-0.014
	(0.013)	(0.021)	(0.021)	(0.021)
T×Has dinner with parents 7 days a week	0.007	0.014	-0.005	-0.003
	(0.011)	(0.020)	(0.020)	(0.023)
T×Baseline financial literacy score	-0.004	-0.012	-0.001	-0.002
	(0.007)	(0.010)	(0.012)	(0.011)
Number of Observations	16296	5557	5320	5419
Number of schools	296	295	295	296
Mean in Control	0.16	0.17	0.16	0.16
Joint significace of interactions				
F-test	1.63	0.83	1.78	1.42
p-value	0.09	0.59	0.07	0.18

NOTE: Financial literacy exam score is standardized by grade relative to the control group in the original experimental sample of 300 schools. * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs of schools.

Table A.7: Share of Missing Data by Construct and Survey Round

	Financial Literacy (1)	Financial Autonomy (2)	Saves (3)	Keeps a Budget (4)	Saves Before Buying (5)	Compares Prices (6)	Bargains (7)
A. Baseline Survey							
Treatment	0.001	-0.001	0.014	0.002	0.005	-0.001	-0.001
	(0.002)	(0.007)	(0.012)	(0.008)	(0.008)	(0.008)	(0.008)
Number of Observations	20622	20622	20622	20622	20622	20622	20622
Number of schools	296	296	296	296	296	296	296
Mean in Control	0.11	0.16	0.28	0.21	0.19	0.21	0.21
R-squared	0.03	0.08	0.05	0.05	0.05	0.05	0.05
B. Endline Survey							
Treatment	0.000	0.016	-0.007	-0.006	0.007	0.010	0.010
	(.)	(0.013)	(0.014)	(0.012)	(0.012)	(0.012)	(0.012)
Number of Observations	19462	19462	19462	19462	19462	19462	19462
Number of schools	296	296	296	296	296	296	296
Mean in Control	0.15	0.26	0.39	0.31	0.27	0.29	0.29
R-squared	.	0.10	0.11	0.07	0.09	0.09	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 parentheses.

Table A.8: Effects on Students' Academic Outcomes

	GPAs				Grade Progression	University Aspirations
	Cumulative	Math	Verbal	HGE		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	–0.015	–0.007	0.033*	0.001	0.002	–0.002
	(0.014)	(0.019)	(0.018)	(0.020)	(0.009)	(0.005)
Number of Observations	19035	19035	19035	19035	18558	19032
Number of Schools	296	296	296	296	296	296
Mean in Control	–0.008	–0.006	–0.006	–0.005	0.814	0.833

GPAs are measured at the end of the 2016 academic year and standardized by grade relative to the control group in the original experimental sample of 300 schools. Grade progression is a binary variable indicating if the student was promoted to the next grade at the end of the 2016 academic year (graduated in the case of 11th grade students). School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for all GPA outcomes, grade progression, and university aspirations. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs 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. The models for grades also include the value of the dependent variable at the end of the 2015 academic year as a control.

Table A.9: ATT Effect on Students' Financial Literacy and Financial Behavior

	Financial Literacy	Financial Autonomy	Pr(Saving)	Financial Savviness
	(1)	(2)	(3)	(4)
Treatment	0.253***†	0.040**†	0.037***†	0.053***†
	(0.036)	(0.016)	(0.013)	(0.011)
Number of Observations	17685	15227	12734	15629
Number of Schools	291	291	291	291
Mean in Control	–0.005	–0.004	0.593	–0.009

Note: All outcomes are measured at the end of the 2016 academic year. Scores in the financial literacy exams are standardized at the grade level, using the distribution of the control group in the baseline exam as a benchmark. The financial autonomy index aggregates fifteen binary variables capturing whether students felt empowered, confident, and capable of making independent financial choices and influencing their households' financial decisions. The probability of saving captures both formal and informal savings decisions. The financial savviness index aggregates four binary variables measuring if the student keeps a budget, saves before buying something that cannot be afforded, compares prices, and bargains before shopping. The financial autonomy and financial savviness indexes are computed as an equally weighted average of the z-scores of each of its components. These z-scores are obtained by subtracting the control group mean and dividing by the control group standard deviation. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for financial literacy, financial autonomy, probability of saving, and financial savviness. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs 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. The value of the dependent variable at baseline is also included as a control.

Table A.10: ATT Effect on Teachers' Financial Literacy and Financial Behavior

	Financial Literacy (1)	Financial Autonomy (2)	Pr(Save) (3)	Savings Balance (4)	Financial Savviness
Treatment	0.460***††	0.117	0.118***††	700.4**†	0.012
	(0.119)	(0.075)	(0.038)	(297.4)	(0.047)
Number of Observations	417	347	334	334	335
Number of Schools	250	214	214	214	214
Mean in Control	0.025	–0.006	0.839	789.1	0.011

Note: All outcomes are measured at the end of the 2016 academic year. Scores in the financial literacy exams are standardized at the grade level, using the distribution of the control group in the baseline exam as a benchmark. The financial autonomy index aggregates fifteen binary variables capturing whether students felt empowered, confident, and capable of making independent financial choices and influencing their households' financial decisions. The probability of saving captures both formal and informal savings decisions. Savings balances are expressed in US dollars. The financial savviness index aggregates four binary variables measuring if the student keeps a budget, saves before buying something that cannot be afforded, compares prices, and bargains before shopping. The financial autonomy and financial savviness indexes are computed as an equally weighted average of the z-scores of each of its components. These z-scores are obtained by subtracting the control group mean and dividing by the control group standard deviation. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for financial literacy, financial autonomy, probability of saving, and financial savviness. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs 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.11: Determinants of the Probability of Teaching 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)
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 parentheses. 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 Online Appendix

B.1 Additional Figures and Tables

Table B.1: 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%.

B.2 Heterogeneous Treatment Effects

Several studies have documented large differences in financial literacy between women and men and also across socioeconomic status, with marked disadvantages for women and poorer individuals [Lusardi and Mitchell, 2014; Bucher-Koenen et al., 2017]. These differences in baseline levels can potentially mediate the treatment impacts estimated above. On one hand, students who start off behind may benefit more from the provision of financial education as there is more room for them to catch up. On the other hand, students with higher baseline levels of financial literacy may be in better shape to grasp the concepts delivered through the lessons and thus extract greater gains from these programs.

However, heterogeneous treatment impacts by sex or socioeconomic status may emerge due to other reasons besides differential baseline levels of financial literacy. For instance, several studies document differences in the economic decisions, risk preferences, and valuation of money [Eckel and Grossman, 2001, 2008*b*,*a*; Lynn, 1993] of men and women which may emerge from biased gender roles that portray men as the breadwinner. Moreover, Lusardi et al. [2017] show that higher income individuals have more incentives to become more financially literate as they have relatively greater needs to save. Thus, youth from more affluent families are more likely to be exposed to relatively more sophisticated strategies to manage their household finances. They are also more likely to have greater budgets allocated to them, which can make them more eager to learn how to better administer the resources at hand. When exposed to financial education in the school, children from wealthier families could be more interested in learning financial skills and several concepts and definitions may be easier to grasp for them.

This Appendix thus focuses on the analysis of heterogeneous effects by sex and by socioeconomic status, the latter measured by an asset index. To try to disentangle between heterogeneous effects due to baseline financial literacy as opposed to other drivers linked to intrinsic sex or socioeconomic status differences, the analysis also looks at the heterogeneity of the impacts by baseline financial literacy scores.

Tables B.2-B.4 present heterogeneous treatment impacts on financial literacy and financial behavior in the short run. First, notice that financial literacy is not significantly different by sex, though certain differences emerge in terms of behavior with boys being more likely to save, but less financially savvy (see Table B.2). Moreover, the estimated treatment impacts on financial skills and behavior at follow up do not seem to vary across boys and girls in the experimental sample. In turn, Table B.3 shows that the treatment gives youth from more affluent families larger financial literacy gains. These gains are not entirely driven by differential baseline scores, since B.4 finds that the treatment yields similar effects along the distribution of initial performance in the financial literacy exam.

All in all, there are no significant differences in the *immediate* effects of the treatment on financial literacy nor behavior by sex or baseline score. However, students from households with higher asset indexes benefit relatively more from the financial literacy lessons: a one standard deviation increase in the asset index significantly raises the treatment effect on financial literacy

Table B.2: Effect on Students' Financial Literacy and Financial Behavior, by Sex

	Financial Literacy (1)	Financial Autonomy (2)	Pr(Saving) (3)	Financial Savviness (4)
Treatment	0.156*** † † †	0.035** ††	0.013	0.024**
	(0.029)	(0.015)	(0.013)	(0.012)
Treatment X Male	0.001	-0.021	-0.000	0.012
	(0.033)	(0.021)	(0.018)	(0.016)
Male	0.005	0.001	0.034*** ††	-0.032*** ††
	(0.023)	(0.016)	(0.012)	(0.012)
Number of Observations	19462	16675	13917	17116
Number of schools	296	296	296	296
Mean in Control	-0.005	-0.004	0.593	-0.009

Note: All outcomes are measured at the end of the 2016 academic year. Scores in the financial literacy exams are standardized at the grade level, using the distribution of the control group in the baseline exam as a benchmark. The financial autonomy index aggregates fifteen binary variables capturing whether students felt empowered, confident, and capable of making independent financial choices and influencing their households' financial decisions. The probability of saving captures both formal and informal savings decisions. The financial savviness index aggregates four binary variables measuring if the student keeps a budget, saves before buying something that cannot be afforded, compares prices, and bargains before shopping. The financial autonomy and financial savviness indexes are computed as an equally weighted average of the z-scores of each of its components. These z-scores are obtained by subtracting the control group mean and dividing by the control group standard deviation. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for financial literacy, financial autonomy, probability of saving, and financial savviness. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs 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. The value of the dependent variable at baseline is also included as a control.

by 0.05 SD. However, this advantage does not pass onto differential changes in financial behavior in the short run.

Table B.5 moves on to explore if the larger knowledge gains accrued by youth from households with a higher asset index provide them with an advantage in terms of their credit management outcomes in the medium run. A higher asset index is indeed associated with a significant effect on the probability of having debt. While the average treatment effect on the extensive margin is null, a one standard deviation increase in the asset index yields a 0.007 increase in the probability of having outstanding debt, a 14% increase relative to the control. Since this effect is not accompanied by an increase in the probability of having arrears, this is potentially a welfare-improving effect. The average effect on arrears is similar along the distribution of the asset index in the experimental sample.

Table B.3: Effect on Students' Financial Literacy and Financial Behavior, by Socioeconomic Status

	Financial Literacy (1)	Financial Autonomy (2)	Pr(Saving) (3)	Financial Savviness (4)
Treatment	0.158*** † † †	0.024**	0.013	0.030*** † † †
	(0.022)	(0.010)	(0.009)	(0.008)
Treatment X Asset Index	0.055*** ††	-0.002	0.007	0.005
	(0.020)	(0.011)	(0.009)	(0.008)
Asset Index	0.009	0.004	0.015**	0.016** †
	(0.014)	(0.009)	(0.007)	(0.006)
Number of Observations	19462	16675	13917	17116
Number of schools	296	296	296	296
Mean in Control	-0.005	-0.004	0.593	-0.009

Note: All outcomes are measured at the end of the 2016 academic year. Scores in the financial literacy exams are standardized at the grade level, using the distribution of the control group in the baseline exam as a benchmark. The financial autonomy index aggregates fifteen binary variables capturing whether students felt empowered, confident, and capable of making independent financial choices and influencing their households' financial decisions. The probability of saving captures both formal and informal savings decisions. The financial savviness index aggregates four binary variables measuring if the student keeps a budget, saves before buying something that cannot be afforded, compares prices, and bargains before shopping. The financial autonomy and financial savviness indexes are computed as an equally weighted average of the z-scores of each of its components. These z-scores are obtained by subtracting the control group mean and dividing by the control group standard deviation. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for financial literacy, financial autonomy, probability of saving, and financial savviness. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs 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. The value of the dependent variable at baseline is also included as a control.

Table B.4: Effect on Students' Financial Literacy and Financial Behavior, by Baseline Score

	Financial Literacy (1)	Financial Autonomy (2)	Pr(Saving) (3)	Financial Savviness (4)
Treatment	0.156*** † † †	0.023**	0.011	0.028*** † † †
	(0.023)	(0.010)	(0.010)	(0.008)
Treatment X Baseline Score	0.012	0.014	0.012	-0.010
	(0.023)	(0.011)	(0.010)	(0.008)
Baseline Score	0.374*** † † †	0.024*** ††	0.032*** † † †	0.053*** † † †
	(0.014)	(0.008)	(0.007)	(0.007)
Number of Observations	19462	16675	13917	17116
Number of schools	296	296	296	296
Mean in Control	-0.005	-0.004	0.593	-0.009

Note: All outcomes are measured at the end of the 2016 academic year. Scores in the financial literacy exams are standardized at the grade level, using the distribution of the control group in the baseline exam as a benchmark. The financial autonomy index aggregates fifteen binary variables capturing whether students felt empowered, confident, and capable of making independent financial choices and influencing their households' financial decisions. The probability of saving captures both formal and informal savings decisions. The financial savviness index aggregates four binary variables measuring if the student keeps a budget, saves before buying something that cannot be afforded, compares prices, and bargains before shopping. The financial autonomy and financial savviness indexes are computed as an equally weighted average of the z-scores of each of its components. These z-scores are obtained by subtracting the control group mean and dividing by the control group standard deviation. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for financial literacy, financial autonomy, probability of saving, and financial savviness. OLS estimates, standard errors clustered at the school level are reported in parentheses. All specifications include a set of dummy variables that correspond to the matched-pairs 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. The value of the dependent variable at baseline is also included as a control.

Table B.5: Effects on Students' Credit and Delinquency Outcomes, by Socioeconomic Status

	Pr(Debt)	Pr(Arrears)		Log(Current	Log(Debt Arrears)	
		Loans	Other bills	Debt)	Loans	Other bills
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.002	-0.001	0.002	0.169	-0.218*	-0.028
	(0.003)	(0.001)	(0.002)	(0.111)	(0.113)	(0.118)
Treatment X Asset Index	0.007**†	0.001	0.003	0.065	0.060	0.116
	(0.003)	(0.001)	(0.003)	(0.147)	(0.152)	(0.156)
Asset Index	-0.004*	-0.001	-0.003	-0.078	-0.005	0.034
	(0.003)	(0.001)	(0.002)	(0.129)	(0.126)	(0.116)
Number of Observations	19113	19113	19113	902	902	658
Number of schools	296	296	296	249	249	251
Mean in Control	0.049	0.006	0.034	5.203	0.571	3.960

Note: Students' credit and default outcomes measured in June 2019. Current debt and debt in arrears are measured in US dollars and log-transformed. School pairs with incomplete survey records for at least one school are excluded from estimation. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for two families of outcomes: (i) probability of having debt, probability of having loan arrears, and probability of having arrears from other bills and (ii) current debt and debt arrears in loans. OLS estimates, standard errors clustered at the school level are reported in parentheses. 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.

B.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.

In the Peruvian case, the fixed cost of developing the workbooks amounted to US$56,100. As these should not be considered for scaling up efforts, they are excluded from the cost estimates below. Following the "incremental cost" approach, which reflects the additional monetary resources used by the treatment group relative to those assigned to the control group, and the ingredients method proposed by McEwan [2015], marginal implementation costs of the school-based financial education program in 150 schools (31,000 high school students) are estimated at 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 high cost-to-effectiveness ratio of 3.24, measured as additional standard deviations in test scores for a cost of US $100 allocated to the program.

Following Evans and Popova [2016], a sensitivity analysis of the cost-effectiveness of the school-based financial education is performed. Taking into account the 90% confidence intervals of the estimated effect on financial literacy for students yields robust estimates: the lower and upper bounds of the cost-effectiveness ratio are 2.32 SD and 4.16 SD, respectively.

B.4 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}:

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

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. 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}.