Lasting Lessons: The Long-Term Impacts of School-Based Financial Education*

Veronica Frisancho*† Antonella García‡ Edgar Ventura‡ Juan Carlos Chong‡

This version: June 25, 2026

Abstract

How does school-based financial education shape adult financial behavior in the long run? We leverage a randomized controlled trial tracking 60,000 students in Peru over seven years using administrative credit, pension, and tax registries. While the extensive margin of holding debt is unaffected, the program induces a strategic portfolio recomposition: treated individuals shift away from high-interest revolving debt toward structured non-revolving credit (a 14.5% expansion without compromising repayment capacity. Long-term impacts are broadly equitable across gender and socioeconomic status, though higher-performing students experience greater credit access over time. The program also fostered resilience during COVID-19, significantly reducing revolving debt reliance.

Keywords: financial education, youth, financial literacy, credit records, treatment effects, long-lasting impacts

JEL Codes: C93, D14, G53, O16

*We would like to thank Alexander Pacheco and Maria Lujan Puchot for their excellent research assistance. Financial support from the Inter-American Development Bank and CAF, Development Bank of Latin America and the Caribbean, is gratefully acknowledged. This study is registered in the AEA RCT Registry with the unique identifying number AEARCTR-0004719. All data collection activities were conducted once the Chesapeake Institutional Review Board determined that the evaluation activities were exempt from its oversight (protocol number Pro00016325).

†CAF, Development Bank of Latin America and the Caribbean - Avenida Eduardo Madero, N° 900 Edificio Catalinas Plaza, piso 15, C1106ACV Ciudad Autónoma de Buenos Aires, Argentina. E-mail: vfrisancho@caf.com.

‡Superintendencia de Banca y Seguros (SBS) - Los Laureles 214, San Isidro - Lima, Peru.

1 Introduction

School-based financial education is increasingly promoted as a cornerstone for enhancing consumer welfare and macroeconomic financial stability. However, its long-term efficacy remains a critical policy question. Advocates argue that early interventions equip individuals with the foundational knowledge necessary to navigate complex financial markets and build resilience against economic shocks. Yet, the economic return on these programs depends fundamentally on whether immediate gains in financial literacy translate into sustained behavioral changes as participants transition into adulthood. Evaluating this link is essential, as the ultimate objective of youth-targeted financial education is not merely short-term knowledge retention, but a sustained ability to shape sound financial habits and promote systemic resilience during economic crises.

A growing body of literature examines the effectiveness of financial education interventions Lusardi and Kaiser [2025]; Kaiser et al. [2022]; Kaiser and Menkhoff [2019]; Frisancho [2019]. Most studies focus on programs aimed at youth, with impact assessments primarily capturing short-term outcomes such as financial literacy and basic habits [Bruhn et al., 2016; Bover et al., 2024]. Understanding the long-term impacts of youth-targeted programs is crucial because participants become economically active agents several years after exposure. Measuring sustained behavioral changes is also important for evaluating cost-effectiveness. While school-based (mandatory) programs yield high uptake and retention rates and have great impact on immediate learning, it has yet to be proved that this learning is retained over time [Willis, 2011] and leads to better financial choices once youth become adults. Two exceptions along these lines are Frisancho [2022] and Bruhn et al. [2025], who have experimentally measured the impact of school-based financial education programs on credit outcomes three and nine years after the lessons were delivered. Outside the school setting, Horn et al. [2023] provide experimental evidence on the effects of group-based financial education delivered through youth groups, showing impacts on savings and income five years after the intervention.[1]

This paper builds on Frisancho [2022] and contributes to the literature by providing novel evidence on the long-term impacts of a school-based financial education program implemented in Peru. Leveraging administrative credit records spanning seven years and tracking almost 60,000 individuals, we analyze the effects of a school-based financial education program on a

[1] Hvidberg [2023] is also worth mentioning. He relies on quasi-experimental evidence to measure the impact of majoring in business or economics in college on the risk of default and delinquency more than 10 years after the year of application to the degree program.

range of credit-related outcomes, including probability of holding debt, debt balances, loan size, and credit terms. Complementary administrative records from the Peruvian tax administration and private pension system enable us to evaluate potential effects of the treatment on formal labor market indicators seven years after exposure to the treatment. Unlike previous studies that have relied on self-reported data or short-term assessments, our analysis captures actual financial behavior over an extended period, mitigating concerns about social desirability bias and overcoming the limitations of immediate post-intervention evaluations McKenzie [2012]; Stoddard and Urban [2020]. Moreover, the coverage of the data enables us to look at the dynamic effects of the program, placing particular emphasis on the COVID-19 pandemic, a period during which the financial system faced a broad economic crisis.

This study uses data from a large-scale randomized controlled trial conducted in 300 public high-schools across six regions in Peru, encompassing grades 9 through 11.[2] The intervention was randomized at the school level and involved delivering financial education lessons during school hours from August to December 2016. The curriculum varied by grade: ninth graders learned about needs versus resources and budgeting; tenth graders studied financial products, services, and future planning; and eleventh graders explored responsible financial behavior and market information access. Teachers, trained in the curriculum, delivered the lessons.

Frisancho [2022] found that the school-based financial education program produced notable short-term knowledge gains: scores on the exit exam were 0.16 SD higher than the scores of the control group. The program also led to modest immediate improvements in financial autonomy and savvy, such as budgeting and healthy shopping habits. Three years later, credit bureau data indicated that early literacy improvements had led to lasting behavioral changes: among students with loans, those who had received the lessons had reduced delinquent debt by 20%, reflecting sustained benefits in financial management. Frisancho [2023] examined the spillover effects of the intervention on parents and found limited average impacts, but significant improvements in certain subgroups. The intervention improved financial outcomes for parents in poorer households, notably reducing default risk and arrears, and increasing credit scores and debt levels. Parents of daughters experienced stronger effects than parents of sons, with significant improvements in credit scores and contraction of arrears.

While Frisancho [2022] advanced the literature on the long-term effects of financial education, the study only tracked students until they were 18 to 20 years old, when their economic lives were still in the early stages. This study addresses this gap by following students in the same sample for seven years – until they were between 22 and 24 years of age. Our findings

[2]The local grade equivalent corresponds to grades 3 to 5 at the secondary level.

reveal that the intervention did not affect the broad extensive margin of holding debt, but it fundamentally shaped the underlying structural composition of individuals' debt portfolios. Crucially, we document a robust, 14.5% expansion in non-revolving consumption credit that survives conservative multiple hypothesis testing adjustments. While unadjusted metrics also suggest an expansion in overall borrowing scale—including a 7.2% increase in total debt and a 7.8% increase in average loan size—these aggregate volume shifts sit right on the margin of statistical precision and are sensitive to multiple testing corrections. The core, robust impact of the program in the long run is a strategic portfolio recomposition toward structured installment consumption loans, moving away from high-interest revolving consumption debt. Importantly, these effects are not at the expense of repayment capacity, as we find no significant changes in the probability of holding overdue or written-off debt. Unadjusted estimates also point to slightly more favorable borrowing terms. In fact, when looking at the implied changes in the financial cost of debt, we find suggestive evidence supporting a reduction in the monthly costs of revolving debt.

We evaluate whether these long-term financial shifts translate into real-sector labor market outcomes. Our findings indicate that financial education does not lead to significant changes in formal employment, business formation, or formal labor earnings. Rather than indicating a lack of economic productivity, this result may reflect a measurement gap inherent to highly informal economies: because formal administrative records fail to capture informal earnings, the true underlying entrepreneurial activity is instead captured by the robust accumulation of non-revolving credit, frequently used by micro-entrepreneurs to capitalize their ventures.

Few experimental research studies have delved into the heterogeneity of treatment effects in financial education programs. While leveraging administrative data offers the benefit of accurately tracking actual financial behaviors, it also constrains the range of observable individual characteristics. In this context, we concentrate on three key dimensions of potential variation: gender, baseline academic achievement, and socioeconomic status (SES). Our findings partially replicate the inclusive effects documented by Frisancho [2019], with long-term impacts that are broadly equitable across sex and SES. However, we observe that higher performing students gain more in terms of credit access and use over time – a divergence from the short-term results, which showed no differential impacts on financial literacy. These outcomes suggest that the program did not exacerbate initial literacy inequalities, but better educated students adapted more effectively, leading to distinct downstream financial behaviors. Given the variation of the program's curriculum by grade, exploring heterogeneous treatment impacts across grade levels may provide guidance on curriculum design. While extensive-margin borrowing and interest rate margins remain uniform across cohorts, suggestive grade-specific variations appear in credit composition. Specifically, the reduction in revolving debt balances is more pronounced and significant among students exposed to the 10th-grade modules. This pattern aligns closely with the curriculum's structure: while the 9th-grade curriculum focuses on general budgeting, 10th-grade material provided targeted instruction on financial products and responsible consumption.

We also explore the dynamic effects of financial education on credit behavior between 2017 and 2023, focusing particularly on the unprecedented challenges presented by the COVID-19 pandemic. By focusing on these critical years, we assess how financial education equipped individuals to navigate financial hardships while managing their credit effectively. The program led to a contraction of the probability of holding revolving debt between 2020 and 2021. This reduction in revolving debt reliance is mirrored by intensive margin results indicating a drop in revolving balances over the same timeframe. These findings are consistent with Lee et al. [2024], who exploit variation in US state-mandated high school financial education and find that individuals required to take such courses reduced their credit card balances more than their non-mandated peers during COVID-19. At the same time, treated individuals strategically opted for productive loans supporting micro and small enterprises -particularly in 2021-, increasing their reliance on more sustainable forms of credit with expected returns. Repayment outcomes during the pandemic were not significantly affected by the school-based financial education program.

This study contributes to the growing body of literature on financial education by providing comprehensive evidence on the long-term impacts of school-based programs. Beyond our paper, there is only one other experimental study that has measured the long-term impacts of school-based financial education, Bruhn et al. [2025]. They use administrative data on 16,000 students in Brazil to measure the impact of financial education lessons targeting secondary students nine years after exposure to the program. The authors find that treated students are less likely to borrow from expensive sources or to make delayed loan repayments than control students. They also find that the treatment caused students to shift from formal jobs to ownership of formal businesses. Our paper relies on similar administrative records to measure impacts on credit behavior and formality, but extends their work in at least four ways. First, we focus on the universe of students in the experimental sample of schools, almost 60,000 secondary students. Second, we include novel outcomes such as loan size and interest rates. Focusing on borrowing conditions is essential to understanding individuals' ability to obtain better loan terms in the financial market. Third, we present estimates of heterogeneous treatment effects. Evaluating the distributional effects of financial education is crucial for tailoring interventions, understanding whether impacts are driven by specific groups, tracking inequality trajectories, and assessing their applicability to different populations—yet this analysis is often missing in previous studies. Fourth, our work investigates the sustained impacts of financial education on credit behavior at a time when financial stress was heightened due to the COVID-19 pandemic. The Brazilian study stopped following students in February 2020 and therefore their findings do not capture potential effects on resilience and financial distress during the 2020-2021 period.

The remainder of this paper is structured as follows: Section 2 provides background information on the financial education program and describes our data sources. Section 3 outlines our empirical strategy. Section 4 presents our main results, focusing on the cumulative and dynamic treatment impacts on credit behavior. Section 5 concludes.

2 Literature Review and Expected Outcomes

In the simplest version of the life cycle saving model, individuals will optimally consume only a portion of their lifetime incomes each period, borrowing in some periods and saving in others. Imperfect financial knowledge introduces frictions and leads households to incur substantial welfare losses due to their portfolio choices [Calvet et al., 2009; Spataro and Corsini, 2017]. The evidence in fact shows that lower financial literacy is frequently associated with suboptimal saving and portfolio decisions. For instance, individuals with less financial knowledge are less likely to plan for retirement [Lusardi and Mitchell, 2008, 2011; van Rooij et al., 2012], accumulate wealth [Lusardi and Mitchell, 2014], participate in the stock market [van Rooij et al., 2011], or select lower-fee mutual funds [Choi et al., 2009].

Lower financial literacy is also linked to suboptimal decisions during key borrowing phases of the life cycle, typically corresponding to early career establishment or periods of high capital demands, such as financing housing or education. Previous evidence suggests that less financially literate respondents are more likely to hold costly mortgages [Lusardi and Tufano, 2015; Guiso et al., 2022], use high-cost alternative financial services like payday loans, title loans, or revolving credit [Lusardi and de Bassa Scheresberg, 2013; Lusardi and Tufano, 2015; Agarwal et al., 2009; Disney and Gathergood, 2013], and suffer from mortgage stress or default on loans [Hu et al., 2024; Brown et al., 2016; Mustonen et al., 2025].

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 better financial choices. 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 can reduce welfare losses due to suboptimal financial choices. 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]. Delivering financial education through the school system effectively addresses two key barriers: (out-of-pocket) pecuniary costs and opportunity costs. This delivery channel leverages students' attendance during regular school hours and takes advantage of the early life cycle stage, when students have fewer competing uses of their time, maximizing the efficiency of the intervention.

Beyond the immediate acquisition of technical knowledge, the long-term impact of school-based financial education hinges on the development of navigational competence. While curricula understandably focus on specific concepts like compound interest or inflation, the primary benefit of early intervention is the cultivation of robust information-seeking tools. This navigational resilience transforms financial education from passive absorption of content to an enduring behavioral asset. Insofar as financial education teaches individuals to identify and navigate financial data in real-world contexts, it can ensure that the benefits of literacy persist, allowing for the continuous optimization of financial choices over the life cycle.

We thus expect the provision of school-based financial education to be able to set the basis to improve young adults' financial behavior. In particular, we anticipate that the impact on financial knowledge recorded in Frisancho [2022] will translate into more optimal borrowing decisions during a life cycle stage in which individuals tend to be net borrowers. Financial education can fundamentally alter an individual's relationship with credit, transforming it from a source of financial avoidance or distress into a strategically productive tool for consumption smoothing and investment. Increased financial literacy equips individuals with the knowledge to distinguish between high-cost, consumption-driven debt and advantageous loans used for investment or long-term value creation [Lusardi and Tufano, 2015]. Consequently, we expect the treatment to potentially reduce aversion to formal credit [Cole et al., 2011], leading to measurable increases in the total debt held and the average loan size over time, as individuals become more willing to borrow strategically. The impacts may extend to credit quality variables: improved literacy can allow for better selection and risk management by mitigating cognitive errors like exponential growth bias [Levy and Tasoff, 2016], resulting in shifts from high-cost revolving credit toward lower-interest non-revolving credit, marked improvements in repayment performance, and access to improved borrowing terms [Agarwal et al., 2009; Gathergood, 2012].

Smarter access and use of credit can directly translate into superior economic and productive outcomes across the life cycle. While some studies question the extent to which borrowing constraints condition entrepreneurship across the wealth distribution [Hurst and Lusardi, 2004], a robust body of empirical evidence demonstrates that credit availability heavily dictates human capital investments [Solis, 2017] and business creation by allowing individuals to overcome binding liquidity constraints [Fairlie and Krashinsky, 2012]. We may therefore observe impacts on key employment and business variables.

Financial literacy can also equip households with greater resilience during adverse economic shocks. During periods of sudden income loss or market turbulence, financially literate individuals are better equipped to navigate complex decisions regarding forbearance, government relief programs, or emergency liquidity. For instance, Lee et al. [2025] find that financial knowledge weakens the negative impact of COVID-19-related labor market shocks on retirement savings withdrawals in the US. Similarly, Clark et al. [2021] show that the more financially literate were better able to handle financial difficulties brought up by the pandemic, suggesting that knowledge provided additional protection. It is therefore critical to test whether financial literacy functions as an economic buffer that protects long-term stability by guiding individuals to utilize credit lines prudently, access cheaper liquidity, and avoid costly defaults.

3 Context and Data

3.1 The Peruvian Credit Market and Consumer Vulnerability

The Peruvian financial system is characterized by high concentration in the banking sector. The four largest banks collectively held an approximate market share of 83.95% in terms of total assets at the close of 2024. The single largest bank in the system held an individual market share of 33.8%, influencing pricing and product offerings across the board.[3]

The allocation of credit, which totaled over 365 billion of nuevos soles as of September 2025, is primarily driven by corporate and large enterprise loans. The consumer credit segment—comprising both revolving debt (like credit cards) and non-revolving debt (such as personal and vehicle loans)—is nonetheless a dynamic and critical area for household finance, especially given its role in granting initial access to formal credit for younger and

[3] See financial system statistics on market shares by December 2024 here.

lower-income individuals. As of September 2025, the proportion of credit associated with credit cards represents around 5% of total credit.[4] Crucially, it represents almost a quarter of the consumption credit portfolio, which was around 77 billion of nuevos soles in September 2025.

Revolving debt portfolio represents a disproportionate source of risk for households due to its automatic nature, high cost, and the specific vulnerability it creates for households coping with income fluctuations. Recourse to high-cost revolving debt in Peru is fundamentally driven by a combination of both income fluctuations and limitations in financial capability and access to superior forms of credit. Given the high rate of informal employment in Peru, many individuals face unstable monthly incomes and rely on revolving credit as an easily accessible, immediate liquidity buffer to smooth consumption during income shocks. This excessive use was particularly visible during the COVID-19 pandemic [BCRP, 2022]. Moreover, credit cards are frequently the first formal debt instrument extended to young consumers due to low entry barriers. Its "ready-to-use" nature and lack of an evaluation for each transaction make it a default option, often chosen without full awareness of its high interest rates.

According to data from the Superintendency of Banking, Insurance, and Private Pension Fund Administrators (SBS, by its Spanish acronym) the average effective annual interest rate (EAR) for credit cards in Peru is exceptionally high, typically exceeding 65%.[5] As of November 2025, the average rate was approximately 65.97% in local currency, with some store-affiliated banks charging rates over 90%. These rates far surpass those for non-revolving personal loans, such as loans for automobiles and for free availability exceeding 360 days, which can range from 10% to 55% EAR. Beyond the high costs, credit card debt generates a higher risk of default than other credits. Credit card delinquency indicators are highly sensitive to the deterioration of economic conditions, particularly among the lowest credit line segment, where young and lower-income consumers primarily begin their credit history. By September 2025, credit cards and other revolving loans recorded delinquency rates of 6.29% and 4.56%, respectively, exceeding delinquency rates of 3.09% among non-revolving consumption loans.

Data from the RCC reveals that young adults aged 25–35 constitute one of the most significant segments in the Peruvian financial system, with over 2.58 million active debtors by April 2026. Debtors in this cohort hold an average outstanding debt balance of 18,575 nuevos soles ($5,442 USD). A critical component of this financial profile is revolving consumer credit

[4] See here, last accessed November 17th, 2025.
[5] Data obtained from this link, last accessed November 17th, 2025.

(mainly credit cards), which accounts for 10.35% of their total debt portfolio, with more than 1.02 million young consumers holding an average credit card balance of 4,831 nuevos soles ($1,415 USD). In terms of financial vulnerability, the regulatory data indicates a notable delinquency footprint: 10% of the borrowers in this age bracket (258,695 individuals) are currently in arrears, generating an overdue portfolio of more than 1,637 million nuevos soles ($479.8 million USD) and a portfolio delinquency ratio of 3.41% for total debt, which climbs to 3.79% exclusively within revolving consumer lines.

3.2 The Experiment

In 2007, the SBS, within the framework of an inter-institutional cooperation agreement with the Ministry of Education (MINEDU), designed the teacher training program *Finanzas en el Cole* (FEC). The program aimed to strengthen teachers' financial competencies by providing them with knowledge and tools to help students develop the skills necessary to make responsible and informed financial decisions. In 2016, MINEDU, SBS, and the Peruvian Association of Banks launched the pilot program *Finanzas en mi Colegio*, inspired by the FEC, to deliver financial education to high-school students.

The intervention was implemented as part of a bundled package that included student and teacher materials as well as a teacher training component. The materials consisted of student workbooks for each of the last three high-school grades as well as a teachers' guide.[6] The workbooks and teachers' guide supported teachers in the delivery of the lessons, using a mix of case analysis, exercises, group activities, and homework. In addition, teachers were offered a 20-hour training plan divided into five sessions, which included a training component on the financial literacy content (four sessions) and one on pedagogy (one session).

Financial education lessons were delivered during the regular classes of a course, "History, Geography, and Economics".[7] Teachers had the autonomy to decide how to incorporate the material under two basic guidelines. First, they were to include the material in the economics portion of the course. Second, they were provided with rough estimates of the duration of the sessions covered in each workbook.[8] While the content of the lessons was not officially

[6]The content of the workbooks varied by grade. The lessons provided to ninth graders focused on the differences between needs and resources, and on budgeting. The lessons imparted to tenth graders focused on financial products and services and forward-looking choices. The curriculum for eleventh graders covered topics on how to be a responsible financial consumer, as well as access to and use of personal information in financial markets.

[7]By 2016, MINEDU had established the competency "Responsible Management of Economic Resources" within the National Curriculum for Regular Basic Education.

[8]The suggested number of hours required to cover all the lessons in the workbooks varied by grade,

incorporated as a stand-alone course, the inclusion within an existing course reinforced the mandatory component of the program.

The pilot was launched in six regions of the country: Lima and Callao, Arequipa, Piura, Junin, Puno, and San Martin. Due to logistical and implementation constraints, the sampling frame was limited to urban, full-day public schools that were close to cities, which yielded a restricted universe of 308 eligible schools.[9] The sample of eligible schools was stratified by region, and schools were paired by their similarity within each of the six strata.[10] 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 treatment was fully implemented in all grades and regions in 2016. In 2017, the workbooks were still printed and distributed to the treatment schools, but the partners did not provide specific instructions to continue with the delivery of the lessons, nor did they continue to offer teacher training sessions. After 2017, smaller pilots continued in specific regions (e.g., Piura), always respecting the original treatment assignment. That is, no school in the control group received program materials.

The first three columns in Table A.1 presents basic descriptive statistics of the experimental sample, as well as balancing tests of the randomization. The average age in our sample is almost 23 years, with a standard deviation of 1.043. No significant differences are detected across treatment and control groups, which is consistent with the random treatment assignment.

3.3 Data Sources

The pilot program included 60,466 students enrolled in grades 9, 10, and 11 across 300 schools during the 2016 academic year. Beyond the original randomization records, all data sources

ranging from 16 (grade 9) to 24 (grade 8) to 32 (grade 7). 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].

[9] Our power calculations yielded a target sample of 300 schools, 150 in each treatment arm. We set 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.

[10] The Mahalanobis distance was minimized for 10 selected characteristics: electricity connection; availability of water and drainage services; presence of a principal; number of desks in good condition; number of teachers; number of students in grades 9, 10, and 11; dropout rate; passing rate; and whether the school belonged to the experimental sample of any other ongoing pilot.

from this study constitute administrative records. This yields two advantages. First, these records do not suffer from misreporting biases, which tend to influence survey responses. Second, we are able to follow the full universe of students in the pilot.

(a) School Academic Records. MINEDU's academic records provide data for all high-school students enrolled in any of the 300 schools in the experimental sample. From these data, we gather students' national identification numbers (IDs), which enable us to match students across all administrative records we rely on. We also obtain their grade point averages at the end of 2015, the academic year prior to the implementation of the pilot.

(b) Public Credit Bureau Records. Credit outcomes between 2016 and 2023 were obtained from the Peruvian Public Credit Bureau (RCC, by its Spanish acronym), managed by the SBS. This database compiles credit data from all formal and regulated lenders in the Peruvian credit market, including private and public banks, microfinance institutions (such as rural and municipal savings and loan institutions), and financial companies or *Financieras*.[11] These records are very similar to those obtained by Urban et al. [2020], who rely on credit report data from the Federal Reserve Bank of New York/Equifax Consumer Credit Panel to track young individuals. Bruhn et al. [2025] also uses similar data from the Registry of Clients of the Financial System, maintained by the Central Bank of Brazil.

The RCC enables us to track individuals' credit standing over time, with monthly data available from the start of the intervention in 2016. The database is structured at the individual financial account level, providing detailed information on debt balances, classified by loan type (e.g., micro and small enterprises (MSEs) versus revolving and non-revolving consumption loans) and loan status (e.g., current, past-due, and refinanced debt). In sum, these records capture the *stock* of all outstanding credit balances as of any given date.

(c) Credit Portfolio Operation Records. The SBS also collects data from all regulated financial institutions through the Detail by Operation of the Credit Portfolio (DOC, by its acronym in Spanish). However, as data collection began in 2022, these records are only available for a few years. This database includes variables related to credit disbursements, such as the approved loan amount and interest rate granted. In the DOC, the unit of analysis is the credit operation, with monthly records available. The DOC only records *flows*, specifically new credit operations originated within a specific window.

We also obtained access to additional data sources useful for measuring the treatment impacts on formal labor market outcomes:

[11] All financial institutions that are authorized to hold consumer deposits are regulated by the SBS and report their credit records to the public credit bureau.

(d) Private Pension System Records. These records contain information on all formal workers contributing to the private pension system. In addition to an indicator of formality based on individual contributions to a pension fund, we also obtain a measure of formal income from these data. Specifically, we retrieve average monthly earnings for those contributing to an individual pension account between 2017 and 2023.

(e) Peru's Tax Administration (SUNAT) Data. SUNAT's records include active taxpayers as of the date of extraction. Historical records of previously active taxpayers are not included in these data. We rely on SUNAT's classification of taxpayers to determine which individuals are registered as formal business owners.

3.4 Linking Administrative Records

We obtained students' national IDs from MINEDU's administrative records and provided them to the SBS. The regulator proceeded to match these IDs with their records in RCC and DOC. The SBS also linked selected data from the private pension system and SUNAT records.

National IDs are nearly universally merged with the credit bureau database. However, the merge still yields a 5% attrition rate that stems from occasional recording errors in the MINEDU administrative records, leading to "false" matches. Specifically, among the initial 60,466 students, we identified 3,031 cases where the student's SBS-reported age is inconsistent with the school-age cohort. After dropping these observations, we achieved a 95% matching rate between 2017 and 2023, yielding a final analytical sample of 57,435 students, with 49.9% assigned to the control group and 50.1% to the treatment group.[12] Table A.2 in the appendix confirms that attrition in our sample is not differential by treatment status.

As of December 2023, 27% of students in the control group held outstanding credit with a formal financial institution. Among those with debt, 51% had an MSE loan, with an average debt balance between 2017 and 2023 of 3,593 nuevos soles ($ 962 USD); 23% held a revolving consumption loan, with an average balance of 2,037 nuevos soles ($ 546 USD); and 40% had a non-revolving consumption loan, with an average balance of 4,495 nuevos soles ($ 1,204

[12] SBS conducted the merge under very strict protocols to ensure confidentiality of sensitive information. To conduct the analysis, the research team provided the codes and guidelines to conduct the estimations and the results were generated in the SBS offices, using SBS computers with restricted access. Authors external to SBS received the results in the form of tables or figures and never had access to the merged individual records.

USD).[13]

4 Empirical Analysis

4.1 Outcome Variables

Relying on administrative records of students' credit behavior, we define four families of outcomes to measure the impact of financial education on borrowing conditions. First, we measure impacts on the extensive margin of credit. That is, we define the probability of having an outstanding loan, either current or past due, during 2023. We also construct three other probabilities depending on the type of outstanding loan: productive (i.e., MSE), revolving consumption credit, or non-revolving consumption credit. MSE refers to loans given to natural or legal persons for production, trade, or service activities. Revolving credit lines include loans—most notably credit cards—where the outstanding balance varies continuously based on the borrower's choices. By contrast, non-revolving credit includes loans repaid in fixed installments, permanently reducing the debt without the possibility of reuse. Some typical examples of such consumption loans are mortgages, auto loans, and student loans.

Second, to capture the effects of the treatment on the intensive margin, four additional outcomes are measured: current and past due debt, as well as due debt by type of loan (i.e., MSE, revolving, and non-revolving). These four dependent variables are continuous measures of debt balances in December 2023, expressed in Peruvian soles. We also rely on DOC records to measure the impact on average loan size in 2023, both in aggregate terms as well as by type of loan. To estimate the impact on debt balances and loan size, we fit Poisson regressions.[14] Examining the impact on both debt balance and loan size

[13] All credit reported was converted to soles, the local currency. Descriptive statistics in dollars were obtained using the December 2023 exchange rate.

[14] A Poisson regression is employed for analyzing continuous, non-negative, and highly skewed variables, such as income, debt balances, and loan sizes. We utilize the Poisson estimator as a Quasi-Maximum Likelihood Estimator (QMLE), following the approach validated in applied microeconometrics for corner-solution outcomes [Wooldridge, 2010]. This approach is justified not by the assumption that the outcomes are discrete counts, but by the following properties. First, robustness and consistency. The Poisson QMLE consistently estimates the conditional mean function $\mathbb{E}(y|x) = \exp(x'\beta)$, even when the conditional distribution is not Poisson (i.e., for non-count, heteroskedastic data). Second, the handling of zeroes. It naturally accommodates the large proportion of zero values (individuals with no debt or loan) without resorting to two-part models or ad-hoc adjustments like adding a constant for logarithmic transformation. Third, interpretability. The log-link function provides coefficients that are easily converted into Incidence Rate Ratios (IRR). This is particularly advantageous as it allows us to directly interpret the marginal effects in terms of percentage

provides complementary insights into participants' financial behavior. Debt balance reflects the overall financial obligations individuals manage, offering a comprehensive picture of their financial health, while loan size indicates the extent to which individuals engage with credit markets and their willingness to take on larger financial commitments.

Third, we define a measure of the terms of loans by focusing on median annual percentage rates (APR) attached to loans obtained in 2023. For each individual, we define a median APR based on all the loans she obtained during 2023. Once more, we study the impacts on APR for any type of loan as well as those paid for production and consumption loans.

Fourth, we construct outcome variables that try to capture individuals' ability to repay in 2023. Two dependent variables are defined as binary indicators that take a value of 1 if the overdue or written-off amount exceeds zero in any month between January and December 2023. We also construct the balances of overdue and written-off debt as of December 2023.

Finally, we measure formal labor market outcomes. From the tax administrator records, we are able to define a dummy variable that is equal to 1 if the individual is registered as the owner of a formal business. Using the private pension system records, we also define a second binary indicator that is equal to 1 if the individual has ever made a contribution to an individual pension fund. We also measure the aggregate effect on formality by combining these two binary outcomes into one unique probability of participating in the formal sector. Finally, we measure average monthly formal earnings in 2023, as reported by the pension administrator. The regression for formal income is also fitted using a Poisson model.

4.2 Estimation Strategy

The impact of the financial education program on different outcomes is measured as the difference across treatment arms, captured by an intention-to-treat (ITT), ordinary least squares (OLS) regression:

$$y_{ijp} = \alpha + \beta T_{jp} + \delta X_{ijp} + \sum_{p} \theta_p d_{jp} + \epsilon_{ijp} \tag{1}$$

changes in the expected debt volume, which offers a more intuitive understanding than OLS coefficients based on transformed data (e.g., inverse hyperbolic sine or logarithmic transformations), which can obscure meaningful interpretation. To address potential concerns regarding the functional form of our continuous outcomes, Table A.5 compares our baseline Poisson estimates with OLS specifications in levels, winsorized at the top 1% and 2% of the distribution, generally confirming the results from our preferred specification.

where y_{ijp} are credit outcomes of student i in school j from pair p. The impact of the treatment is measured by β, the coefficient on the indicator of treatment status, T_{jp}, which is equal to 1 if 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. The matrix X_{ijp} includes gender, age, GPA, and SES. The Romano-Wolf correction is implemented for families of outcomes to deal with potential issues of simultaneous inference [Romano and Wolf, 2005].

Since potential variation in the years of exposure to the program arises after 2016, ITT effects are more suitable to measure the impact on credit outcomes. This approach is feasible, as the treatment assignment at the school level was respected throughout the analysis period (between 2016 and 2023). 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.

5 Results

Financial education is anticipated to enhance financial literacy by lowering the costs associated with gathering and processing information when making financial decisions, which can ultimately lead to tangible changes in financial behavior. While previous studies have indicated that school-based financial education may yield modest improvements in behavior in the short run [Kaiser and Menkhoff, 2019], there is very limited evidence on the sustained effect that early personal finance lessons may have on adult financial behavior. Moreover, much of the existing evidence on behavioral outcomes is limited due to two main issues. First, behavioral outcomes tend to be measured early in the students' life cycle as economic agents, immediately following the conclusion of an intervention. Second, most studies rely on survey-based outcomes that are susceptible to biases, particularly social desirability bias within the treatment group.

Our data enable us to observe actual credit behavior seven years after the program was delivered. Such data reflect the credit and repayment choices made by the students between 2017 and 2023, providing a more accurate measure of their financial behavior over time.

With seven years of monthly credit records at hand, we are able to capture both cumulative treatment impacts at the latest available data point in 2023, as well as the evolution of these impacts over time. This extensive dataset allows for a comprehensive analysis of how financial education influences credit behavior not just immediately, but across several years.

By examining the cumulative effects, we can assess the total impact of the treatment on credit behavior as of the most recent measurement, providing insights into the lasting benefits of financial education. Furthermore, by analyzing the yearly impacts, we can track trends and changes in credit behavior over time, revealing patterns that may indicate how the benefits of the program develop and stabilize as participants mature into their roles as economic agents. In particular, our period of analysis covers the COVID-19 pandemic, which provides an opportunity for us to observe how individuals adapt their credit behavior during a crisis.

5.1 Cumulative Treatment Impacts

Panel A in Table 1 illustrates the estimated treatment impacts on the likelihood of holding outstanding debt. Column (1) shows that the intervention does not affect the likelihood of holding debt in the formal financial system. This result is homogeneous across types of loan, as shown in columns (2)-(4). It also holds across institution types, as presented in Panel A, in Table A.4 in the Appendix.

As there is no sample selection into borrowing due to the treatment, Panel B in Table 1 presents the treatment impacts on total debt balances. The financial education lessons primarily shape the structural composition of individual debt portfolios seven years post-intervention. While unadjusted metrics indicate a marginal 7.6% increase in total debt balances (column 1), this aggregate volume shift sits on the margin of statistical precision. Instead, the core impact of the program is a pronounced portfolio recomposition within consumption debt: treated individuals strategically substitute away from high-interest revolving loans (column 3, a reduction of 12.2%) toward structured, non-revolving credit lines (column 4, a robust 14.5% expansion). This portfolio rebalancing is mirrored in Panel C, which examines average loan sizes, though these results are sensitive to multiple testing corrections. Still, Panel C also suggests a shift toward structured installment credit: the average size of non-revolving loans expands by 7.5% (column 4).

This targeted shift from high-cost, discretionary revolving debt to structured, predictable non-revolving credit underscores a fundamental behavioral adjustment. Rather than increasing financial vulnerability through higher absolute leverage, treated individuals optimize their credit mix. To contextualize the scale of this portfolio reallocation, the 14.5% expansion in structured non-revolving balances represents approximately 23% of the average monthly formal income in our sample (see Table 3). For a young adult in a highly informal economy, this structural pivot toward formal installment credit reflects a first-order improvement in credit market navigation and risk management, rather than a speculative expansion of debt. This transition not only helps individuals to manage their finances more effectively, reducing their risk of accumulating unmanageable debt, but also promotes more sustainable financial behavior that aligns with long-term goals of asset accumulation and economic stability. These results align with Bruhn et al. [2025], who measured the impact of a school-based financial education pilot in Brazil nine years after the intervention. Using similar administrative records, they also found that treated students tend to avoid the most expensive sources of credit, such as credit card debt and overdrafts.

We primarily suspect that uninformed choices drive an economically inefficient and excessive reliance on revolving debt, which the financial education successfully corrects. The core evidence of the positive effect of the intervention is that subjects did not decrease their overall borrowing (if anything, they increased it slightly), while simultaneously changing its composition toward non-revolving credit. This suggests that financial education did not merely teach people to "fear debt", but instead provided the financial literacy to recognize the high cost of revolving debt and make a more informed, optimal decision to select productive, lower-cost, non-revolving financing alternatives. The treatment, i.e., school-based financial education, empowered consumers to bypass high-cost revolving debt in favor of cheaper non-revolving loans. This shift suggests that, for the control group, reliance on revolving debt was in part an uninformed choice or a symptom of the inability to effectively navigate the credit market and access superior debt products. The reduction of revolving debt is therefore a highly relevant welfare-enhancing outcome.

These improvements in terms of credit portfolio can potentially lead to better financial terms when borrowing. Crucially, since the financial education did not affect the likelihood of holding debt, the subsequent observed changes in borrowing terms are attributable to this improved debt composition, rather than the selection of new borrowers with different underlying characteristics. On the one hand, the decreased reliance on revolving credit can lead to lower interest rates paid by treated individuals. On the other hand, the treatment can directly affect treated individuals' ability to search for better conditions in the market and negotiate better rates. Panel D in Table 1 shows limited effects of personal finance lessons on the terms of loan agreements. Median interest rates linked to micro and small enterprises decreased by 1 percentage point or, alternatively, by 1.5%. This effect on the price of productive loans is small and does not survive multiple hypothesis testing.[15]

To address sample selection concerns inherent to observing interest rates only among active

[15] Table A.6 in the Appendix shows that the coefficients presented in panel D of Table 1 are not driven by sample selection (see column 2).

borrowers, columns (3) and (4) of Table A.6 in the Appendix present estimates on constructed individual-level monthly financial costs using the full experimental sample. Because these estimates stem from Poisson specifications, coefficients are interpreted as percentage changes. Column (3) evaluates the equivalent monthly debt service, which incorporates both principal and interest payments. We document a significant 14.6% reduction in revolving debt service obligations and a 27.6% decline in structured non-revolving debt service. Crucially, the drop in non-revolving debt service occurs despite the expansion in absolute non-revolving volumes documented in Table 1, suggesting a strategic shift toward more favorable repayment structures or longer maturities. Column (4) isolates the pure monthly interest expense. We find a significant 14.4% contraction in the interest costs associated with revolving debt, directly reflecting the reduced reliance on high-interest revolving balances, while the interest expense for non-revolving loans remains statistically unchanged. These results do not survive multiple hypothesis testing, but are still suggestive.

Given our cluster-randomized design across 300 schools and large sample size, our empirical framework yields strong precision boundaries. Our Minimum Detectable Effects (MDEs) scale strictly as a function of our clustered standard errors ($MDE \approx 2.8 \times SE$), with full details provided in Appendix Table A.3. For the extensive-margin probability of borrowing and the median interest rate, our framework can statistically reject shifts larger than 1.7 and 1.1 percentage points, respectively. This confirms that our key null findings represent precisely estimated economic zeros rather than statistical noise. For log-linear volume metrics, the econometric model is powered to detect overall debt balance and average loan size shifts of 10.6% and 9.5%. Because our observed aggregate treatment effects (7.6% and 7.8%) sit just below these high-bar detection thresholds, they are highly sensitive to multiple hypothesis testing adjustments. Consequently, we treat aggregate volume shifts as suggestive and instead anchor our robust conclusions on credit portfolio composition. Crucially, the expansion in non-revolving consumption balances (14.5%) nearly clears its MDE threshold of 15.1%, explaining why it robustly survives conservative Romano-Wolf corrections at the 5% level and confirms a strategic shift toward structured installment credit. In the case of median interest rates, our econometric model is powered to detect overall changes of 1.1 percentage points. Since our observed treatment effects sit below these thresholds, these results are sensitive to multiple hypothesis testing adjustments.

These findings collectively indicate a strategic optimization of the debt portfolio rather than a speculative accumulation of credit. Treated individuals successfully transition away from high-interest revolving credit toward structured, non-revolving loans. However, a critical policy question remains: does this structural reallocation of debt compromise overall repayment capacity or induce over-indebtedness? Table 2 confirms that this portfolio restructuring does not come at the expense of financial distress. As shown in columns (1) and (3), the treatment exerts no significant impact on the probability of holding overdue or written-off debt. Furthermore, the intensive margin reveals that both overdue and written-off debt balances remain statistically and economically indistinguishable from the levels observed in the control group (columns 2 and 4).

As detailed in Sub-section 3.1, the average annual active interest rate (TEA) for credit cards in Peru is exceptionally high, typically exceeding 70%. In contrast, non-revolving personal loans range from 10% to 55% TEA. By substituting revolving for non-revolving loans, treated students are achieving the same smoothing goals at a significantly lower cost. Back of the envelope calculations show significant improvements in the cost efficiency of the credit portfolios among treated individuals. The shift away from high-cost revolving credit (mean rate of 83.9%) toward lower-cost non-revolving and MSE loans (mean rates of 66.4% and 64.8%, respectively) led to a 0.5 percentage point reduction in the weighted average interest rate (WAIR) of the treated group's portfolio compared to the control group (from 57.68% to 57.18%). This shift translates to an annual welfare gain (interest savings) of roughly 7 nuevos soles per borrower, calculated as the difference between the treated group's actual interest bill and the counterfactual bill they would have paid at the control group's WAIR. To put this into perspective, this welfare gain represents roughly a 1.1% reduction in the control group's total annual interest bill, which stands at 654.72 nuevos soles (extrapolated from an average monthly cost of 54.56 soles). While the absolute monetary gain is modest, it demonstrates a persistent, more efficient management of expensive credit portfolios driven by the intervention. A 1.1% structural decrease in borrowing costs per year accumulates significantly over a lifetime.

Financial education often emphasizes the importance of financial literacy not only for managing personal finances, but also for understanding the broader economic landscape, which includes the benefits of formal employment and formal business operations. By equipping students with the knowledge of credit management, budgeting, and financing options, they may be better prepared to navigate the pathways to formality. Furthermore, formal employment and business ownership typically enhance access to financial products and support systems, which are crucial for long-term financial success.

However, as presented in Table 3, our results indicate that participation in the program does not lead to significant changes in formal employment or business formation. Column (1) presents a null effect on formality, either through employment or business ownership, while columns (2) and (3) examine these two distinct probabilities. This lack of impact suggests that while financial education can enhance individuals' financial decision-making, it may not be sufficient on its own to drive structural changes in formality within the labor market or entrepreneurial landscape. Future research could explore complementary interventions that should accompany financial education initiatives. Moreover, column (4) shows that there is no intensive margin effect on income among formal workers: annual average monthly labor earnings in the treatment group do not differ from those in the control group.

The results on formality differ from the evidence presented in Bruhn et al. [2025] and Horn et al. [2023]. The Brazilian program led to lower levels of formality, driven by an increased likelihood of becoming an informal business owner. This divergence from the Peruvian case may be linked to a curriculum focus on entrepreneurship, emphasized through case studies of successful role models in the textbooks used in the classroom. Beyond variations in curriculum design, differences in data collection methods and target populations also explain why our long-term income results diverge from other prominent youth interventions, such as the one evaluated by Horn et al. [2023] in Uganda. Horn et al. [2023] found significant positive effects on savings and income five years after their youth club intervention. These downstream effects are likely explained by the different target population—older adults (average age 24 at baseline), one-third of whom were household heads and only 40% attending school at the time of the intervention. More importantly, Horn et al. [2023] rely on survey data to measure total income, coming both from formal and informal sources. In our context, where 7 out of 10 workers are employed informally, our results on formal income do not fully capture actual changes in total income. While formal income does not yet show a significant increase in the Peruvian case, the accumulation of productive debt serves as a leading indicator of entrepreneurial activity that typically precedes formal income growth. Moreover, since the boundaries between household and business finance are inherently blurred in predominantly informal settings, micro-entrepreneurs frequently capitalize their ventures using personal non-revolving installment loans to bypass high barriers to formal commercial credit [Banerjee and Duflo, 2007; Karlan and Zinman, 2009; Tundui and Tundui, 2024]. Consequently, the absence of an effect on formal administrative records may reflect a measurement gap rather than a lack of underlying economic productivity.

Taken together, the results presented in Tables 1-3 suggest that financial illiteracy manifests as inefficiency in the credit portfolio. Rather than acting as an absolute barrier to formal credit entry—as evidenced by the lack of significant impacts on the extensive margin of borrowing (Table 1, Panel A)—financial illiteracy primarily leads to allocative inefficiency. Absent financial education, individuals tend to borrow suboptimally, disproportionately relying on expensive, flexible revolving credit lines and securing smaller, fragmented loan sizes that likely incur high fixed administrative or psychological transaction costs.

The introduction of the financial literacy program alleviates this friction, enabling individuals to optimize their credit portfolios along the intensive margin. This optimization operates through a compositional shift: borrowers actively substitute away from high-cost revolving balances and consolidate their liabilities into larger, non-revolving formal loans. These results suggest that the primary return to financial education in emerging markets may not be credit expansion, but rather the mitigation of expensive borrowing mistakes and the optimization of existing liability structures.

5.2 Heterogeneous Treatment Impacts

Very few experimental studies have provided any sort of heterogeneity analysis relating to the treatment impacts of financial education interventions. Two notable exceptions are worth highlighting. Focusing on secondary students, Bover et al. [2024] analyze a school-based financial education program in Spain and document homogeneous average improvements in financial knowledge across public and private schools. However, students in public schools, often coming from more disadvantaged backgrounds, experienced larger gains at the lower end of the initial financial literacy distribution, highlighting the potential of financial education to reduce inequality. Along the same lines, Bruhn et al. [2016] find that the financial education program implemented in Brazilian secondary schools proved suitable for all types of students, achieving distribution-wide improvement in financial literacy scores, which translated into significant gains at both the low and high ends of the performance spectrum. Focusing on elementary school students, Alan and Ertac [2018] evaluate the impact of a program aimed at enhancing individuals' ability to think ahead and exercise self-control in intertemporal decision-making. While the program shows a broadly uniform effect across children with different characteristics such as gender, academic performance, and SES in the short term, its influence on patience persists in the long term, particularly among girls, high-performing students, and those who initially made dynamically consistent choices.

Analysis based on the Peruvian experiment presented in Frisancho [2019] reveals that individual traits, parental background, baseline academic performance (as measured by average or math grades) or baseline financial skills do not significantly mediate the impact of financial education on financial literacy in the short run. Notably, ownership of increasingly valuable household assets — particularly access to technology — amplifies learning gains, although the effect size is small. In general, the intervention's impact on knowledge in the short run is uniform along the entire distribution of initial financial skills and academic performance, suggesting that financial education benefits all students equally, regardless of prior knowledge.

But, are these inclusive impacts sustained over the long run? Even if knowledge gains are homogeneously distributed, the effects on downstream behaviors may vary over time, depending on individual and background characteristics. Although working with administrative data offers huge advantages in terms of measurement of actual financial behavior, it does limit the extent of observable individual characteristics. Still, we focus on three key dimensions of potential heterogeneity: sex, baseline academic performance, and SES.

Analysis of baseline test scores from Frisancho [2022] reveal that the overall gender gap in financial literacy pre-treatment was 0.06 standard deviations (SD), with males outperforming females. This gap, however, is not constant, shrinking considerably from 0.12 SD in Grade 9 to 0.02 SD in Grade 11. The reduction of the gender gap over time recorded in our sample is consistent with the most recent OECD/INFE 2020 Survey, which documents a moderate financial literacy gender gap in Peru among adults over 18: on average, men score higher than women, but the gap is less than 4 points out of 100 in financial knowledge [OECD, 2023]. Despite this modest initial knowledge deficit, analysis of public credit bureau records for young consumers (ages 18–24) in Peru reveals only minor gender gaps in overall debt participation, with 20% of women holding formal debt compared to 19% of men. Furthermore, the nature of credit differs, with women accessing micro-enterprise loans (productive credit) more often while men favor non-revolving consumer credit. In summary, data from the Peruvian context suggest that girls start off at a modest disadvantage in terms of baseline financial knowledge while being equally or slightly more engaged with the formal credit market. Therefore, the pilot may yield differential treatment impacts by sex.

Evidence from the economics of education literature supports two facts related to skills acquisition: i) acquiring skills early on facilitates the acquisition of additional skills at later stages (self-productivity), and ii) early investments in skills make later investments more productive (dynamic complementarity) [Cunha et al., 2010]. These facts imply that students with stronger academic skills and cognitive abilities are more likely to advantageously absorb, interpret, and apply new concepts. Therefore, baseline gaps in performance could lead to divergent treatment impacts with educational interventions. In the case of financial education, both knowledge gains as well as transmission to downstream behaviors and their trajectories could be influenced by students' initial performance levels in schools, with higher performing students being more likely to learn and apply the economic and financial concepts taught under the pilot program. Since we lack performance data from a pre-treatment standardized evaluation, we rely on grade point averages (GPAs) in 2015, the academic year prior to the pilot's implementation. To minimize issues with noisy measurement of performance, we divide students based on the median GPA in their school of origin, generating groups of top and bottom performers.

There is ample evidence documenting significant disparities in performance and academic achievement based on SES. Numerous studies have shown that students from higher SES backgrounds tend to outperform their counterparts from lower SES backgrounds across various educational outcomes, reflecting persistent inequalities in access to resources, quality of schooling, and support systems [Blanden et al., 2023]. If financial education is similar to other skills taught while in school, we would expect the pilot to generate greater knowledge gains and stronger ability to apply knowledge among higher SES students. Even though administrative records do not provide a measure of SES at the individual level, we can rely on students' secondary school location as a proxy for poverty. We use data from the 2017 Population and Housing Census to construct a multidimensional poverty index ranging from 0 to 1, where 1 indicates that a household has deprivations in all five dimensions considered: health, education, employment, basic services, and housing, each weighted equally. We define an individual as poor if they attended a school located in a district where 50% or more of the population has at least two out of five deprivations or, equivalently, an index that is greater than or equal to 0.4.

Tables A.7 and A.8 show very homogeneous effects across male and female students. Despite modest initial differences in debt balances, loan sizes, and interest rates favoring male students, the impact of the intervention is very balanced across all credit outcomes. Panel A in Table A.7 confirms that the non-effect on the probability of holding debt persists even when evaluated by sex of the student. Panels B and C show that increases in debt balances and loan sizes are greater among men, but the difference by sex is not statistically significant. Panel D confirms that there are no significant differences by sex in terms of the impacts on interest rates. Moreover, Panel A in Table A.8 shows that both men and women replicate the average result of null impact on the probability of holding delinquent debt. Panel B shows no significant differences by sex in terms of delinquent debt balances. Finally, Table A.9 in the Appendix finds a small difference by sex favoring women in terms of the probability of having a formal business seven years after the intervention. However, this result does not survive multiple hypothesis testing.

The heterogeneous treatment effects by initial academic performance presented in Table 4 reveal stronger gains among higher performing students at baseline. On the one hand, higher performing students experience a significant 2.5 percentage-point increase in their probability of holding formal financial obligations – an effect that is entirely driven by an expansion of the extensive margin with productive and non-revolving credit (see Panel A). Moreover, Panels B and C show that, among top performers, debt balances and loan sizes experience similar increases due to the treatment. These growth along the intensive margin is particularly led by the expansion of non-revolving debt portfolios. Panel D, in turn, shows inclusive gains for bottom and top students in terms of interest rates paid. Please note that selection into borrowing (specifically, the change in who borrows as a result of the treatment among top performers, as shown in Panel A) prevents us from interpreting the coefficients in Panels B through D as causal. These results should therefore be regarded as suggestive. Table 5 rules out differential impacts on the probability of being delinquent or the size of delinquent debt: both top and bottom students replicate the null average impacts reported in Table 2. Table A.10 in the Appendix shows no differences in long-term formality status by academic performance.

Finally, Table A.11 supports inclusive impacts on financial literacy by SES. At baseline, students from lower SES backgrounds face a modest disadvantage relative to students from higher SES backgrounds in terms of the probability of holding debt, but they tend to obtain slightly larger loans and better credit conditions. Despite these baseline differences, Panel A in Table A.11 fails to show any significant divergence in terms of the null effects on the probability of holding debt. Similarly, Panels B and C show homogeneous gains by SES in terms of debt balances and loan sizes across individuals from both high and low SES backgrounds. Panel D also provides evidence of homogeneous impacts by SES. Focusing on the effects on delinquent debt, we do find that the treatment may hurt students from lower SES backgrounds. Table A.12 shows that the treatment significantly increases poor students' balances of delinquent debt by about 30%. However, this result is quite noisy and non-significant. Table A.13 in the Appendix shows no differences in long-term formality status by SES.

Taken together, our results confirm that the inclusive impacts of financial education identified in Frisancho [2019] are partially sustained over time.[16] On the one hand, the long-term effects of school-based financial education seem to be equitable across sex and SES. On

[16]Columns 1 and 4 in Table A.1 in the Appendix shows that the survey subsample is representative of the broader population covered in the administrative records. Thus, we argue that differences between short-run and long-run effects reported in the paper reflect temporal dynamics rather than changes in sample composition.

the other hand, higher performing students derive greater long-term benefits from financial education programs, particularly in terms of their access to and usage of credit. This result is quite interesting, since prior evidence drawn from the same study sample failed to find differential impacts on financial literacy in the short run. These two findings imply that the financial education program in Peru did not widen financial literacy inequalities by initial performance in the short run; however, top students were in a better position to absorb and apply new concepts over time, which led to differential effects in their downstream financial behavior. Interestingly, the advantage in credit outcomes recorded among top-performing treated students is not confounded by differential treatment impacts by SES.

We also tried to check if early gains in financial literacy condition longer-term impacts on credit behavior. We linked the administrative data with the subsample of students who participated in the short-run survey and calculated a residual from a regression of the exit exam score on the baseline score and a set of individual controls. This residual serves as a proxy for the specific impact of the program on each student's financial knowledge, independent of their initial ability. We then interacted the treatment dummy with an indicator for being below the median of this residual distribution. The results, presented in Table A.17 in the Appendix, show that the program's long-term effects on credit outcomes are robust across different levels of short-term learning intensity. Across all panels, the p-value for equal effects suggests that we cannot reject that the treatment impact in the long run is similar for both high and low learners. This indicates that the program's influence on long-term financial behavior was not restricted to only those who showed the highest immediate financial literacy improvement.

Given the variation in the material imparted across grades, we can further explore the role of curriculum design on the observed treatment effects. This is particularly relevant given that the curriculum was tailored to be age-appropriate, shifting from foundational concepts to complex financial decision-making as students progressed. Tables A.14-A.16 in the Appendix examine whether the impact of the intervention varies by the grade of the students. While the treatment effects on the probability of holding debt and median interest rates remain remarkably consistent across cohorts, we observe notable heterogeneity in debt balances. As shown in Table A.14, the reduction in revolving debt balances is significantly more pronounced for students in 10th grade. This result mirrors the curriculum's evolution: while 9th graders focused on general budgeting and the distinction between needs and resources, the older cohorts received targeted instruction on financial products, forward-looking choices, and responsible consumption. In particular, 10th-graders focused on financial products and services, financial system, and adequate use of financial products. These results suggest that specific instruction on financial products is a more effective lever for curbing the accumulation of high-cost revolving debt. We do not find any significant differences in the effects by grade on overdue and written off debt (see Table A.15) or on formality and earnings (see Table A.16).

5.3 Dynamic Treatment Impacts on Credit Behavior

The multi-year panel spanning 2017 to 2023 allows us to trace out the trajectory of treatment effects over seven years of individual credit histories. This dynamic analysis serves two purposes: it captures how the effects of a school-based intervention mature as participants transition into adulthood, and it offers a unique window into how financial education provides a buffer against severe macro-economic shocks, specifically the COVID-19 pandemic. The COVID-19 pandemic introduced unprecedented economic challenges, resulting in significant shifts in financial circumstances, consumer behavior, and credit access. By focusing on these critical years, we can assess how financial education equipped individuals to navigate financial hardships, such as job losses and reduced income, while managing their credit effectively.

The dynamic effects are presented in Figures 1 through 3, where coefficients are estimated via separate cross-sectional regressions for each year. All outcomes are defined symmetrically to the main specifications in Tables 1 and 2. For the extensive margin (Figure 1 and Panel a in Figure 3), the dependent variable is a binary indicator taking a value of 1 if the individual held a positive balance in that specific debt category at any point between January and December of that year. For the intensive margin (Figure 2 and Panel b in Figure 3), the dependent variable is the continuous credit balance measured precisely as a snapshot in December of the respective year. Because the continuous balances in Table 1 (Panel B) utilize this exact same December 2023 window and specification, the average treatment effects reported in our main tables correspond to the final-period point estimates plotted in the dynamic treatment effects figures.[17]

The joint analysis of the yearly coefficients reveals an interplay between the extensive and

[17] Visually, several yearly point estimates in the early post-intervention period appear compressed against the baseline. This is an artifact of graphical axis scaling rather than an absence of shifting dynamics. Because credit market participation is initially low and early-career balances are highly volatile, the standard errors around these early periods are wide. To accommodate these large confidence intervals, the y-axis scale expands, compressing later-year point estimates visually. Additionally, for revolving debt balances (Figure 2, Panel c), coefficients for 2017 and 2018 are not reported due to model non-convergence, stemming from the low frequency of strictly positive debt values in those early years. To ensure complete transparency, Appendix Tables A.18-A.20 provides the exact point estimates, standard errors, and Romano-Wolf adjusted p-values.

intensive margins over time. Figure 1 shows that, by the final period under study (December 2023), the treatment effects on the probability of holding debt across all categories converge toward zero and lack statistical significance. However, focusing only on this end-point hides intermediate effects in the panel. Specifically, starting in 2019 and peaking during 2021-2022, treated individuals show a statistically significant decrease in the probability of holding revolving credit lines (Panel c). This result indicates that treated individuals resisted turning to expensive credit cards or revolving overdrafts to smooth the income shock brought upon by the pandemic. Instead, their probability of using revolving credit dropped significantly, and revolving debt balances reached their lowest point in 2021 (see panel c in Figure 2). Simultaneously, the treatment led to a suggestive surge in productive credit usage in 2021, directly coinciding with the phasing out of emergency government cash transfers. This indicates a targeted pivot toward self-employment and income-generating entrepreneurial activities when formal labor markets collapsed.

Rather than indicating a temporary advantage that disappears by 2023, the evolution of extensive margin effects reflects a catch-up process by the control group as macroeconomic conditions normalized after the pandemic. During the crisis (2020–2021), credit access froze up. The control group got locked out or relied on high-interest revolving lines. The treated group successfully shut down revolving lines while opened formal MSE credit channels. In other words, the treatment enables resilience to respond to the shock. Once the macroeconomic shock subsided and overall credit access unfroze, the extensive margin gap naturally closed.

However, early effects on the extensive margin secured by the treated group permanently altered their subsequent credit trajectory. That is, the temporary extensive-margin advantage during the crisis served as the catalyst for the long-term intensive-margin shift observed at the end of our sample period. While both groups became equally likely to hold debt by 2023, the treated group used their established financial relationships to reorganize their portfolios on the intensive margin. This is shown in Figure 2: revolving credit balances (Panel c) remain persistently lower for the treated group. In turn, non-revolving debt balances (Panel d) remained flat during the pandemic before embarking on a steady upward trajectory that culminates in the significant positive impacts observed by December 2023. This pattern indicates that financial education enables individuals to swiftly correct costly mistakes on the revolving credit margin, but embarking on a complete structural shift toward cheaper, fixed-term sources of credit required a longer window of maturity. Importantly, these behavioral adjustments persist over the long run.

During the pandemic, financial stress increased significantly among households, leading to heightened vulnerability in the financial system. Many families faced unprecedented economic challenges, including job losses, reduced incomes, and increased expenses related to health and safety measures. As a result, default levels surged, with more households unable to meet their financial obligations, particularly in terms of loan repayments and credit card debts. On average, a third of the households in Latin American and Caribbean countries ceased to pay rent or loan obligations [Camacho and Hernandez, 2023]. The data at the country level show that a comparatively greater share of households in Peru were faced with the prospect of having to halt rent or loan payments to meet more pressing needs (41%). Data from the SBS show that, before the pandemic, financial system clients allocated an average of 6% of their credit principal balance to monthly payments. However, this indicator plummeted to 1.6% in April 2020, reflecting the severe economic stresses of the time [SBS, 2021]. While there was a gradual recovery, it was only toward the end of 2021 that this indicator came close to pre-pandemic levels, highlighting the persistent impact of the crisis on the repayment capacity of financial system users.

Figure 3 tracks the dynamic treatment impacts on loan delinquency to evaluate whether the restructuring of debt portfolios, particularly during the pandemic, triggered heightened financial distress. The results show an absence of treatment effects on both default margins. Panel A reveals that the program had no impact on the binary probability of carrying overdue or written-off debt across the entire multi-year panel. Similarly, Panel B shows that the intensive margin of delinquent debt balances remained statistically indistinguishable from zero in all periods, including the pandemic years.

This neutral effect on default is good news. Typically, an intensive-margin expansion of formal credit is accompanied by a proportional rise in credit risk and repayment strain. The fact that treated individuals accumulated higher structured credit balances without experiencing any parallel increase in default probabilities or default volumes over time suggests that the program fostered sustainable financial depth. Financial education equipped individuals to expand their formal borrowing structures while keeping up with responsible repayment.

5.4 Cost-Effectiveness

To evaluate the economic viability of the intervention and directly quantify its long-term return, this section presents a formal cost-benefit calculation. The intervention was designed for scalability by integrating financial literacy into regular classes during the school day in public secondary schools. Frisancho [2022] previously estimated the marginal cost of the program at $4.80 USD per student. This figure accounts for teacher training and the provision of pedagogical materials, while leveraging existing school infrastructure and instructional hours to minimize overhead.

While the intervention did not alter the probability of holding debt, it significantly influenced the composition of borrowers' portfolios. We value the financial return of this structural reallocation using the Weighted Average Interest Rate (WAIR) detailed in Subsection 5.1.[18] Based on the average treated balance of S/ 1,412, this efficiency gain translates to an annual interest saving of S/ 10 per borrower (approximately $2.75 USD). Under these parameters, the program's upfront investment of $4.80 USD is fully recovered within 1.75 years of active credit market participation. Assuming a conservative 10-year horizon and a standard 5% discount rate, the Net Present Value (NPV) of these interest savings stands at $21.24 USD per participant, yielding a highly favorable benefit-cost ratio of 4.41.

As discussed in Subsection 5.1, the absence of an immediate, detectable impact on formal business ownership (Table 3) or formal income must be interpreted within the specific context of an emerging credit market characterized by high labor informality. Because early-stage entrepreneurial activity and self-employment among young cohorts in Peru predominantly occur outside formal registries, these outcomes escape tracking in administrative data. Given our dynamic results, showing a significant expansion in productive MSE credit during the pandemic (Figures 1 and 2), it is highly probable that treated individuals utilized this liquidity to scale informal micro-enterprises. Consequently, our cost-benefit parameters should be interpreted as a conservative lower bound, as they omit the broader, unobserved welfare and productivity gains generated within the informal economic sector.

6 Conclusion

This paper provides novel, large-scale experimental evidence on the long-term impacts of school-based financial education. By tracking nearly 60,000 individuals over a seven-year horizon using comprehensive administrative credit, pension, and tax registries in Peru, we overcome the common limitations of self-reported data and short-term post-intervention horizons. Our findings demonstrate that early-stage personal finance interventions can induce

[18] Evaluating portfolio shifts via the WAIR is critical: unlike unweighted median interest rates—which treat a minor revolving overdraft and a large structured balance with equal weight—the WAIR accurately captures changes in the aggregate interest burden relative to total exposure.

deep, structural shifts in financial behavior that persist well into adulthood.

The primary takeaway is that while the intervention does not alter the extensive margin of holding formal debt, it fundamentally reshapes the composition of borrowers' credit portfolios. Rather than fueling reckless credit expansion, financial education addresses allocative inefficiencies by prompting a strategic shift away from high-interest revolving consumption debt and toward structured, installment-based non-revolving credit (representing a robust 14.5% expansion). This portfolio recomposition achieves consumption-smoothing goals at a lower financial cost without compromising repayment capacity, as evidenced by precisely estimated null effects on overdue or written-off debt balances.

These results underscore the role of financial education as a mechanism for long-term behavioral change. While the specific knowledge acquired during a program may partially decay over time, the intervention's efficacy is sustained by the development of navigational competence. Financial education equips individuals to identify and interpret financial data within real-world contexts. The development of these competences ensures that the benefits of literacy persist throughout their adult lives.

Our analysis also offers crucial insights for the optimal design of future programs. Importantly, the impacts were broadly equitable across gender and SES, with no evidence of exacerbating existing inequalities in access to credit. Higher performing students experienced larger and more persistent gains – both in terms of credit access and the amount borrowed – which suggests that prior skills and capabilities influence how students absorb and apply financial knowledge over time. Since earlier evidence showed no differential impacts on financial literacy across different initial performance levels, these novel results suggest that the financial education program in Peru did not increase initial disparities in financial literacy; however, top students were better able to absorb and apply new concepts over time, resulting in divergent effects in their subsequent financial behaviors. Overall, the heterogeneous treatment results presented here challenge the common assumption that educational interventions tend to widen socioeconomic or skill-related gaps. Instead, we find that the effects of financial education are largely uniform, with only modest heterogeneity favoring higher performing students. This suggests that delivering high-quality financial education at scale has the potential to be an inclusive policy, capable of benefiting diverse groups without increasing existing inequalities.

Furthermore, our multi-year panel underscores the protective role of financial literacy during economic downturns. During the COVID-19 pandemic, treated individuals exhibited heightened financial resilience, contracting their reliance on expensive revolving credit balances and expanding access to productive MSE loans. This strategic response during a major economic crisis clearly shows how early provision of financial education can turn into an effective buffer in the long-term.

Policy implications are clear: scaling up school-based financial education offers a promising, cost-effective avenue to improve financial literacy, promote responsible borrowing, and enhance household resilience amid economic uncertainties. Our evidence shows that well-designed, large-scale financial education programs can produce durable benefits across a broad population, contributing to improved financial behaviors, reduced vulnerability, and potentially greater financial inclusion. Given the persistently low levels of financial literacy in developing countries, these findings reinforce the importance of integrating financial education as a core component of broader development strategies aimed at reducing poverty and promoting shared prosperity.

References

Agarwal, S., Driscoll, J. C., Gabaix, X., and Laibson, D. (2009). The Age of Reason: Financial Decisions over the Life-Cycle with Implications for Regulation. Brookings Papers on Economic Activity, 2: 51–117.

Alan, S. and Ertac, S. (2018). Fostering Patience in the Classroom: Results from a Randomized Educational Intervention. *Journal of Political Economy*, 126(5):1865–1911.

Banerjee, A. V. and Duflo, E. (2007). The economic lives of the poor. *Journal of Economic Perspectives*, 21(1):141–168.

BCRP (2022). Reporte de Estabilidad Financiera. May.

Blanden, J., Doepke, M., and Stuhler, J. (2023). Educational inequality. In *Handbook of the Economics of Education*, pages 405 – 497. Elsevier.

Bover, O., Hospido, L., and Villanueva, E. (2024). The impact of high school financial education on financial knowledge and saving choices. *Journal of Human Resources*.

Brown, M., Grigsby, J., van der Klaauw, W., Wen, J., and Zafar, B. (2016). Financial Education and the Debt Behavior of the Young. *The Review of Financial Studies*, 29(9):2490–2522.

Bruhn, M., de Souza Leão, L., Legovini, A., Marchetti, R., and Zia, B. (2016). The Impact of High School Financial Education: Evidence from a Large-Scale Evaluation in Brazil. *American Economic Journal: Applied Economics*, 8(4):256–295.

Bruhn, M., Garber, G., Koyama, S., and Zia, B. (2025). The long-term impact of high school financial education: Evidence from brazil. *The Review of Economics and Statistics*, pages 1–45.

Calvet, L. E., Campbell, J. Y., and Sodini, P. (2009). Measuring the financial sophistication of households. *American Economic Review*, 99(2):393–98.

Camacho, A. and Hernandez, P. (2023). Financial distress in Latin America and the Caribbean during the COVID-19 pandemic: coping mechanisms and unfolding consequences. Technical report, UNDP Policy brief.

Choi, J. J., Laibson, D., and Madrian, B. C. (2009). Why does the law of one price fail? an experiment on index mutual funds. *The Review of Financial Studies*, 23(4):1405–1432.

Clark, R. L., Lusardi, A., and Mitchell, O. S. (2021). Financial fragility during the covid-19 pandemic. *AEA Papers and Proceedings*, 111:292–96.

Cole, S., Sampson, T., and Zia, B. (2011). Prices or knowledge? what drives demand for financial services in emerging markets? *The Journal of Finance*, 66(6):1933–1967.

Cunha, F., Heckman, J. J., and Schennach, S. M. (2010). Estimating the technology of cognitive and noncognitive skill formation. *Econometrica*, 78(3):883–931.

Disney, R. and Gathergood, J. (2013). Financial literacy and consumer credit portfolios. *Journal of Banking Finance*, 37(7):2246–2254.

Fairlie, R. W. and Krashinsky, H. A. (2012). Liquidity constraints, household wealth, and entrepreneurship revisited. *Journal of Labor Economics*, 30(2):279–306.

Frisancho, V. (2019). The impact of financial education for youth. *Economics of Education Review*, page 101918.

Frisancho, V. (2022). Is school-based financial education effective? immediate and long-lasting impacts on high school students. *The Economic Journal*, 133(651):1147–1180.

Frisancho, V. (2023). Spillover effects of financial education: The impact of school-based programs on parents. *Journal of Financial Literacy and Wellbeing*, 1(1):138–153.

Gathergood, J. (2012). Self-control, financial literacy and consumer over-indebtness. *Journal of Economic Psychology*, 33:590–602.

Guiso, L., Pozzi, A., Tsoy, A., Gambacorta, L., and Mistrulli, P. E. (2022). The cost of steering in financial markets: Evidence from the mortgage market. *Journal of Financial Economics*, 143(3):1209–1226.

Horn, S., Jamison, J. C., Karlan, D., and Zinman, J. (2023). Five-year impacts of group-based financial education and savings promotion for ugandan youth. *The Review of Economics and Statistics*, pages 1–53.

Hu, M., Lin, Z., and Liu, Y. (2024). Financial literacy and mortgage stress. *Journal of Banking / Finance*, 163:107170.

Hurst, E. and Lusardi, A. (2004). Liquidity constraints, household wealth, and entrepreneurship. *Journal of Political Economy*, 112(2):319–347.

Hvidberg, K. B. (2023). Field of study and financial problems: How economics reduces the risk of default. *The Review of Financial Studies*, 36(11):4677–4711.

Jappelli, T. and Padula, M. (2013). Investment in financial literacy and saving decisions. *Journal of Banking and Finance*, 37:2779–2792.

Kaiser, T. and Menkhoff, L. (2019). Financial education in schools: A meta-analysis of experimental studies. *Economics of Education Review*.

Kaiser, T., Menkhoff, L., Lusardi, A., and Urban, C. (2022). Financial education affects financial knowledge and downstream behaviors. *Journal of Financial Economics*, 145(2):255–272.

Karlan, D. and Zinman, J. (2009). Expanding credit access: Using randomized supply decisions to estimate the impacts. *The Review of Financial Studies*, 23(1):433–464.

Lee, D., Mangrum, D., van der Klaauw, W., and Wang, C. (2024). Financial education and household financial decisions during the pandemic. Federal Reserve Bank of New York Staff Reports, no. 1131.

Lee, S., Kim, K. T., and Hanna, S. D. (2025). Covid-19 labor market shocks and withdrawals from retirement accounts: Understanding the moderating role of financial knowledge. *The Journal of Consumer Affairs*, 59(1).

Levy, M. and Tasoff, J. (2016). Exponential-growth bias and lifecycle consumption. *Journal of the European Economic Association*, 14(3):545–583.

Lusardi, A. and de Bassa Scheresberg, C. (2013). Financial Literacy and High-Cost Borrowing in the United States. NBER Working Paper No. 18969.

Lusardi, A. and Kaiser, T. (2025). 326financial literacy and financial education: An overview. In *The Oxford Handbook of Banking: 4th Edition*. Oxford University Press.

Lusardi, A., Michaud, P.-C., and Mitchell, O. (2017). Optimal financial knowledge and wealth inequality. *Journal of Political Economy*, 125(2):431–477.

Lusardi, A. and Mitchell, O. (2014). The economic importance of financial literacy: Theory and evidence. *Journal of Economic Literature*, 52(1):5–44.

Lusardi, A. and Mitchell, O. S. (2008). Planning and financial literacy: How do women fare? *American Economic Review*, 98(2):413–17.

Lusardi, A. and Mitchell, O. S. (2011). Financial literacy and retirement planning in the united states. *Journal of Pension Economics and Finance*, 10(4):509–525.

Lusardi, A. and Tufano, P. (2015). Debt literacy, financial experiences, and overindebtedness. *Journal of Pension Economics and Finance*, 14(4):332–368.

McKenzie, D. (2012). Beyond baseline and follow-up: The case for more t in experiments. *Journal of Development Economics*, 99(2):210 – 221.

Mustonen, J., Buturak, G., Ruuskanen, O.-P., Kalmi, P., and Vaahtoniemi, S. (2025). The role of financial literacy in payment defaults. *Applied Economics*, 0(0):1–18.

OECD (2023). Joining forces for gender equality: What is holding us back?

Romano, J. P. and Wolf, M. (2005). Exact and approximate stepdown methods for multiple hypothesis testing. *Journal of the American Statistical Association*, 100:94–108.

SBS (2021). Informe de Estabilidad del Sistema Financiero. Technical report, Noviembre, SBS series.

Solis, A. (2017). Credit access and college enrollment. *Journal of Political Economy*, 125(2):562–622.

Spataro, L. and Corsini, L. (2017). Endogenous financial literacy, saving, and stock market participation. *FinanzArchiv / Public Finance Analysis*, 73(2):135–162.

Stoddard, C. and Urban, C. (2020). The Effects of State-Mandated Financial Education on College Financing Behaviors. *Journal of Money, Credit, and Banking*, 52(4):747–776.

Tundui, C. S. and Tundui, H. P. (2024). Microcredit fungibility and effect on business performance among women entrepreneurs in tanzania. *Journal of Global Entrepreneurship Research*, 14:2251–7316.

Urban, C., Schmeiser, M., Collins, M., and Brown, A. (2020). The Effects of High School Personal Financial Education Policies on Financial Behavior. *Economics of Education Review*, 78.

van Rooij, M., Lusardi, A., and Alessie, R. (2011). Financial literacy and stock market participation. *Journal of Financial Economics*, 101(2):449–472.

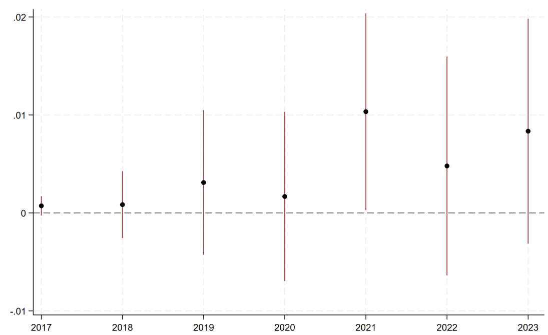
van Rooij, M. C., Lusardi, A., and Alessie, R. J. (2012). Financial Literacy, Retirement Planning and Household Wealth. *The Economic Journal*, 122(560):449–478.

Willis, L. (2011). The financial education fallacy. *American Economic Review Papers and Proceedings*, 101(3):429–434.

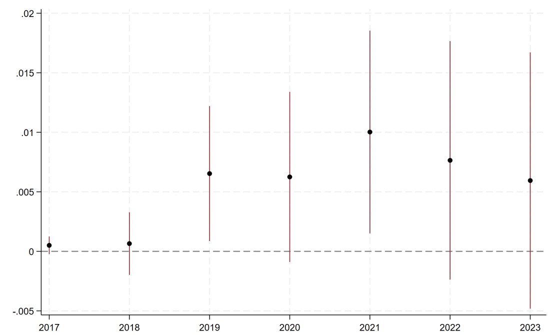
Wooldridge, J. M. (2010). *Econometric Analysis of Cross Section and Panel Data*. MIT Press, Cambridge, MA, 2 edition.

Figures and Tables

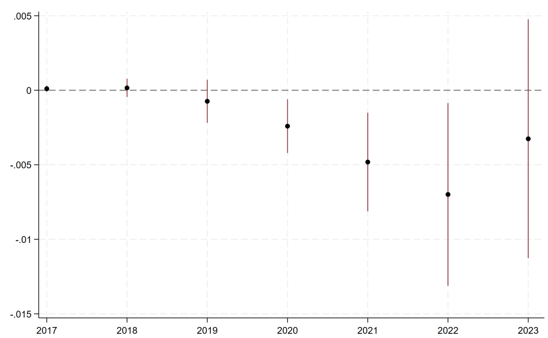
Figure 1: Dynamic Effects on the Probability of Holding Debt



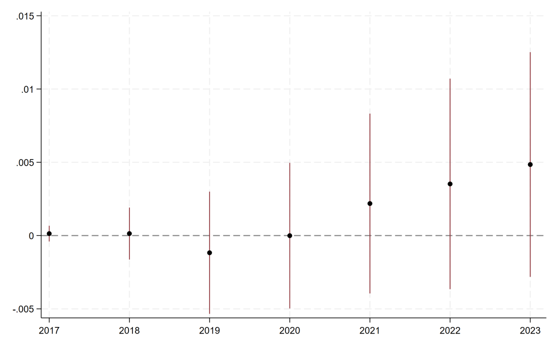
(a) Total



(b) MSE



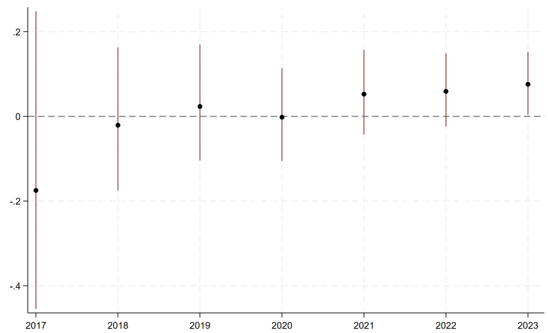
(c) Revolving



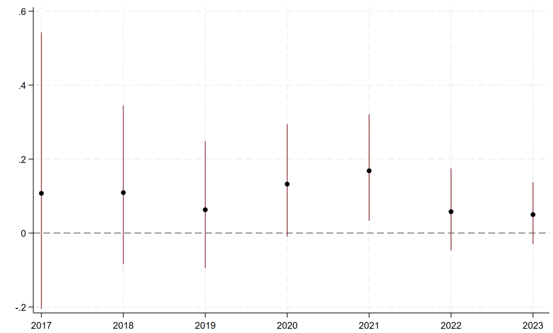
(d) Non-Revolving

Note: The figure plots the estimated effect of the treatment by year on the probability of having direct debt. The dependent variable is a binary indicator, defined consistently with the variables in Table 1: it takes a value of 1 if the individual had a positive balance in the specific type of debt at any point between January and December of each year. All regressions include gender, age, baseline GPA, SES, and missing-value indicators for these controls, as well as paired fixed effects, and are estimated using OLS. Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the school level. Yearly coefficients for the early post-intervention years are not reported because the model did not converge. This is attributed to the low frequency of strictly positive debt values in those periods, which provided insufficient variation for estimation.

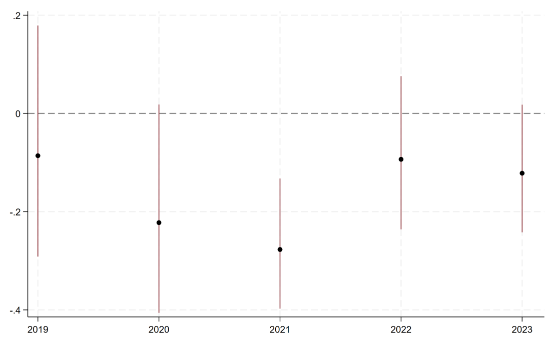
Figure 2: Dynamic Effects on Debt Balances



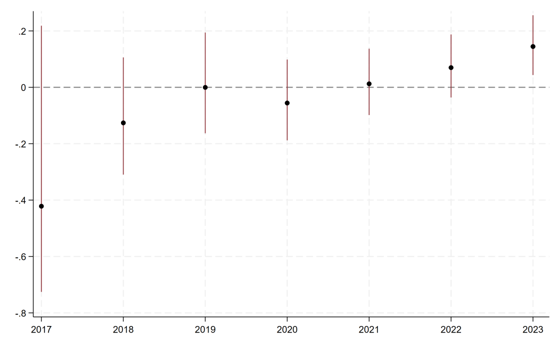
(a) Total



(b) MSE



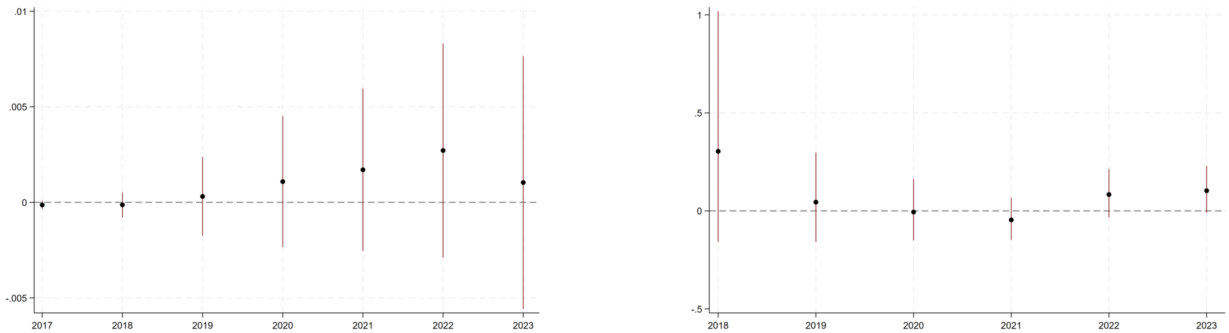
(c) Revolving



(d) Non-Revolving

Note: The figure plots the estimated effect of the treatment by year on debt balances measured in December of each year.The dependent variable is a continuous variable measured in December of each year and defined consistently with the variables in Table 1. The figure reports $\exp(\hat{\beta}) - 1$, based on Poisson regressions that control for gender, age, baseline GPA, SES, and missing-value indicators for these controls, as well as paired fixed effects. Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the school level. Yearly coefficients for the early post-intervention years are not reported because the model did not converge. This is attributed to the low frequency of strictly positive debt values in those periods, which provided insufficient variation for estimation.

Figure 3: Dynamic Effects on Overdue and Written-Off Debt



(a) Pr(Overdue or Written-Off Debt)

(b) Overdue and Written-Off Debt Balances

Note: The figure in Panel (a) plots the estimated effect of the treatment by year on the probability of having overdue or written-off debt. The dependent variable is a binary indicator, defined consistently with the variables in Table 1: it takes a value of 1 if the individual had a positive balance in overdue or written-off debt at any point from January to December of each year. Overdue debt includes past-due loans and loans under judicial collection. Written-off debt is a loan that the lender has classified as uncollectible and removed from its balance sheet. All regressions include gender, age, baseline GPA, SES, and missing-value indicators for these controls, as well as paired fixed effects. The figure in Panel (b) displays the estimated effect of the treatment by year on the sum of overdue and written-off debt balances. The figure reports $exp(\hat{\beta})-1$, based on Poisson regressions that control for gender, age, baseline GPA, SES, and paired fixed effects. Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the school level Yearly coefficients for the early post-intervention years are not reported because the model did not converge. This is attributed to the low frequency of strictly positive debt values in those periods, which provided insufficient variation for estimation.

Table 1. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions

	Total	MSE	Revolving	Non-Revolving
	(1)	(2)	(3)	(4)
Panel A: Probability of Holding Debt				
Treatment	0.008	0.006	-0.003	0.005
	(0.006)	(0.005)	(0.004)	(0.004)
Number of Observations	57,435	57,435	57,435	57,435
Number of schools	300	300	300	300
Mean in Control	0.381	0.196	0.090	0.198
Panel B: Debt Balance				
Treatment	0.076**	0.050	-0.122*	0.145***†
	(0.038)	(0.043)	(0.066)	(0.054)
Number of Observations	57,435	57,435	57,435	57,435
Number of schools	300	300	300	300
Mean in Control	1312.7	502.4	127.2	489.3
Panel C: Average Loan Size				
Treatment	0.078**	0.061	-0.005	0.075*
	(0.034)	(0.046)	(0.107)	(0.045)
Number of Observations	57,435	57,435	57,435	57,435
Number of schools	300	300	300	300
Mean in Control	934.3	565.3	113.9	410.8
Panel D: Median Interest Rate				
Treatment	-0.005	-0.010*	-0.001	0.006
	(0.004)	(0.005)	(0.006)	(0.004)
Number of Observations	16,059	8,733	2,486	7,588
Number of schools	300	300	250	300
Mean in Control	0.681	0.648	0.839	0.664

Notes: Panel A reports binary indicators for 2023 (January–December) equal to one if the individual holds a positive balance in the corresponding debt category in any month. Panel B reports outstanding balances as of December 2023. Panel C reports average loan size, computed as the annual mean of monthly balances between January and December 2023. Panel D reports annual median interest rates, computed from monthly observations over the same period. Data from Panels A through C comes from the RCC and reflect stocks. MSE refers to loans for production, trade, or service activities. Revolving debt includes loans whose outstanding balance may fluctuate with borrower decisions, such as credit cards. Non-revolving debt refers to loans repaid in fixed installments, where payments reduce the outstanding balance without reopening credit availability. Panels A and D are estimated using OLS. Panels B and C are estimated using Poisson regressions, with coefficients reported as $\exp(\hat{\beta}) - 1$. All specifications include controls for age, gender, GPA in 2015, socioeconomic status (SES), and matched-pair fixed effects. For control variables with missing values, missing observations are imputed to zero and missing-value indicators are included. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications, implemented separately within each outcome family across the four debt categories. The implied minimum detectable effects (MDEs) at 80% power are roughly bounded by $2.8 \times$ SE. See Table A.3 for a full discussion of precision bounds.

Table 2. Intention to Treat Effects on Overdue and Written-off Debt

	Pr(Overdue) (1)	Overdue (2)	Pr(Written-Off) (3)	Written-Off (4)
Treatment	-0.001	0.106	0.001	0.098
	(0.002)	(0.097)	(0.003)	(0.067)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.100	73.7	0.097	162.1

Notes: Columns (1) and (3) report binary indicators equal to one if the individual has a positive balance in overdue or written-off debt, respectively, in any month between January and December 2023; estimates are obtained using OLS. Columns (2) and (4) report balances as of December 2023; coefficients are expressed as $\exp(\hat{\beta}) - 1$ from Poisson regressions. Overdue debt includes past-due loans and loans under judicial collection. Written-off debt refers to loans classified as uncollectible and removed from the lender's balance sheet. All specifications control for age, gender, GPA in 2015, socioeconomic status (SES), and missing-value indicators for these controls, and include matched-pair fixed effects. Missing values in the controls are imputed to zero and flagged with the corresponding missing-value indicators. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied to the family of four outcomes reported in this table.

Table 3. Intention to Treat Effects on Formality and Income

	Contributes to PFA or Owns Formal Business (1)	Owns Formal Business (2)	Contributes to PFA (3)	Formal Monthly Income (4)
Treatment	0.001	-0.001	0.001	0.006
	(0.006)	(0.002)	(0.006)	(0.022)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.296	0.043	0.263	307.4

Notes: Column (1) equals one if the individual either contributed to a private pension fund administrator (PFA) in at least one month during 2023 or if she is recorded by SUNAT as owning a formal business in 2023. Column (2) equals one if SUNAT reports formal business ownership in 2023. Column (3) equals one if the individual contributed to a PFA in at least one month during 2023. Column (4) reports average monthly income reported to the PFA during 2023. All specifications include controls for age, gender, GPA in 2015, socioeconomic status (SES), and matched-pair fixed effects. Missing values in control variables are imputed to zero and flagged with missing-value indicators. Columns (1)–(3) are estimated using OLS, while Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied to the family of four outcomes reported in this table.

Table 4. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions, by Academic Performance

	Total (1)	MSE (2)	Revolving (3)	Non-Revolving (4)
Panel A: Probability of Holding Debt				
Treatment	-0.005	0.000	-0.005	-0.004
	(0.009)	(0.008)	(0.005)	(0.006)
Top	-0.058***	-0.058***	0.016***	-0.027***
	(0.008)	(0.006)	(0.004)	(0.006)
Treatment × Top	0.030***††	0.016*	0.004	0.017**
	(0.011)	(0.008)	(0.005)	(0.008)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	0.379	0.197	0.088	0.197
Panel B: Debt Balance				
Treatment	0.014	0.020	-0.083	0.003
	(0.053)	(0.057)	(0.104)	(0.079)
Top	-0.015	-0.218***	0.620***	0.038
	(0.056)	(0.042)	(0.167)	(0.105)
Treatment × Top	0.172**	0.132	-0.064	0.402**†
	(0.092)	(0.095)	(0.124)	(0.188)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	1287.4	505.7	121.8	468.0
Panel C: Average Loan Size				
Treatment	0.015	0.051	-0.018	-0.014
	(0.046)	(0.062)	(0.164)	(0.071)
Top	-0.034	-0.138**	0.641***	0.040
	(0.046)	(0.051)	(0.254)	(0.093)
Treatment × Top	0.132*	0.013	0.027	0.225*
	(0.077)	(0.087)	(0.188)	(0.150)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	927.9	567.2	108.4	401.9
Panel D: Median Interest Rate				
Treatment	-0.005	-0.010	0.003	0.007
	(0.005)	(0.007)	(0.009)	(0.007)
Top	-0.017***	-0.030***	-0.008	-0.019**
	(0.005)	(0.006)	(0.010)	(0.008)
Treatment × Top	-0.002	0.004	-0.010	-0.006
	(0.007)	(0.009)	(0.013)	(0.011)
Number of Observations	14,754	8,051	2,229	6,957
Number of Schools	300	299	245	300
Mean in Control	0.680	0.647	0.839	0.666

Notes: See Table 1 notes for outcome definitions. Heterogeneous effects by academic performance are estimated from pooled regressions that include a treatment indicator, an indicator equal to one if the student's GPA in 2015 is strictly above the within-school median, and their interaction. The omitted group consists of students at or below the within-school median. The coefficient on Treatment corresponds to the effect for students at or below the within-school median GPA; the coefficient on Top captures baseline differences between students above and at or below the median in the control group; and the coefficient on Treatment × Top captures differential effects for students above the median. Panels A and D report OLS estimates. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta})-1$. All specifications include controls for gender, age, socioeconomic status (SES), and matched-pair fixed effects. The sample is restricted to students with observed GPA in 2015. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of eight treatment-related coefficients: the four Treatment coefficients and the four Treatment × Top coefficients.

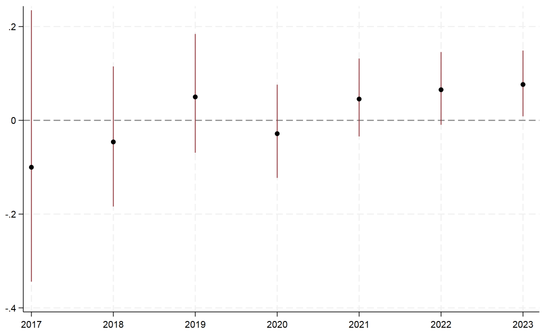
Table 5. Intention to Treat Effects on Overdue and Written-Off Debt, by Academic Performance

	Overdue + Written-Off	Overdue	Written-Off
	(1)	(2)	(3)
Panel A: Probability of Holding Debt			
Treatment	0.006	-0.002	0.005
	(0.005)	(0.004)	(0.005)
Top	-0.063***	-0.042***	-0.048***
	(0.005)	(0.004)	(0.005)
Treatment × Top	-0.007	0.003	-0.005
	(0.007)	(0.005)	(0.006)
Number of Observations	52,849	52,849	52,849
Number of Schools	300	300	300
Mean in Control	0.143	0.098	0.096
Panel B: Debt Balance			
Treatment	0.086	0.160	0.050
	(0.084)	(0.152)	(0.088)
Top	-0.382***	-0.319**	-0.412***
	(0.050)	(0.105)	(0.062)
Treatment × Top	0.081	-0.105	0.184
	(0.149)	(0.232)	(0.182)
Number of Observations	52,849	52,849	52,849
Number of Schools	300	300	300
Mean in Control	228.2	74.1	154.1

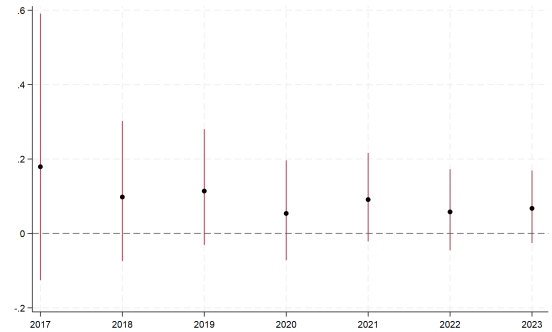
Notes: See Table 2 notes for outcome definitions. Top indicates GPA in 2015 strictly above the within-school median; the omitted group consists of students at or below the within-school median. Heterogeneous effects by academic performance are estimated from pooled regressions that include a treatment indicator, the Top indicator, and their interaction. The coefficient on Treatment corresponds to the treatment effect for students at or below the within-school median GPA; the coefficient on Top captures baseline differences between students above and at or below the median in the control group; and the coefficient on Treatment × Top captures differential treatment effects for students above the median relative to those at or below it. Panel A reports OLS estimates for binary indicators equal to one if overdue or written-off debt is positive in any month between January and December 2023. Panel B reports Poisson estimates for December 2023 balances, expressed as $\exp(\hat{\beta}) - 1$. All specifications include controls for gender, age, socioeconomic status (SES), and matched-pair fixed effects. The sample is restricted to students with observed GPA in 2015. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, ††† 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of six treatment-related coefficients: the three Treatment coefficients and the three Treatment × Top coefficients.

A Appendix: Additional Figures and Tables

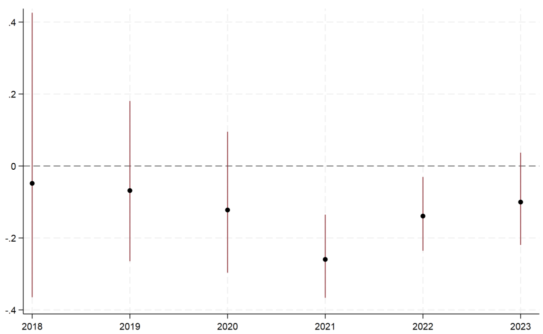
Figure A.1: Dynamic Effects on Annual Average Monthly Debt Balances



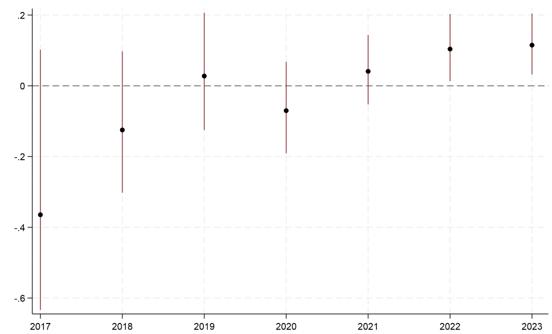
(a) Total



(b) MSE



(c) Revolving



(d) Non-Revolving

Note: The figure replicates Figure 2, but defines the yearly outcome as the annual average of monthly debt balances rather than the balance measured in December of each year. The dependent variable is a continuous variable, computed as the average balance between January and December of each year. Debt categories are defined consistently with the variables in Table 1. The figure reports $\exp(\hat{\beta})-1$, based on Poisson regressions that control for gender, age, baseline GPA, SES, and missing-value indicators for these controls, as well as paired fixed effects. Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the school level. Yearly coefficients for the early post-intervention years are not reported because the model did not converge. This is attributed to the low frequency of strictly positive debt values in those periods, which provided insufficient variation for estimation.

Table A.1. Balance of Baseline Characteristics

Variable	Full sample			Survey sample		
	Control mean	Treatment-Control	N	Control mean	Treatment-Control	N
	(1)	(2)	(3)	(4)	(5)	(6)
Male	0.499	0.012	57,435	0.484	0.012	15,265
	[0.500]	[0.025]		[0.500]	[0.019]	
Age	22.799	0.010	57,435	22.789	-0.014	15,265
	[1.043]	[0.021]		[1.022]	[0.029]	
GPA 2015 (0–20)	13.674	-0.059	52,849	13.825	-0.033	14,315
	[1.491]	[0.057]		[1.487]	[0.060]	
SES: Poor	0.198	0.014	57,435	0.248	-0.017	15,265
	[0.398]	[0.050]		[0.432]	[0.051]	

Notes: This table reports baseline balance for the full administrative sample and the short-run survey sample. Columns (1)–(3) report the control-group mean, the treatment–control difference, and the number of observations for the full sample. Columns (4)–(6) report the corresponding statistics for the survey sample. Treatment–control differences are estimated from separate OLS regressions of each baseline characteristic on the treatment indicator. Test for joint covariates orthogonality p-values are 0.8726 for the full sample and 0.8900 for the survey sample. Significance levels (* 10%; ** 5%; *** 1%) are based on OLS estimates with standard errors clustered at the school level. Standard errors of treatment–control differences and standard deviations of control means are reported in brackets. The poverty indicator equals one for students enrolled in schools located in poor districts. Age is measured using the CAF administrative age variable, with missing values replaced by the alternative administrative age measure when available. The sample excludes observations above the age trimming threshold used in the main analysis.

Table A.2. Differential Attrition by Treatment

	No controls	Controls
	(1)	(2)
Treatment	-0.002	-0.004
	(0.003)	(0.003)
Number of Observations	60,466	60,466
Number of Schools	300	300
Mean in Control	0.051	0.051

Notes: The dependent variable equals one if the observation is excluded from the estimation sample because age is above the 95th percentile threshold, and zero otherwise. Column (1) reports the specification without controls. Column (2) adds baseline controls and matched-pair fixed effects. Standard errors clustered at the school level are reported in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.3. Minimum Detectable Effects (MDE) and Statistical Precision Analysis

Dependent Variable	Control Mean (1)	Clustered SE (2)	Implied MDE (3)	MDE as % of Control (4)
Probability of holding debt				
Total	0.381	0.006	0.0168	4.41%
MSE	0.196	0.005	0.0140	7.14%
Revolving	0.090	0.004	0.0112	12.44%
Non-Revolving	0.198	0.004	0.0112	5.66%
Debt Balance				
Total	1,312.7	0.038	0.1064	10.64%
MSE	502.4	0.043	0.1204	12.04%
Revolving	127.2	0.066	0.1848	18.48%
Non-Revolving	489.3	0.054	0.1512	15.12%
Average Loan Size				
Total	934.3	0.034	0.0952	9.52%
MSE	565.3	0.046	0.1288	12.88%
Revolving	113.9	0.107	0.2996	29.96%
Non-Revolving	410.8	0.045	0.1260	12.60%
Median Interest Rate				
Total	0.681	0.004	0.0112	1.64%
MSE	0.648	0.005	0.0140	2.16%
Revolving	0.839	0.006	0.0168	2.00%
Non-Revolving	0.664	0.004	0.0112	1.69%

Notes: This table presents the statistical precision and power boundaries corresponding to the main empirical specifications reported in Table 1.

Implied Minimum Detectable Effects (MDEs) are calculated following standard protocols for cluster-randomized experimental designs assuming an 80% statistical power threshold and a 5% significance level ($\alpha = 0.05$, two-tailed). Given that our empirical framework accounts for sample size, school-level clustering ($J = 300$), and the underlying intraclass correlation (ρ), the exact precision boundaries are mapped directly as a linear function of the estimated clustered standard errors ($MDE \approx 2.8 \times \text{SE}$).

The final column, "MDE as % of Control," benchmarks these precision thresholds relative to baseline control group means. For binary indicators in the extensive margin and interest rate panels, the percentage represents the relative baseline shift detectable by the framework; for log-linear metrics in the volume panels (Debt Balance and Average Loan Size), the final column registers the precise semi-elasticity percentage change required for statistical detection.

Table A.4. Intention to Treat Effects on Debt Outcomes and Borrowing Conditions, by Type of Institution

	Private and Public Banks	Microfinance	Others
	(1)	(2)	(3)
Panel A: Probability of Holding Debt			
Treatment	0.006	0.001	0.003
	(0.004)	(0.005)	(0.004)
Number of Observations	57,435	57,435	57,435
Number of schools	300	300	300
P-value	0.175	0.838	0.481
Mean in Control	0.190	0.182	0.112
Panel B: Debt Balance			
Treatment	0.124**	0.053	-0.004
	(0.062)	(0.050)	(0.056)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
P-value	0.034	0.277	0.946
Mean in Control	517.1	584.0	211.5
Panel C: Average Loan Size			
Treatment	0.160***†	0.076*	-0.033
	(0.064)	(0.043)	(0.050)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
P-value	0.007	0.065	0.523
Mean in Control	390.6	474.5	246.3
Panel D: Median Interest Rate			
Treatment	-0.007	-0.006	-0.004
	(0.005)	(0.004)	(0.006)
Number of Observations	6,995	7,258	4,937
Number of Schools	297	299	296
P-value	0.116	0.113	0.529
Mean in Control	0.774	0.502	0.813

Notes: Panel A reports effects on binary indicators equal to one if the individual holds a positive debt balance in the corresponding type of institution in any month between January and December 2023. Panel B reports debt balances as of December 2023. Panel C reports average loan size, computed as the annual average of monthly values in 2023. Panel D reports annual median interest rates. Column (1) refers to private and public banks; Column (2) refers to microfinance institutions; and Column (3) refers to other regulated financial institutions. Panels A and D report OLS estimates. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta})-1$. All specifications include controls for age, gender, GPA in 2015, socioeconomic status (SES), and matched-pair fixed effects. Missing values in control variables are imputed to zero and flagged with missing-value indicators. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of three treatment coefficients.

Table A.5. Effects on Debt, Loan size and Income: Robustness to Functional Form for Outcomes Estimated with Poisson

	Baseline Poisson	OLS Winsorization top 1%	OLS Winsorization top 2%
	(1)	(2)	(3)
Panel A: Debt Balances			
Total	0.076**	58.5*	46.0*
	(0.038)	(33.6)	(26.2)
MSE	0.050	22.2	17.7
	(0.043)	(16.0)	(13.5)
Revolving	-0.122*	-5.7	-3.2
	(0.066)	(5.6)	(3.7)
Non-revolving	0.145***	28.2***	19.8***
	(0.054)	(10.1)	(7.1)
Overdue	0.106	-0.8	-0.1
	(0.097)	(1.5)	(0.8)
Written-off	0.098	3.2	2.3
	(0.067)	(4.4)	(3.2)
Observations	57,435	57,435	57,435
Panel B: Average Loan Size			
Total	0.078**	30.3	25.7
	(0.034)	(20.8)	(17.7)
MSE	0.061	15.7	14.6
	(0.046)	(16.3)	(13.7)
Revolving	-0.005	-2.9	-1.7
	(0.107)	(5.4)	(3.0)
Non-revolving	0.075*	8.9	8.3
	(0.045)	(8.3)	(6.7)
Observations	57,435	57,435	57,435
Panel C: Income			
Income	0.006	2.8	2.5
	(0.022)	(6.8)	(6.7)
Observations	57,435	57,435	57,435

Notes: This table reports robustness checks for the outcomes estimated with Poisson in the main text. Panel A corresponds to Table 1, Panel B, columns (1)–(4), plus Table 2, columns (2) and (4). Panel B corresponds to Table 1, Panel C, columns (1)–(4). Panel C corresponds to Table 3, column (4). Column (1) reproduces the baseline Poisson specification and reports $\exp(\hat{\beta}) - 1$. Columns (2) and (3) replace Poisson with OLS after winsorizing the dependent variable at the top 1% and 2% of its distribution, respectively; OLS coefficients are reported in levels. For outcomes defined in the main text with missing values set to zero, the same recoding is applied before winsorization. All specifications control for gender, age, GPA in 2015, socioeconomic status (SES), missing-value indicators for the control variables, pair fixed effects, and school-level clustered standard errors. Stars denote significance levels based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote significance levels based on Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%), with the correction applied separately within each panel across all estimates reported in that panel, using 1,000 bootstrap replications.

Table A.6. Effects on Interest Rates: Robustness to Sample Selection and Effects on Alternative Monthly Cost Measures

	Interest Rates		*Financial Costs*	
	Baseline OLS	IPW-OLS	Equivalent Monthly Debt Service	Implied Monthly Financial Cost
	(1)	(2)	(3)	(4)
Total	-0.005	-0.005	-0.110	0.014
	(0.004)	(0.004)	(0.130)	(0.037)
Observations	16,059	16,059	57,435	57,435
Schools	300	300	300	300
MSE	-0.010*	-0.009*	0.009	0.025
	(0.005)	(0.005)	(0.193)	(0.045)
Observations	8,733	8,733	57,435	57,435
Schools	300	300	300	300
Revolving	-0.001	-0.006	-0.146*	-0.144*
	(0.006)	(0.005)	(0.073)	(0.071)
Observations	2,486	2,486	57,435	57,435
Schools	250	250	300	300
Non-revolving	0.006	0.003	-0.276**	0.008
	(0.004)	(0.005)	(0.104)	(0.046)
Observations	7,588	7,588	57,435	57,435
Schools	300	300	300	300

Notes: Column (1) reports baseline OLS estimates for annual median interest rates using the sample with observed rates. Column (2) reports inverse-probability-weighted OLS estimates, where weights are constructed as the inverse of the predicted probability that the corresponding interest-rate outcome is observed. Predicted probabilities are obtained from a logit selection equation and trimmed to the [0.01, 0.99] interval. Columns (3) and (4) report treatment effects on two constructed monthly cost measures from Poisson regressions, reported as $\exp(\hat{\beta}) - 1$. Column (3) reports equivalent monthly debt service, which approximates the monthly payment associated with each credit operation, including principal and interest. For non-revolving loans, this measure is based on the original loan amount, the effective annual interest rate converted to a monthly rate, and the scheduled number of installments. For revolving credit, it is based on the outstanding used balance and assumes repayment over a common 12-month horizon. Column (4) reports implied monthly interest costs, computed as the outstanding capital balance multiplied by the monthly interest rate implied by the effective annual rate; this measure captures interest costs only and excludes principal repayment. Operation-level measures are summed at the individual level. Debt categories are defined as in Table 1, and individuals without the corresponding credit product are assigned zero for the relevant outcome in columns (3) and (4). All specifications include controls for gender, age, GPA in 2015, socioeconomic status (SES), and matched-pair fixed effects. Missing values in the control variables are imputed to zero and flagged with missing-value indicators. Standard errors clustered at the school level are reported in parentheses. Observations and schools are reported separately for each outcome and specification. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is implemented separately for the family of four interest-rate outcomes in columns (1) and (2), and within each column across the four debt categories for columns (3) and (4).

Table A.7. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions, by Sex

	Total (1)	MSE (2)	Revolving (3)	Non-Revolving (4)
Panel A: Probability of Holding Debt				
Treatment	0.014*	0.009	-0.003	0.006
	(0.008)	(0.008)	(0.005)	(0.005)
Male	-0.005	-0.077***	0.022***	0.044***
	(0.008)	(0.007)	(0.004)	(0.007)
Treatment × Male	-0.011	-0.007	-0.001	-0.002
	(0.010)	(0.010)	(0.006)	(0.009)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.381	0.196	0.090	0.198
Panel B: Debt Balance				
Treatment	0.043	0.030	-0.211**	0.126
	(0.054)	(0.057)	(0.091)	(0.105)
Male	0.372***	-0.187***	0.263**	1.689***
	(0.074)	(0.050)	(0.125)	(0.248)
Treatment × Male	0.055	0.042	0.200	0.023
	(0.075)	(0.085)	(0.167)	(0.128)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	1312.7	502.4	127.2	489.3
Panel C: Average Loan Size				
Treatment	0.072	0.051	-0.034	0.066
	(0.051)	(0.062)	(0.138)	(0.085)
Male	0.251***	-0.136**	0.293**	0.934***
	(0.064)	(0.058)	(0.156)	(0.153)
Treatment × Male	0.011	0.020	0.054	0.012
	(0.072)	(0.092)	(0.169)	(0.116)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	934.3	565.3	113.9	410.8
Panel D: Median Interest Rate				
Treatment	-0.009*	-0.014**	0.002	0.003
	(0.005)	(0.007)	(0.008)	(0.007)
Male	-0.061***	-0.098***	-0.023**	-0.031***
	(0.006)	(0.008)	(0.009)	(0.008)
Treatment × Male	0.007	0.011	-0.007	0.005
	(0.008)	(0.012)	(0.013)	(0.011)
Number of Observations	16,059	8,733	2,486	7,588
Number of Schools	300	300	250	300
Mean in Control	0.681	0.648	0.839	0.664

Notes: See notes in Table 1 for definitions of dependent variables. Heterogeneous treatment effects by sex are estimated using pooled regressions that include a treatment indicator, a male indicator, and their interaction. The coefficient on Treatment corresponds to the treatment effect for females, the omitted group; the coefficient on Male captures baseline differences between males and females in the control group; and the coefficient on Treatment × Male captures differential treatment effects for males relative to females. Panels A and D are estimated using OLS. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include controls for age, GPA in 2015, socioeconomic status (SES), and matched-pair fixed effects. Missing values in control variables are imputed to zero and flagged with missing-value indicators. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of eight treatment-related coefficients: the four Treatment coefficients and the four Treatment × Male coefficients.

Table A.8. Intention to Treat Effects on Overdue and Written-Off Debt, by Sex

	Overdue + Written-Off	Overdue	Written-Off
	(1)	(2)	(3)
Panel A: Probability of Holding Debt			
Treatment	0.001	-0.001	0.002
	(0.005)	(0.004)	(0.004)
Male	-0.020***	-0.006	-0.021***
	(0.005)	(0.004)	(0.004)
Treatment × Male	-0.000	0.001	-0.001
	(0.007)	(0.006)	(0.005)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
Mean in Control	0.145	0.100	0.097
Panel B: Debt Balance			
Treatment	0.005	-0.007	0.008
	(0.087)	(0.176)	(0.089)
Male	-0.057	-0.048	-0.061
	(0.076)	(0.160)	(0.078)
Treatment × Male	0.184	0.218	0.169
	(0.153)	(0.319)	(0.164)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
Mean in Control	235.7	73.7	162.1

Notes: See Table 2 notes for outcome definitions. Heterogeneous effects by sex are estimated from pooled regressions that include a treatment indicator, a male indicator, and their interaction. The coefficient on Treatment corresponds to the effect for females (the omitted group), the coefficient on Male captures baseline differences between males and females in the control group, and the coefficient on Treatment × Male captures differential effects for males relative to females. Panel A reports OLS estimates; Panel B reports Poisson estimates expressed as $\exp(\hat{\beta})-1$. All specifications include controls for age, GPA in 2015, socioeconomic status (SES), and matched-pair fixed effects. Missing values in control variables are imputed to zero and flagged with missing-value indicators. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of six treatment-related coefficients (the three Treatment coefficients and the three Treatment × Male coefficients).

Table A.9. Intention to Treat Effects on Formality and Income, by Sex

	Formality	Business	PFA Contributor	Income
	(1)	(2)	(3)	(4)
Treatment	0.003	0.002	0.000	-0.022
	(0.008)	(0.003)	(0.008)	(0.040)
Male	0.076***	0.002	0.077***	0.539***
	(0.008)	(0.002)	(0.008)	(0.063)
Treatment × Male	-0.003	-0.006*	0.001	0.047
	(0.011)	(0.003)	(0.011)	(0.058)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.296	0.043	0.263	307.4

Notes: See notes to Table 3 for outcome definitions. Heterogeneous effects by sex are estimated from pooled regressions that include a treatment indicator, a male indicator, and their interaction. The coefficient on Treatment corresponds to the treatment effect for females, the omitted group; the coefficient on Male captures baseline differences between males and females in the control group; and the coefficient on Treatment × Male captures differential treatment effects for males relative to females. Columns (1)–(3) report OLS estimates. Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include controls for age, GPA in 2015, socioeconomic status (SES), and matched-pair fixed effects. Missing values in control variables are imputed to zero and flagged with missing-value indicators. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied to the family of eight treatment-related coefficients: the four Treatment coefficients and the four Treatment × Male coefficients.

Table A.10. Intention to Treat Effects on Formality and Income, by Academic Performance

	Formality	Business	PFA Contributor	Income
	(1)	(2)	(3)	(4)
Treatment	-0.004	-0.002	-0.003	-0.005
	(0.008)	(0.002)	(0.008)	(0.028)
Top	0.006	0.004	0.002	0.086***
	(0.006)	(0.003)	(0.006)	(0.027)
Treatment × Top	0.011	0.002	0.010	0.025
	(0.008)	(0.003)	(0.008)	(0.035)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	0.294	0.042	0.261	306.5

Notes: See notes to Table 3 for outcome definitions. Heterogeneous effects by academic performance are estimated from pooled regressions that include a treatment indicator, an indicator equal to one if the student's GPA in 2015 is strictly above the within-school median, and their interaction. The omitted group consists of students at or below the within-school median. The coefficient on Treatment corresponds to the treatment effect for students at or below the within-school median GPA; the coefficient on Top captures baseline differences between students above and at or below the median in the control group; and the coefficient on Treatment × Top captures differential treatment effects for students above the median relative to those at or below it. Columns (1)–(3) report OLS estimates. Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include controls for gender, age, socioeconomic status (SES), and matched-pair fixed effects. The sample is restricted to students with observed GPA in 2015 and observed controls. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied to the family of eight treatment-related coefficients: the four Treatment coefficients and the four Treatment × Top coefficients.

Table A.11. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions, by Socioeconomic Level

	Total	MSE	Revolving	Non-Revolving
	(1)	(2)	(3)	(4)
Panel A: Probability of Holding Debt				
Treatment	0.004	0.001	-0.005	0.002
	(0.007)	(0.007)	(0.006)	(0.005)
Poor	-0.095***	-0.082***	-0.012**	-0.034***
	(0.019)	(0.016)	(0.005)	(0.010)
Treatment × Poor	0.022	0.026	0.008	0.008
	(0.020)	(0.019)	(0.008)	(0.012)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	0.379	0.197	0.088	0.197
Panel B: Debt Balance				
Treatment	0.082*	0.072	-0.128*	0.142**
	(0.045)	(0.059)	(0.069)	(0.064)
Poor	-0.083	-0.185**	-0.328**	0.010
	(0.084)	(0.076)	(0.106)	(0.146)
Treatment × Poor	0.044	-0.000	0.238	0.180
	(0.116)	(0.114)	(0.259)	(0.169)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	1287.4	505.7	121.8	468.0
Panel C: Average Loan Size				
Treatment	0.079**	0.060	0.003	0.086*
	(0.040)	(0.060)	(0.117)	(0.052)
Poor	0.013	-0.104	0.076	0.182
	(0.075)	(0.086)	(0.274)	(0.139)
Treatment × Poor	-0.013	-0.023	-0.165	0.009
	(0.088)	(0.123)	(0.231)	(0.125)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	927.9	567.2	108.4	401.9
Panel D: Median Interest Rate				
Treatment	-0.004	-0.009	-0.001	0.007
	(0.004)	(0.006)	(0.006)	(0.005)
Poor	-0.038***	-0.057***	0.000	-0.002
	(0.012)	(0.015)	(0.017)	(0.013)
Treatment × Poor	-0.013	-0.002	-0.004	-0.013
	(0.011)	(0.014)	(0.018)	(0.015)
Number of Observations	14,754	8,051	2,229	6,957
Number of Schools	300	299	245	300
Mean in Control	0.680	0.647	0.839	0.666

Notes: See Table 1 notes for outcome definitions. Heterogeneous effects by socioeconomic level are estimated from pooled regressions that include a treatment indicator, an indicator equal to one if the student's school is in a poor district, and their interaction. The coefficient on Treatment corresponds to the effect for students in non-poor districts, the omitted group; the coefficient on Poor captures baseline differences between students in poor and non-poor districts in the control group; and the coefficient on Treatment × Poor captures differential effects for students in poor districts relative to those in non-poor districts. Panels A and D report OLS estimates. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include controls for gender, age, GPA in 2015, and matched-pair fixed effects. The sample is restricted to students with non-missing district poverty classification and observed controls. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of eight treatment-related coefficients: the four Treatment coefficients and the four Treatment × Poor coefficients.

Table A.12. Intention to Treat Effects on Overdue and Written-Off Debt, by Socioeconomic Level

	Overdue + Written-Off	Overdue	Written-Off
	(1)	(2)	(3)
Panel A: Probability of Overdue/Written-Off Debt			
Treatment	-0.001	-0.003	-0.001
	(0.004)	(0.003)	(0.003)
Poor	-0.073***	-0.046***	-0.056***
	(0.009)	(0.007)	(0.007)
Treatment × Poor	0.008	0.006	0.010
	(0.010)	(0.006)	(0.007)
Number of Observations	52,849	52,849	52,849
Number of Schools	300	300	300
Mean in Control	0.143	0.098	0.096
Panel B: Debt Balance			
Treatment	0.066	0.089	0.054
	(0.072)	(0.123)	(0.078)
Poor	-0.547***	-0.366**	-0.616***
	(0.071)	(0.143)	(0.063)
Treatment × Poor	0.248	0.096	0.340
	(0.219)	(0.309)	(0.241)
Number of Observations	52,849	52,849	52,849
Number of Schools	300	300	300
Mean in Control	228.2	74.1	154.1

Notes: See Table 2 notes for outcome definitions. Heterogeneous effects by socioeconomic level are estimated from pooled regressions that include a treatment indicator, an indicator equal to one if the student's school is in a poor district, and their interaction. The coefficient on Treatment corresponds to the effect for students in non-poor districts, the omitted group; the coefficient on Poor captures baseline differences between students in poor and non-poor districts in the control group; and the coefficient on Treatment × Poor captures differential effects for students in poor districts relative to those in non-poor districts. Panel A reports OLS estimates for binary indicators equal to one if overdue or written-off debt is positive in any month between January and December 2023. Panel B reports Poisson estimates for December 2023 balances, expressed as $\exp(\hat{\beta}) - 1$. All specifications include controls for gender, age, GPA in 2015, and matched-pair fixed effects. The sample is restricted to students with non-missing district poverty classification and observed controls. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, ††† 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of six treatment-related coefficients: the three Treatment coefficients and the three Treatment × Poor coefficients.

Table A.13. Intention to Treat Effects on Formality and Income, by Socioeconomic Level

	Formality	Business	PFA Contributor	Income
	(1)	(2)	(3)	(4)
Treatment	-0.001	-0.002	-0.000	-0.006
	(0.006)	(0.002)	(0.006)	(0.022)
Poor	-0.014	-0.005	-0.012	-0.080
	(0.023)	(0.003)	(0.023)	(0.088)
Treatment × Poor	0.011	0.004	0.010	0.095
	(0.019)	(0.004)	(0.018)	(0.101)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	0.294	0.042	0.261	306.5

Notes: See notes to Table 3 for outcome definitions. Heterogeneous effects by socioeconomic level are estimated from pooled regressions that include a treatment indicator, an indicator equal to one if the student's school is in a poor district, and their interaction. The coefficient on Treatment corresponds to the effect for students in non-poor districts, the omitted group; the coefficient on Poor captures baseline differences between students in poor and non-poor districts in the control group; and the coefficient on Treatment × Poor captures differential effects for students in poor districts relative to those in non-poor districts. Columns (1)–(3) report OLS estimates. Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include controls for gender, age, GPA in 2015, and matched-pair fixed effects. The sample is restricted to students with non-missing district poverty classification and observed controls. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied to the family of eight treatment-related coefficients: the four Treatment coefficients and the four Treatment × Poor coefficients.

Table A.14. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions, by Grade

	Total	MSE	Revolving	Non-Revolving
	(1)	(2)	(3)	(4)
Panel A: Probability of Holding Debt				
Treatment	0.007	0.005	-0.001	0.009
	(0.008)	(0.007)	(0.005)	(0.006)
Grade 10	0.035***	-0.005	0.029***	0.037***
	(0.009)	(0.006)	(0.005)	(0.006)
Grade 11	0.065***	-0.003	0.050***	0.065***
	(0.009)	(0.007)	(0.007)	(0.007)
Treatment × Grade 10	0.004	0.002	-0.006	-0.007
	(0.011)	(0.008)	(0.007)	(0.007)
Treatment × Grade 11	0.001	0.000	0.000	-0.005
	(0.011)	(0.008)	(0.010)	(0.009)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.381	0.196	0.090	0.198
Panel B: Debt Balance				
Treatment	0.056	0.091	0.065	0.051
	(0.069)	(0.078)	(0.125)	(0.120)
Grade 10	0.502***	0.145**	0.766***	0.915***
	(0.088)	(0.079)	(0.185)	(0.202)
Grade 11	0.851***	0.230**	1.493***	1.590***
	(0.150)	(0.107)	(0.334)	(0.385)
Treatment × Grade 10	0.030	-0.053	-0.268**†	0.177
	(0.082)	(0.085)	(0.105)	(0.165)
Treatment × Grade 11	0.022	-0.053	-0.174	0.067
	(0.096)	(0.094)	(0.122)	(0.168)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	1312.7	502.4	127.2	489.3
Panel C: Average Loan Size				
Treatment	0.065	0.035	0.221	0.133
	(0.060)	(0.071)	(0.198)	(0.106)
Grade 10	0.409***	0.229***	0.356**	0.878***
	(0.073)	(0.088)	(0.207)	(0.175)
Grade 11	0.705***	0.438***	1.256***	1.247***
	(0.114)	(0.112)	(0.389)	(0.274)
Treatment × Grade 10	0.021	-0.005	-0.235	-0.032
	(0.075)	(0.088)	(0.165)	(0.124)
Treatment × Grade 11	0.013	0.072	-0.242	-0.094
	(0.083)	(0.111)	(0.142)	(0.121)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	934.3	565.3	113.9	410.8
Panel D: Median Interest Rate				
Treatment	-0.008	-0.012*	-0.006	0.001
	(0.006)	(0.007)	(0.009)	(0.008)
Grade 10	-0.020***	-0.027***	-0.000	-0.027***
	(0.006)	(0.008)	(0.010)	(0.010)
Grade 11	-0.027***	-0.043***	-0.026**	-0.024**
	(0.007)	(0.009)	(0.011)	(0.011)
Treatment × Grade 10	0.002	-0.001	-0.003	0.014
	(0.008)	(0.010)	(0.014)	(0.013)
Treatment × Grade 11	0.005	0.010	0.015	0.001
	(0.008)	(0.010)	(0.011)	(0.012)
Number of Observations	16,059	8,733	2,486	7,588
Number of Schools	300	300	250	300
Mean in Control	0.681	0.648	0.839	0.664

Notes: See Table 1 notes for outcome definitions. Grade 9, Grade 10, and Grade 11 correspond to `grade_admin` equal to Third, Fourth, and Fifth, respectively. Heterogeneous treatment effects are estimated from pooled regressions including Treatment, Grade 10, Grade 11, and their interactions with Treatment. The coefficient on Treatment corresponds to Grade 9 students, the omitted group; the interaction coefficients capture differential effects for Grade 10 and Grade 11 relative to Grade 9. Panels A and D report OLS estimates; Panels B and C report Poisson estimates as $\exp(\hat{\beta}) - 1$. All specifications control for gender, age, GPA in 2015, socioeconomic status (SES), missing-value indicators for the controls, and pair fixed effects. Standard errors are clustered at the school level. Stars denote unadjusted significance levels (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf adjusted significance levels († 10%, †† 5%, † † † 1%). Romano–Wolf correction is applied within each panel to the twelve treatment-related coefficients.

Table A.15. Intention to Treat Effects on Overdue and Written-Off Debt, by Grade

	Pr(Overdue) (1)	Overdue (2)	Pr(Written-Off) (3)	Written-Off (4)
Treatment	-0.004	-0.090	0.005	0.120
	(0.004)	(0.157)	(0.004)	(0.112)
Grade 10	-0.001	0.145	0.001	0.221*
	(0.005)	(0.187)	(0.005)	(0.130)
Grade 11	0.001	0.703**	-0.006	0.356**
	(0.005)	(0.404)	(0.006)	(0.176)
Treatment × Grade 10	0.005	0.418	-0.007	-0.033
	(0.006)	(0.359)	(0.006)	(0.140)
Treatment × Grade 11	0.005	0.203	-0.005	-0.016
	(0.006)	(0.324)	(0.006)	(0.153)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.100	73.7	0.097	162.1

Notes: Columns (1) and (3) use binary indicators equal to one if overdue or written-off debt is positive in any month between January and December 2023; Columns (2) and (4) use balances measured in December 2023 and report $\exp(\hat{\beta})-1$ from Poisson regressions. Grade 9, Grade 10, and Grade 11 correspond to `grade_admin` equal to Third, Fourth, and Fifth, respectively. Heterogeneous treatment effects are estimated from pooled regressions that include a treatment indicator, indicators for Grade 10 and Grade 11, and their interactions with treatment. The coefficient on Treatment corresponds to Grade 9 students, the omitted group; the interaction coefficients capture differential treatment effects for Grade 10 and Grade 11 relative to Grade 9. All specifications additionally control for gender, age, GPA in 2015, socioeconomic status (SES), missing-value indicators for the control variables, and pair fixed effects. Standard errors are clustered at the school level. Stars denote unadjusted significance levels (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf adjusted significance levels († 10%, †† 5%, † † † 1%). Romano–Wolf correction is applied to the twelve treatment-related coefficients in this table.

Table A.16. Intention to Treat Effects on Formality and Income, by Grade

	Formality (1)	Business (2)	PFA Contributor (3)	Income (4)
Treatment	0.006·	0.000·	0.006·	0.031·
	(0.007)	(0.002)	(0.008)	(0.036)
Grade 10	0.034***	0.013***	0.027***	0.183***
	(0.007)	(0.003)	(0.006)	(0.033)
Grade 11	0.080***	0.024***	0.064***	0.465***
	(0.008)	(0.004)	(0.008)	(0.060)
Treatment × Grade 10	-0.005·	-0.003·	-0.004·	-0.015·
	(0.008)	(0.004)	(0.008)	(0.035)
Treatment × Grade 11	-0.012·	-0.001·	-0.011·	-0.052·
	(0.010)	(0.004)	(0.010)	(0.046)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.296	0.043	0.263	307.4

Notes: See Table 3 notes for definitions of dependent variables. Grade 9, Grade 10, and Grade 11 correspond to `grade_admin` equal to Third, Fourth, and Fifth, respectively. Heterogeneous treatment effects are estimated from pooled regressions that include a treatment indicator, indicators for Grade 10 and Grade 11, and their interactions with treatment. The coefficient on Treatment corresponds to Grade 9 students, the omitted group; the interaction coefficients capture differential treatment effects for Grade 10 and Grade 11 relative to Grade 9. Columns (1)–(3) are estimated using OLS, while Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications additionally control for gender, age, GPA in 2015, socioeconomic status (SES), missing-value indicators for the control variables, and pair fixed effects. Standard errors are clustered at the school level. Stars denote unadjusted significance levels (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf adjusted significance levels († 10%, †† 5%, † † † 1%). Romano–Wolf correction is applied to the twelve treatment-related coefficients in this table.

Table A.17. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions, by Short-Term Gains in Financial Literacy

	Total (1)	MSE (2)	Revolving (3)	Non-Revolving (4)
Panel A: Probability of Holding Debt				
Treatment	0.011	0.013	−0.002	−0.001
	(0.012)	(0.009)	(0.007)	(0.009)
Below-median residual	−0.027**	−0.004	−0.015**	−0.021**
	(0.012)	(0.009)	(0.006)	(0.010)
Treatment × Below-median residual	0.013	0.001	0.006	0.004
	(0.016)	(0.013)	(0.010)	(0.013)
Number of Observations	14,190	14,190	14,190	14,190
Number of Schools	297	297	297	297
P-value equal effects	0.423	0.956	0.545	0.731
Mean in Control	0.372	0.200	0.075	0.196
Panel B: Debt Balance				
Treatment	0.062	0.274***††	0.388**	−0.034
	(0.097)	(0.113)	(0.212)	(0.159)
Below-median residual	−0.024	0.084	−0.014	−0.160
	(0.104)	(0.104)	(0.153)	(0.146)
Treatment × Below-median residual	−0.092	−0.185	−0.280	−0.126
	(0.133)	(0.118)	(0.181)	(0.227)
Number of Observations	14,190	14,190	14,190	14,190
Number of Schools	297	297	297	297
P-value equal effects	0.513	0.158	0.191	0.604
Mean in Control	1264.4	509.8	85.8	486.6
Panel C: Average Loan Size				
Treatment	0.084	0.088	−0.020	0.066
	(0.082)	(0.095)	(0.171)	(0.143)
Below-median residual	−0.077	−0.052	−0.414***	−0.001
	(0.080)	(0.089)	(0.105)	(0.146)
Treatment × Below-median residual	−0.062	0.034	0.467	−0.211
	(0.117)	(0.154)	(0.432)	(0.174)
Number of Observations	14,190	14,190	14,190	14,190
Number of Schools	297	297	297	297
P-value equal effects	0.611	0.822	0.194	0.283
Mean in Control	899.6	566.3	80.6	387.0
Panel D: Median Interest Rate				
Treatment	−0.000	−0.011	−0.004	0.037**††
	(0.008)	(0.012)	(0.017)	(0.015)
Below-median residual	−0.005	−0.002	0.009	−0.006
	(0.008)	(0.011)	(0.020)	(0.016)
Treatment × Below-median residual	0.007	0.017	0.014	−0.020
	(0.012)	(0.016)	(0.026)	(0.021)
Number of Observations	3,921	2,242	488	1,823
Number of Schools	296	283	185	291
P-value equal effects	0.535	0.273	0.594	0.343
Mean in Control	0.665	0.641	0.842	0.652

Notes: See notes to Table 1 for outcome definitions. The table reproduces Table 1, allowing treatment effects to vary by whether residualized short-term financial literacy gains are below or above the median. Below-median residual equals one for individuals below the median; the above-median group is omitted. The coefficient on Treatment is the effect for the above-median group, and Treatment × Below-median residual is the differential effect for the below-median group. P-value equal effects reports the test of equal treatment effects across groups. Panels A and D report OLS estimates. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include controls for age, gender, GPA in 2015, SES, and matched-pair fixed effects. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) based on 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the eight treatment-related coefficients.

Table A.18. Dynamic Effects on the Probability of Holding Financial Products

	2017	2018	2019	2020	2021	2022	2023
Total	0.001	0.001	0.003	0.002	0.010**	0.005	0.008
	[0.001]	[0.002]	[0.004]	[0.004]	[0.005]	[0.006]	[0.006]
Observations	57,435	57,435	57,435	57,435	57,435	57,435	57,435
MSE	0.001	0.001	0.007**	0.006*	0.010**	0.008	0.006
	[0.000]	[0.001]	[0.003]	[0.004]	[0.004]	[0.005]	[0.005]
Observations	57,435	57,435	57,435	57,435	57,435	57,435	57,435
Revolving	0.000	0.000	-0.001	-0.002***	-0.005***†	-0.007**	-0.003
	[0.000]	[0.000]	[0.001]	[0.001]	[0.002]	[0.003]	[0.004]
Observations	57,435	57,435	57,435	57,435	57,435	57,435	57,435
Non-Revolving	0.000	0.000	-0.001	-0.000	0.002	0.004	0.005
	[0.000]	[0.001]	[0.002]	[0.003]	[0.003]	[0.004]	[0.004]
Observations	57,435	57,435	57,435	57,435	57,435	57,435	57,435

Notes: This table reports the numerical estimates underlying Figure 1. The table reports the estimated effects of the financial education program on the probability of holding each type of debt between 2017 and 2023. Each coefficient comes from a separate OLS regression. All specifications include randomization-pair fixed effects and controls for gender, age, 2015 GPA, and socioeconomic status. Missing values in the controls are replaced with zero, and corresponding missing-value indicators are included. Standard errors, clustered at the school level, are reported in brackets. Romano–Wolf adjusted p-values are computed jointly across the four outcomes within each year. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.10$ based on unadjusted p-values. † indicates significance at the 10 percent level or better based on Romano–Wolf adjusted p-values.

Table A.19. Dynamic Effects on Debt Balances

	2017	2018	2019	2020	2021	2022	2023
Total	-0.175	-0.021	0.023	-0.002	0.053	0.059	0.076**
	[0.174]	[0.086]	[0.070]	[0.056]	[0.051]	[0.044]	[0.038]
Observations	57,435	57,435	57,435	57,435	57,435	57,435	57,435
MSE	0.107	0.109	0.063	0.132*	0.168**	0.058	0.050
	[0.187]	[0.109]	[0.087]	[0.077]	[0.073]	[0.057]	[0.043]
Observations	57,435	57,435	57,435	57,435	57,435	57,435	57,435
Revolving	.	.	-0.086	-0.222*	-0.277***†	-0.093	-0.122*
	[.]	[.]	[0.119]	[0.107]	[0.067]	[0.079]	[0.066]
Observations	.	.	57,435	57,435	57,435	57,435	57,435
Non-Revolving	-0.422	-0.126	-0.000	-0.056	0.012	0.070	0.145***†
	[0.220]	[0.105]	[0.091]	[0.073]	[0.060]	[0.057]	[0.054]
Observations	57,435	57,435	57,435	57,435	57,435	57,435	57,435

Notes: This table reports the numerical estimates underlying Figure 2. It presents the estimated effects of the financial education program on debt balances measured in December of each year from 2017 to 2023. The outcomes are total formal debt, loans to micro and small enterprises (MSE), revolving consumption credit, and non-revolving consumption credit. Each coefficient comes from a separate Poisson regression and is reported as $\exp(\widehat{\beta}) - 1$. All specifications include randomization-pair fixed effects and controls for gender, age, 2015 GPA, and socioeconomic status. Missing values in the controls are replaced with zero, and corresponding missing-value indicators are included. Standard errors, clustered at the school level, are reported in brackets. Romano–Wolf adjusted p-values are computed jointly across the available outcomes within each year. Dots indicate that the corresponding model did not converge. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.10$ based on unadjusted p-values. † indicates significance at the 10 percent level or better based on Romano–Wolf adjusted p-values.

Table A.20. Dynamic Effects on Overdue and Written-Off Debt Balances

	2017	2018	2019	2020	2021	2022	2023
Overdue + Written-Off	.	0.304	0.045	-0.006	-0.046	0.083	0.103*
	[.]	[0.291]	[0.116]	[0.080]	[0.055]	[0.063]	[0.061]
Observations	.	57,435	57,435	57,435	57,435	57,435	57,435

Notes: This table reports the numerical estimates underlying Panel (b) of Figure 3. It presents the estimated effects of the financial education program on the sum of overdue and written-off debt balances measured in December of each year from 2017 to 2023. Each coefficient comes from a separate Poisson regression and is reported as $\exp(\widehat{\beta}) - 1$. All specifications include randomization-pair fixed effects and controls for gender, age, 2015 GPA, and socioeconomic status. Missing values in the controls are replaced with zero, and corresponding missing-value indicators are included. Standard errors, clustered at the school level, are reported in brackets. Dots indicate that the corresponding estimate could not be obtained. $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.10$.

B Sensitivity Analysis: Main Results without Covariates

Table B.1. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions

	(1) Total	(2) MSE	(3) Revolving	(4) Non-Revolving
Panel A: Probability of Holding Debt				
Treatment	0.007	0.003	-0.003	0.005
	(0.006)	(0.006)	(0.004)	(0.004)
Number of Observations	57,435	57,435	57,435	57,435
Number of schools	300	300	300	300
Mean in Control	0.381	0.196	0.090	0.198
Panel B: Debt Balance				
Treatment	0.083**	0.051	-0.121*	0.174***††
	(0.037)	(0.044)	(0.066)	(0.055)
Number of Observations	57,435	57,435	57,435	57,435
Number of schools	300	300	300	300
Mean in Control	1312.7	502.4	127.2	489.3
Panel C: Average Loan Size				
Treatment	0.086***	0.060	-0.008	0.096**
	(0.034)	(0.046)	(0.110)	(0.047)
Number of Observations	57,435	57,435	57,435	57,435
Number of schools	300	300	300	300
Mean in Control	934.3	565.3	113.9	410.8
Panel D: Median Interest Rate				
Treatment	-0.008*	-0.014**	-0.005	0.006
	(0.004)	(0.006)	(0.006)	(0.004)
Number of Observations	16,059	8,733	2,486	7,588
Number of schools	300	300	250	300
Mean in Control	0.681	0.648	0.839	0.664

Notes: See Table 1 for outcome definitions. Panel A reports indicators equal to one if the individual holds positive debt in a given category at any point between January and December 2023. Panel B reports outstanding debt balances measured in December 2023. Panel C reports average loan size, computed as the annual mean of monthly balances over 2023. Panel D reports annual median interest rates computed from monthly records over the same period. Columns correspond to total debt, micro and small enterprise (MSE) debt, revolving debt, and non-revolving debt. Panels A and D are estimated using OLS. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the four treatment coefficients.

Table B.2. Intention to Treat Effects on Overdue and Written-off Debt

	(1) Pr(Overdue)	(2) Overdue	(3) Pr(Written-Off)	(4) Written-Off
Treatment	-0.001	0.106	0.000	0.083
	(0.003)	(0.098)	(0.003)	(0.074)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.100	73.7	0.097	162.1

Notes: Columns (1) and (3) report indicators equal to one if the individual has positive overdue or written-off debt, respectively, at any point between January and December 2023. Columns (2) and (4) report outstanding balances in overdue and written-off debt measured in December 2023. Columns (1) and (3) are estimated using OLS. Columns (2) and (4) report Poisson estimates expressed as $\exp(\hat{\beta})-1$. All specifications include matched-pair fixed effects. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied to the family of four treatment coefficients reported in this table.

Table B.3. Intention to Treat Effects on Formality and Income

	(1) Formality	(2) Business	(3) PFA Contributor	(4) Income
Treatment	0.003	-0.001	0.003	0.019
	(0.006)	(0.002)	(0.006)	(0.022)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.296	0.043	0.263	307.4

Notes: See Table 3 for outcome definitions. Column (1) reports an indicator equal to one if the individual either contributed to the PFA or was recorded as a business owner by SUNAT at any point during 2023. Column (2) reports an indicator for business ownership recorded by SUNAT in 2023. Column (3) reports an indicator equal to one if the individual contributed to the PFA in at least one month during 2023. Column (4) reports the annual average of monthly income contributions to the PFA over 2023. Columns (1)–(3) are estimated using OLS, while Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta})-1$. All specifications include matched-pair fixed effects. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied to the family of four treatment coefficients reported in this table.

Table B.4. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions, by Sex

	Total (1)	MSE (2)	Revolving (3)	Non-Revolving (4)
Panel A: Probability of Holding Debt				
Treatment	0.013	0.008	-0.003	0.005
	(0.008)	(0.008)	(0.005)	(0.005)
Male	0.010	-0.062***	0.020***	0.050***
	(0.008)	(0.007)	(0.004)	(0.007)
Treatment × Male	-0.012	-0.006	-0.002	-0.002
	(0.011)	(0.009)	(0.006)	(0.009)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.381	0.196	0.090	0.198
Panel B: Debt Balance				
Treatment	0.049	0.037	-0.205**	0.137
	(0.054)	(0.058)	(0.090)	(0.105)
Male	0.402***	-0.134**	0.200*	1.669***
	(0.075)	(0.053)	(0.117)	(0.245)
Treatment × Male	0.040	0.033	0.164	0.012
	(0.075)	(0.085)	(0.160)	(0.126)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	1312.7	502.4	127.2	489.3
Panel C: Average Loan Size				
Treatment	0.078	0.058	-0.028	0.076
	(0.051)	(0.062)	(0.137)	(0.086)
Male	0.278***	-0.084	0.203	0.946***
	(0.064)	(0.060)	(0.139)	(0.154)
Treatment × Male	0.001	0.009	0.019	0.005
	(0.072)	(0.090)	(0.159)	(0.116)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	934.3	565.3	113.9	410.8
Panel D: Median Interest Rate				
Treatment	-0.010*	-0.016**	-0.000	0.004
	(0.006)	(0.008)	(0.008)	(0.007)
Male	-0.059***	-0.094***	-0.019**	-0.028***
	(0.006)	(0.008)	(0.009)	(0.008)
Treatment × Male	0.008	0.011	-0.005	0.005
	(0.008)	(0.012)	(0.013)	(0.011)
Number of Observations	16,059	8,733	2,486	7,588
Number of Schools	300	300	250	300
Mean in Control	0.681	0.648	0.839	0.664

Notes: See Table 1 for outcome definitions. Panel A reports indicators equal to one if the individual holds positive debt in a given category at any point between January and December 2023. Panel B reports outstanding debt balances measured in December 2023. Panel C reports average loan size, computed as the annual mean of monthly balances over 2023. Panel D reports annual median interest rates computed from monthly records over the same period. Columns correspond to total debt, micro and small enterprise (MSE) debt, revolving debt, and non-revolving debt. Heterogeneous effects by sex are estimated from regressions including Treatment, Male, and their interaction. Treatment corresponds to females (omitted group), and Treatment × Male captures differential effects for males. Panels A and D are estimated using OLS. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of eight treatment-related coefficients.

Table B.5. Intention to Treat Effects on Overdue and Written-Off Debt, by Sex

	Overdue + Written-Off (1)	Overdue (2)	Written-Off (3)
Panel A: Probability of Holding Debt			
Treatment	0.000	-0.002	0.001
	(0.006)	(0.004)	(0.005)
Male	0.000	0.006	-0.005
	(0.004)	(0.004)	(0.003)
Treatment × Male	-0.001	0.001	-0.001
	(0.007)	(0.006)	(0.006)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
Mean in Control	0.145	0.100	0.097
Panel B: Debt Balance			
Treatment	-0.014	-0.014	-0.014
	(0.088)	(0.174)	(0.090)
Male	0.091	0.084	0.094
	(0.089)	(0.186)	(0.088)
Treatment × Male	0.191	0.222	0.177
	(0.159)	(0.322)	(0.171)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
Mean in Control	235.7	73.7	162.1

Notes: See Table 2 for outcome definitions. Panel A reports indicators equal to one if overdue or written-off debt is positive in any month between January and December 2023. Panel B reports outstanding balances measured in December 2023. Heterogeneous effects by sex are estimated from regressions including Treatment, Male, and their interaction. Treatment corresponds to females (omitted group), and Treatment × Male captures differential effects for males. Panel A is estimated using OLS, while Panel B reports Poisson estimates expressed as $\exp(\hat{\beta})-1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of six treatment-related coefficients.

Table B.6. Intention to Treat Effects on Formality and Income, by Sex

	Formality (1)	Business (2)	PFA Contributor (3)	Income (4)
Treatment	0.003	0.002	0.001	-0.019
	(0.008)	(0.003)	(0.008)	(0.041)
Male	0.077^{***}	0.002	0.078^{***}	0.526^{***}
	(0.008)	(0.002)	(0.008)	(0.061)
Treatment × Male	-0.004	-0.006^{*}	0.000	0.040
	(0.011)	(0.003)	(0.011)	(0.058)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.296	0.043	0.263	307.4

Notes: See Table 3 for outcome definitions. Column (1) reports an indicator equal to one if the individual either contributed to the PFA or was recorded as a business owner by SUNAT at any point during 2023. Column (2) reports an indicator for business ownership recorded by SUNAT in 2023. Column (3) reports an indicator equal to one if the individual contributed to the PFA in at least one month during 2023. Column (4) reports the annual average of monthly income contributions to the PFA over 2023. Heterogeneous effects by sex are estimated from regressions including Treatment, Male, and their interaction. Treatment corresponds to females (omitted group), and Treatment × Male captures differential effects for males. Columns (1)–(3) are estimated using OLS, while Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied to the family of eight treatment-related coefficients.

Table B.7. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions, by Academic Performance

	Total (1)	MSE (2)	Revolving (3)	Non-Revolving (4)
Panel A: Probability of Holding Debt				
Treatment	-0.008	-0.004	-0.005	-0.004
	(0.009)	(0.008)	(0.005)	(0.006)
Top	-0.063***	-0.047***	0.010***	-0.038***
	(0.008)	(0.006)	(0.004)	(0.006)
Treatment × Top	0.030***††	0.016*	0.004	0.017**
	(0.011)	(0.009)	(0.005)	(0.008)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	0.379	0.197	0.088	0.197
Panel B: Debt Balance				
Treatment	0.019	0.015	-0.072	0.036
	(0.052)	(0.057)	(0.103)	(0.080)
Top	-0.118**	-0.212***	0.456***	-0.167*
	(0.050)	(0.043)	(0.148)	(0.086)
Treatment × Top	0.178**	0.135	-0.080	0.411**†
	(0.094)	(0.095)	(0.120)	(0.194)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	1287.4	505.7	121.8	468.0
Panel C: Average Loan Size				
Treatment	0.020	0.042	-0.013	0.009
	(0.045)	(0.062)	(0.165)	(0.071)
Top	-0.106**	-0.144***	0.507***	-0.111
	(0.042)	(0.049)	(0.231)	(0.079)
Treatment × Top	0.136*	0.020	0.021	0.227*
	(0.078)	(0.088)	(0.185)	(0.150)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	927.9	567.2	108.4	401.9
Panel D: Median Interest Rate				
Treatment	-0.008	-0.016**	0.001	0.007
	(0.005)	(0.008)	(0.009)	(0.007)
Top	-0.007	-0.015**	-0.005	-0.012
	(0.005)	(0.006)	(0.010)	(0.008)
Treatment × Top	-0.001	0.005	-0.009	-0.006
	(0.007)	(0.009)	(0.013)	(0.011)
Number of Observations	14,754	8,051	2,229	6,957
Number of Schools	300	299	245	300
Mean in Control	0.680	0.647	0.839	0.666

Notes: See Table 1 for outcome definitions. Panel A reports indicators equal to one if the individual holds positive debt in a given category at any point between January and December 2023. Panel B reports outstanding debt balances measured in December 2023. Panel C reports average loan size, computed as the annual mean of monthly balances over 2023. Panel D reports annual median interest rates computed from monthly records over the same period. Columns correspond to total debt, micro and small enterprise (MSE) debt, revolving debt, and non-revolving debt. Heterogeneous effects by academic performance are estimated from regressions including Treatment, Top (above the within-school median GPA in 2015), and their interaction. Treatment corresponds to students at or below the median (omitted group), and Treatment × Top captures differential effects for higher-performing students. Panels A and D are estimated using OLS. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of eight treatment-related coefficients.

Table B.8. Intention to Treat Effects on Overdue and Written-Off Debt, by Academic Performance

	Overdue + Written-Off	Overdue	Written-Off
	(1)	(2)	(3)
Panel A: Probability of Holding Debt			
Treatment	0.003	-0.003	0.003
	(0.006)	(0.004)	(0.005)
Top	-0.065***	-0.044***	-0.049***
	(0.005)	(0.004)	(0.005)
Treatment × Top	-0.007	0.003	-0.005
	(0.007)	(0.005)	(0.006)
Number of Observations	52,849	52,849	52,849
Number of Schools	300	300	300
Mean in Control	0.143	0.098	0.096
Panel B: Debt Balance			
Treatment	0.071	0.147	0.037
	(0.081)	(0.148)	(0.085)
Top	-0.430***	-0.366***	-0.459***
	(0.045)	(0.100)	(0.055)
Treatment × Top	0.086	-0.102	0.189
	(0.153)	(0.235)	(0.187)
Number of Observations	52,849	52,849	52,849
Number of Schools	300	300	300
Mean in Control	228.2	74.1	154.1

Notes: See Table 2 for outcome definitions. Panel A reports indicators equal to one if overdue or written-off debt is positive in any month between January and December 2023. Panel B reports outstanding balances measured in December 2023. Heterogeneous effects by academic performance are estimated from regressions including Treatment, Top (above the within-school median GPA in 2015), and their interaction. Treatment corresponds to students at or below the median (omitted group), and Treatment × Top captures differential effects for higher-performing students. Panel A is estimated using OLS, while Panel B reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of six treatment-related coefficients.

Table B.9. Intention to Treat Effects on Formality and Income, by Academic Performance

	Formality	Business	PFA Contributor	Income
	(1)	(2)	(3)	(4)
Treatment	-0.002	-0.002	-0.001	0.009
	(0.008)	(0.002)	(0.008)	(0.029)
Top	-0.011*	0.003	-0.015**	-0.019
	(0.006)	(0.003)	(0.006)	(0.027)
Treatment $\times$ Top	0.010	0.002	0.010	0.022
	(0.008)	(0.003)	(0.008)	(0.037)
Number of Observations	52,849	52,849	52,849	52,849
Number of Schools	300	300	300	300
Mean in Control	0.294	0.042	0.261	306.5

Notes: See Table 3 for outcome definitions. Column (1) reports an indicator equal to one if the individual either contributed to the PFA or was recorded as a business owner by SUNAT at any point during 2023. Column (2) reports an indicator for business ownership recorded by SUNAT in 2023. Column (3) reports an indicator equal to one if the individual contributed to the PFA in at least one month during 2023. Column (4) reports the annual average of monthly income contributions to the PFA over 2023. Heterogeneous effects by academic performance are estimated from regressions including Treatment, Top (above the within-school median GPA in 2015), and their interaction. Treatment corresponds to students at or below the median (omitted group), and Treatment $\times$ Top captures differential effects for higher-performing students. Columns (1)–(3) are estimated using OLS, while Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied to the family of eight treatment-related coefficients.

Table B.10. Intention to Treat Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions, by Socioeconomic Level

	Total	MSE	Revolving	Non-Revolving
	(1)	(2)	(3)	(4)
Panel A: Probability of Holding Debt				
Treatment	0.005	-0.000	-0.005	0.005
	(0.007)	(0.007)	(0.006)	(0.005)
Poor	-0.087***	-0.076***	-0.011**	-0.030***
	(0.019)	(0.016)	(0.005)	(0.010)
Treatment × Poor	0.021	0.026	0.007	0.004
	(0.020)	(0.019)	(0.008)	(0.012)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.381	0.196	0.090	0.198
Panel B: Debt Balance				
Treatment	0.074*	0.050	-0.122*	0.139**
	(0.043)	(0.057)	(0.069)	(0.060)
Poor	-0.033	-0.171**	-0.116	0.039
	(0.087)	(0.079)	(0.190)	(0.144)
Treatment × Poor	0.042	0.026	0.038	0.153
	(0.111)	(0.115)	(0.244)	(0.160)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	1312.7	502.4	127.2	489.3
Panel C: Average Loan Size				
Treatment	0.090**	0.077	0.003	0.087*
	(0.042)	(0.061)	(0.117)	(0.053)
Poor	0.073	-0.048	0.298	0.250*
	(0.080)	(0.092)	(0.343)	(0.143)
Treatment × Poor	-0.028	-0.049	-0.216	0.009
	(0.086)	(0.118)	(0.213)	(0.123)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	934.3	565.3	113.9	410.8
Panel D: Median Interest Rate				
Treatment	-0.005	-0.012	-0.005	0.008
	(0.005)	(0.007)	(0.006)	(0.005)
Poor	-0.042***	-0.062***	-0.007	-0.006
	(0.012)	(0.015)	(0.016)	(0.013)
Treatment × Poor	-0.010	0.001	0.009	-0.009
	(0.012)	(0.016)	(0.017)	(0.015)
Number of Observations	16,059	8,733	2,486	7,588
Number of Schools	300	300	250	300
Mean in Control	0.681	0.648	0.839	0.664

Notes: See Table 1 for outcome definitions. Panel A reports indicators equal to one if the individual holds positive debt in a given category at any point between January and December 2023. Panel B reports outstanding debt balances measured in December 2023. Panel C reports average loan size, computed as the annual mean of monthly balances over 2023. Panel D reports annual median interest rates computed from monthly records over the same period. Columns correspond to total debt, micro and small enterprise (MSE) debt, revolving debt, and non-revolving debt. Heterogeneous effects by socioeconomic level are estimated from regressions including Treatment, Poor (district poverty indicator), and their interaction. Treatment corresponds to students in non-poor districts (omitted group), and Treatment × Poor captures differential effects for students in poor districts. Panels A and D are estimated using OLS. Panels B and C report Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of eight treatment-related coefficients.

Table B.11. Intention to Treat Effects on Overdue and Written-Off Debt, by Socioeconomic Level

	Overdue + Written-Off	Overdue	Written-Off
	(1)	(2)	(3)
Panel A: Probability of Overdue/Written-Off Debt			
Treatment	0.001	-0.001	-0.000
	(0.005)	(0.003)	(0.004)
Poor	-0.068***	-0.044***	-0.052***
	(0.010)	(0.007)	(0.007)
Treatment × Poor	0.008	0.005	0.010
	(0.010)	(0.007)	(0.008)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
Mean in Control	0.145	0.100	0.097
Panel B: Debt Balance			
Treatment	0.058	0.083	0.045
	(0.070)	(0.113)	(0.078)
Poor	-0.516***	-0.276	-0.600***
	(0.079)	(0.168)	(0.067)
Treatment × Poor	0.303	0.174	0.363*
	(0.222)	(0.321)	(0.240)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
Mean in Control	235.7	73.7	162.1

Notes: See Table 2 for outcome definitions. Panel A reports indicators equal to one if overdue or written-off debt is positive in any month between January and December 2023. Panel B reports outstanding balances measured in December 2023. Heterogeneous effects by socioeconomic level are estimated from regressions including Treatment, Poor (district poverty indicator), and their interaction. Treatment corresponds to students in non-poor districts (omitted group), and Treatment × Poor captures differential effects for students in poor districts. Observations with missing or non-classifiable socioeconomic status are excluded. Panel A is estimated using OLS, while Panel B reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied within each panel to the family of six treatment-related coefficients.

Table B.12. Intention to Treat Effects on Formality and Income, by Socioeconomic Level

	Formality	Business	PFA Contributor	Income
	(1)	(2)	(3)	(4)
Treatment	0.001	-0.002	0.001	0.007
	(0.006)	(0.002)	(0.006)	(0.021)
Poor	-0.012	-0.005	-0.010	-0.066
	(0.023)	(0.003)	(0.024)	(0.089)
Treatment × Poor	0.012	0.004	0.010	0.084
	(0.019)	(0.004)	(0.018)	(0.098)
Number of Observations	57,435	57,435	57,435	57,435
Number of Schools	300	300	300	300
Mean in Control	0.296	0.043	0.263	307.4

Notes: See Table 3 for outcome definitions. Column (1) reports an indicator equal to one if the individual either contributed to the PFA or was recorded as a business owner by SUNAT at any point during 2023. Column (2) reports an indicator for business ownership recorded by SUNAT in 2023. Column (3) reports an indicator equal to one if the individual contributed to the PFA in at least one month during 2023. Column (4) reports the annual average of monthly income contributions to the PFA over 2023. Heterogeneous effects by socioeconomic level are estimated from regressions including Treatment, Poor (district poverty indicator), and their interaction. Treatment corresponds to students in non-poor districts (omitted group), and Treatment × Poor captures differential effects for students in poor districts. Columns (1)–(3) are estimated using OLS, while Column (4) reports Poisson estimates expressed as $\exp(\hat{\beta}) - 1$. All specifications include matched-pair fixed effects only. Standard errors clustered at the school level are reported in parentheses. Stars denote significance based on unadjusted p-values (* 10%, ** 5%, *** 1%). Daggers denote Romano–Wolf stepdown adjusted p-values († 10%, †† 5%, † † † 1%) using 1,000 bootstrap replications. Multiple-testing correction is applied to the family of eight treatment-related coefficients.