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

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Abstract

This study experimentally examines the long-term effects of school-based financial education, analyzing data from nearly 60,000 individuals in Peru, seven years post-intervention. Treated students increased their total debt by 7.2% and average loan size by 7.8%, shifting from revolving to non-revolving credit. Borrowing terms improved slightly, and repayment performance remained unaffected despite increased borrowing. Formal employment and business formation remained unchanged. Impacts were equitable across sex and socioeconomic status, but higher performing students gained more in credit access. During the COVID-19 pandemic, financial education enhanced resilience by reducing reliance on revolving credit in favor of productive loans.

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

JEL Codes: C93, D14, G53, O16

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1 Introduction

Financial education is increasingly recognized as a critical tool for promoting individual financial well-being and broader economic development, and for contributing to macroeconomic financial stability. By equipping individuals with the knowledge and skills necessary to make informed financial decisions, financial education programs have the potential to foster responsible financial behaviors, improve economic stability, and reduce poverty. Financial education is also capable of influencing individuals' financial resilience and adaptability, fostering their ability to navigate crises.

A growing body of literature is examining 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. [2024], 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 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

[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.

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 frequency and coverage of the data enables us to look at the dynamic effects of the program, placing particular emphasis on a period during which the financial system faced a broad economic crisis: the COVID-19 pandemic.

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 prompted 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 reveal that while the intervention did not affect the likelihood of holding debt, it significantly

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

shaped the composition and size of individuals' debt portfolios. Specifically, we document a 7.2% increase in total debt, driven by a strategic shift toward productive loans for micro and small enterprises, accompanied by a shift from revolving consumption loans to non-revolving consumption credit. 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. The personal finance lessons led to slightly more favorable terms in loan agreements, but this effect is concentrated in productive loans: median interest rates linked to micro and small enterprises declined by 1.7%.

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 – an intriguing 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.

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. While the program had no significant impact on the probability of holding debt over time, our results reveal a notable shift in borrowing patterns. During the pandemic years, treated individuals strategically opted for productive loans supporting micro and small enterprises instead of revolving consumption loans, increasing their reliance on these more sustainable forms of credit. This finding is consistent with Lee et al. [2025], 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. Furthermore, our repayment results point to a protective effect in 2021, with a significant reduction in overdue debt balances as financial stress peaked. This reduction was potentially induced by a shift away from less favorable sources of credit such as revolving lines.

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. [2024]. 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 treatment 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, 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 Context and Data

2.1 The Experiment

In 2007, the Superintendency of Banking, Insurance and Pension Fund Administrators (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.[3] 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".[4] 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.[5] While the content of the lessons was not officially 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.[6] The sample of eligible schools was strat-

[3] 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.

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

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

[6] 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.

ified by region, and schools were paired by their similarity within each of the six strata.[7] 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.

Table A.1 presents basic descriptive statistics, as well as balancing tests of the randomization. The average age in our sample is 22.6 years, with a standard deviation of 1.095. Very few significant differences are detected across treatment and control groups, which is consistent with the random treatment assignment.

2.2 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 results, all data sources 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

[7] 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.

credit market, including private and public banks, microfinance institutions (such as rural and municipal savings and loan institutions), and financial companies or *Financieras*.[8] 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. [2024] 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).

(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 to us 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.

We also obtained access to additional data sources useful for measuring the treatment impacts on formality:

(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.

[8] 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.

2.3 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. They also linked selected data from the private pension system and SUNAT records. This process 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.[9]

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 S/ 3,592.8 (US$ 962); 23% held a revolving consumption loan, with an average balance of S/ 2,036.9 (US$ 546); and 40% had a non-revolving consumption loan, with an average balance of S/ 4,494.9 (US$ 1,204).[10]

3 Empirical Analysis

3.1 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 with which the outstanding balance fluctuates based on the borrower's decisions, as seen in credit cards. 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.

[9] 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.

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

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 (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.[11] Examining the impact on both debt balance and loan size 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 interest rates attached to loans obtained in 2023. Looking at the impact on interest rates provides insight into how financial education affects not only the accessibility of credit but also the economic burdens placed on individuals over time. Once more, we study the impacts on interest rates 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. In addition, we construct the balances of overdue and written-off debt as of December 2023.

Finally, we measure formality status. 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 adopting formal systems. Finally, we measure

[11] A Poisson regression is the best alternative for analyzing continuous variables such as income, debt balances, or loan sizes. Poisson regression is particularly advantageous because it is tailored to modeling count data, which enables us to directly interpret the coefficients in terms of incidence rate ratios. This is crucial when we examine debt balances, which can be viewed as counts of financial obligations. While alternative transformations, such as the inverse hyperbolic sine or logarithmic transformations, may help to achieve linearity and address distributional concerns, they can sometimes obscure the meaningful interpretation of results, particularly when the data include zero values or low counts. Poisson regression retains the original scale of the data, facilitating a more intuitive understanding of how various factors influence the number of debts held by individuals. Furthermore, it allows for the direct estimation of effects without the complications that arise from transforming data. See Wooldridge [2010].

average monthly earnings in 2023, as reported by the pension administrator. The regression for formal income is also fitted using a Poisson model.

3.2 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:

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

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.[12] 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.

4 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

[12] Implementation of an analysis of covariance (ANCOVA) to estimate the treatment effects leads to large improvements in power compared to a difference-in-differences specification [McKenzie, 2012].

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.

4.1 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 Table A.2 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. In general, both the size and the composition of the borrowing portfolios seem to be shaped by the financial education lessons received seven years before. Total debt increases by 7.2%, as shown in column (1). This change is driven by a tendency to increase productive debt directed to micro and small enterprises (see column (2)) and a recomposition of consumption debt balances, with individuals opting for non-revolving instead of revolving loans (see columns (3) and (4)). Most of the growth in debt balances is driven by loans from traditional banks, as shown in Table A.2 in the Appendix. These effects are confirmed in panel C in Table 1, which focuses on the impacts on loan size. The treatment leads to a 7.8% increase in average loan size (see column (1)). This growth in loan size is driven by larger productive loans and larger consumption non-revolving loans.

The shift toward more productive and non-revolving debt, coupled with a reduction in revolving debt, represents a positive development for individuals' financial stability and long-term economic health. Productive loans empower individuals by providing the necessary capital to invest in income-generating activities, thereby fostering entrepreneurial growth and enhancing financial resilience. Meanwhile, the decrease in revolving debt, which is often associated with high interest rates and the potential for cyclical borrowing patterns, indicates a move away from dependence on costly consumption credit options. 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. [2024], 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.

The improvements in terms of credit portfolio can lead to better financial terms when borrowing. 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 that personal finance lessons lead to slightly more favorable terms in loan agreements, but this effect is concentrated in productive loans. Median interest rates linked to micro and small enterprises decrease by 1.7%.

These are all positive effects in terms of credit performance. Treated individuals are able to obtain larger loans at better rates, and these effects are economically significant even with several years separating exposure to the lessons and measurement of outcomes. However, what is the cost in terms of repayment ability? Are these young adults getting over-indebted? Table 2 confirms that the expansion of debt balances does not come at the expense of repayment capacity. Columns (1) and (3) show that the probability to hold overdue debt or written-off debt does not change due to the treatment. Moreover, overdue and written-off debt balances remain similar to the levels observed in the control group (see columns (2) and (4)).

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, the expectation is that they would 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.

These latter results differ from the evidence presented in Bruhn et al. [2014] 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. Conversely, Horn et al. [2023] found significant positive effects on savings and income five years after their youth club intervention. These downstream effects in Uganda 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.

4.2 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. 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, and baseline academic (as measured by average or math grades) or 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 or SES background.

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.

Data from the Global Findex Database show that there is a significant gender gap in terms of financial inclusion in the developing world. For instance, the gap in the probability to borrow from a formal financial institution in Latin America and the Caribbean is 10.8 percentage points (35.1% for men versus 24.3% for women). Moreover, the most recent OECD/INFE 2020 Survey documents a moderate financial literacy gender gap: on average, men score higher than women by 4 points out of 100 in financial knowledge; but, in some countries, this difference is greater than 10 points. In Peru, the gender gap is slightly below the average.

However, the OECD/INFE 2020 Survey did find that women had lower financial resilience than men before the COVID-19 pandemic. On average, women were 22 percentage points more likely than men to report that their income did not cover their expenses; meanwhile, men were about 24 percentage points more likely to be able to sustain their expenses for three months without borrowing or relocating, and 23 percentage points more likely to cover a month of major expenses without external help [OECD, 2023]. The gender gaps documented in the literature suggest that treatment impacts by sex could emerge from the pilot, since girls may start off at a disadvantageous position relative to boys, both in terms of baseline financial knowledge and financial inclusion levels.

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 4 and 5 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 4 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. Still, the gender gap in access to credit, particularly as reflected by debt balances, is jointly driven by a larger drop in revolving credit among women and larger growth in non-revolving loans among men. Panel D confirms that there are no significant differences by sex in terms of the impacts on interest rates. Moreover, Panel A in Table 5 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. However, the results reveal a disadvantage for treated men, who seem to have faced increased overdue and written-off debt balances. Finally, Table A.3 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.

The heterogeneous treatment effects by initial academic performance presented in Table 6 reveal stronger gains among higher performing students at baseline. On the one hand, higher performing students experience a significant 2.2 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, the expansive treatment effects are concentrated in debt balances and loan sizes, once more led by the expansion of productive and non-revolving debt portfolios. Panel D, in turn, shows inclusive gains for bottom and top students in terms of interest rates paid. Table 7 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.4 in the Appendix shows no differences in long-term formality status by academic performance.

Finally, Table 8 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. The main difference at baseline comes from portfolio composition by SES, with students from lower SES backgrounds holding more productive and less consumption debt. Despite these baseline differences, Panel A in Table 8 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. If anything, there is a small advantage favoring lower SES students: relative to their higher SES counterparts, these students' expand their non-revolving credit balances to a greater degree. Panel D also provides evidence of homogeneous impacts by SES, with a small advantage for lower SES individuals in terms of the median interest rate paid (see columns 1 and 2). Focusing on the effects on delinquent debt, we do find that the treatment may hurt students from lower SES backgrounds. Table 9 shows that the treatment significantly increases poor students' balances of delinquent debt by almost 40%. However, the baseline levels of delinquent debt among poor students in the control group are small, under 45 US dollars. Therefore, a 40% increase amounts to 18 US dollars. This effect may be linked to the larger expansion of non-revolving consumption credit among those from lower SES backgrounds (see columns 7 and 8 in Panels B and C in Table 8). Table A.5 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. On the one hand, the long-term effects of school-based financial education seem to be equitable across sex and SES. On 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; 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.

4.3 Dynamic Treatment Impacts on Credit Behavior

The data available also enable us to look at the dynamic effects of the program between 2017 and 2023. While this sub-section evaluates the treatment effects over these seven years of individual credit histories, we place special emphasis on the effects during the pandemic years.

Examining the effects of financial education during the pandemic years is crucial for understanding its long-term impacts on financial behavior, particularly credit behavior. 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. Analyzing data from this period enables us to identify whether the skills gained through the school-based financial education program helped participants make informed decisions during times of economic uncertainty, thereby revealing greater resilience and adaptability relative to the control group. Furthermore, this analysis can provide valuable insights into how financial education can be tailored to better prepare individuals for future economic shocks, ultimately enhancing its relevance and effectiveness.

Figure 1 illustrates the effects of the financial education program on the probability of holding debt starting in 2017, i.e., the year after the lessons were delivered. The dynamic yearly effects in Panel (a) confirm that the probability of having debt does not significantly change before 2023. However, Panels (b) and (c) in Figure 1 illustrate an interesting substitution effect in terms of credit type during the pandemic. Specifically, between 2020 and 2022, treated individuals exhibited a decreased likelihood of holding revolving consumption loans while simultaneously increasing their reliance on productive loans targeted at micro and small businesses.

Figure 2 focuses on the intensive margin effects, plotting the treatment impacts on the size of debt portfolios between 2017 and 2023. Panel (a) shows that debt balances start increasing in 2021, but it is only in 2023 that we record the significant impact on indebtedness levels present in Table 1. Panels (b) and (c) in Figure 2 reveal that, during the pandemic, debt balances for productive loans increased – most notably in 2021 following the phase-out of emergency cash transfers – while simultaneously showing a consistent decline in revolving credit. While consumption non-revolving debt balances were not significantly affected during the pandemic, these balances show a growth pattern that materializes in a substantial increase by 2023.[13]

The substitution of revolving consumption loans for productive loans on the extensive and intensive margins during the pandemic reflects a positive shift in financial behavior among treated individuals. This transition coincided with decreased financial stability among households, marked by heightened financial stress and payment difficulties during the pandemic. By moving away from high-interest revolving credit, which often leads to cyclical debt and financial instability, individuals are instead opting for productive loans. This transition not only indicates a more strategic approach to borrowing but also underscores an increased focus on fostering entrepreneurial activities and investing in income-generating opportunities. Such behavior is particularly beneficial during economic downturns, as it promotes sustainability and resilience, which enabled individuals to better navigate the challenges posed by the pandemic.

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 presents the evolution of the treatment impacts between 2017 and 2023, with the goal of identifying protective effects of the program on default levels. Panel (a) shows that there is no dynamic significant effect on the probability of overdue or written-off debt. However, overdue and written-off debt balances drop in 2021, as shown in Panel (b). This coefficient is significant at the 10% level.

[13]Figure A.1 in Appendix A shows that, during the pandemic, the treatment did not alter borrowing patterns in terms of the identity of the lenders, neither in terms of the probability of holding debt, nor in terms of debt balances.

Overall, financial education lessons yielded a double protective effect during the crisis. At the peak of the COVID-19 pandemic, treated individuals were less likely to rely on worse sources of credit (i.e., revolving lines). Moreover, their repayment capacity slightly improved relative to the control group, exhibiting smaller delinquent debt balances.

5 Conclusion

This study provides robust, long-term evidence on the effects of school-based financial education, leveraging a large-scale randomized controlled trial in Peru and administrative data spanning over seven years. The findings reveal that, while the intervention did not significantly increase the overall likelihood of holding debt, it substantially shaped the composition and size of debt portfolios, particularly promoting productive and non-revolving credit. Personal finance lessons also led to slightly more favorable terms in loan agreements, led by a reduction in the median interest rates paid for productive loans. These results prove that financial education can influence not only knowledge, but also actual financial behaviors in ways that may foster financial resilience and stability.

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.

The pandemic period provided a critical context in which to assess the resilience effects of financial education. During this global crisis, treated individuals were more likely to shift away from high-cost, revolving credit sources, relying instead on more sustainable, productive loans. In addition, delinquent and overdue debt balances decreased among beneficiaries, indicating enhanced financial resilience that could help buffer households against economic shocks. These findings highlight the importance of financial literacy education not only for improving financial habits, but also for developing adaptive strategies during crises.

Despite these positive outcomes, the study also underscores the limitations of the intervention. For example, the intervention's impact on formal employment and business creation was negligible, suggesting that financial literacy alone might be insufficient to drive large-scale structural changes without additional complementary policies. Future efforts should consider integrating targeted support and infrastructure to help translate financial knowledge into broader economic participation, especially among the most vulnerable groups.

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.

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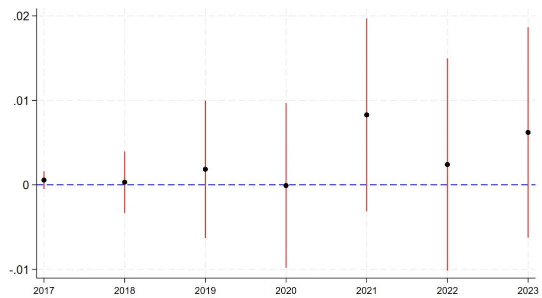
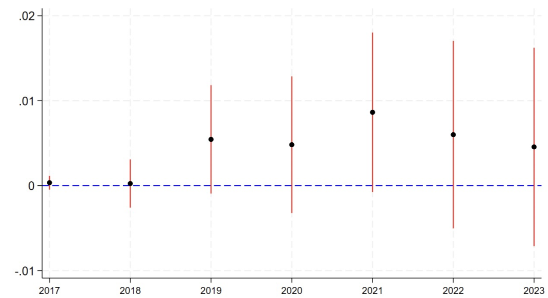
Figures and Tables

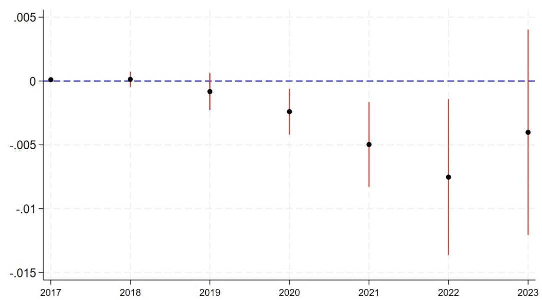
Figure 1: Dynamic Effects on the Probability of Holding Debt



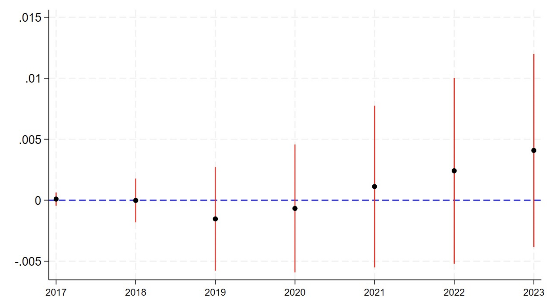
(a) Total



(b) MSE



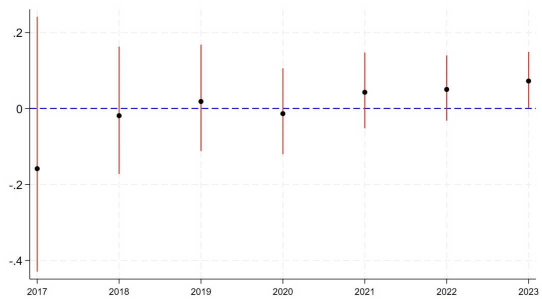
(c) Revolving



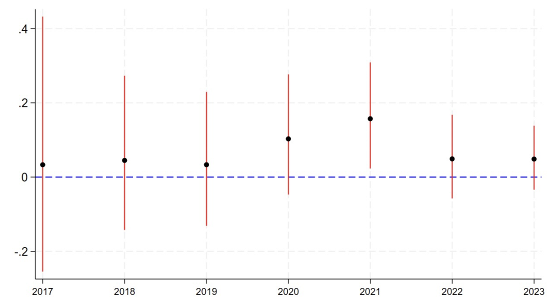
(d) Non-Revolving

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 control for gender and age and are estimated using OLS. Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the school level.

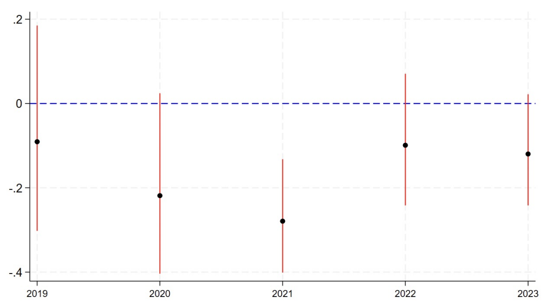
Figure 2: Dynamic Effects on Debt Balances



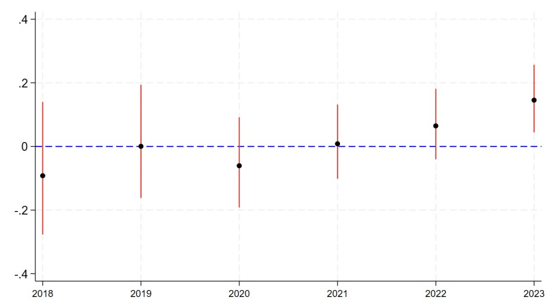
(a) Total



(b) MSE



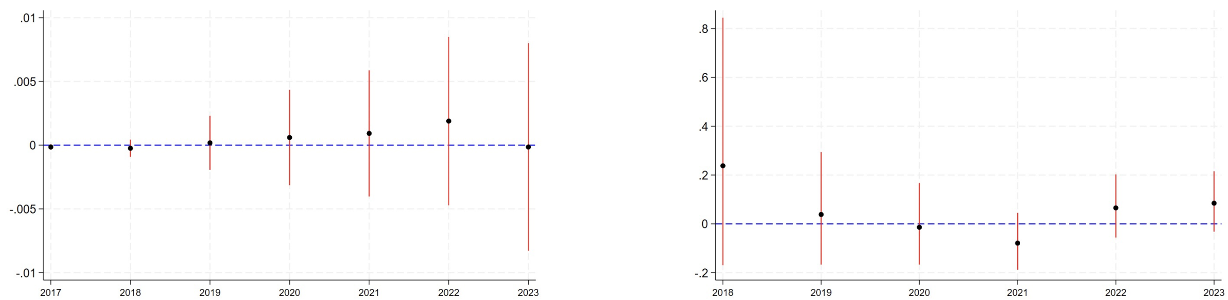
(c) Revolving



(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, defined consistently with the variables in Table 1. The figure reports $exp(\hat{\beta}) - 1$, based on Poisson regressions that control for gender and age. Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the school level.

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 control for gender and age and are estimated using OLS. The figure in Panel (b) displays the estimated effect of the treatment by year on the sum of overdue and written-off debt balance. The figure reports $exp(\hat{\beta}) - 1$, based on Poisson regressions that control for gender and age. Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the school level.

Table 1. Average Treatment 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	0.006	0.005	-0.004	0.004
	(0.006)	(0.006)	(0.004)	(0.004)
Number of Observations	57435	57435	57435	57435
Number of schools	300	300	300	300
Mean in Control	0.381	0.196	0.090	0.198
Panel B: Debt Balance				
Treatment	0.072**	0.049	-0.120*	0.145***††
	(0.038)	(0.044)	(0.067)	(0.054)
Number of Observations	57435	57435	57435	57435
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**	0.059	-0.008	0.079*
	(0.034)	(0.046)	(0.110)	(0.046)
Number of Observations	57435	57435	57435	57435
Number of schools	300	300	300	300
Mean in Control	934.3	565.3	113.9	410.8
Panel D: Median Interest Rate				
Treatment	-0.006	-0.011**	-0.003	0.006
	(0.004)	(0.006)	(0.006)	(0.004)
Number of Observations	16059	8733	2486	7588
Number of schools	300	300	250	300
Mean in Control	0.681	0.648	0.839	0.664

Notes: In Panel A, the dependent variables are binary indicators measured between January and December 2023, based on monthly reports of debt balances by type of debt. Each indicator takes the value of 1 if, in any month during this period, the individual had a positive balance in the corresponding type of debt. Panel B presents the outstanding balance of debt in December 2023, disaggregated by debt type. Panel C reports average loan sizes, calculated as the annual average of monthly values between January and December 2023. In Panel D, annual median interest rates are calculated using monthly reports between January and December 2023. MSE refers to loans given to natural or legal persons for production, trade, or service activities. Revolving includes loans where the outstanding balance fluctuates based on the borrower's decisions, as seen in credit cards. Non-revolving consists of loans repaid in fixed installments, where payments permanently reduce the debt without the possibility of reuse (e.g., mortgages, auto loans, and student loans). Panels A and D report estimates obtained using OLS, while Panels B and C report estimates based on Poisson regressions, expressed as $exp(\hat{\beta}) - 1$, to facilitate interpretation as percentage changes. All regressions control for gender and age. Standard errors clustered at school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for each family of outcomes (probability of holding debt, debt balance, average loan size, and median interest rate) by type of debt.

Table 2. Average Treatment Effects on Overdue and Written-off Debt

	(1) Pr(Overdue)	(2) Overdue	(3) Pr(Written-Off)	(4) Written-Off
Treatment	-0.002	0.095	0.000	0.080
	(0.003)	(0.095)	(0.003)	(0.072)
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: In columns (1) and (3) the dependent variables are binary indicators that take a value of 1 if the amount of overdue or written-off exceeds zero in any month between January and December 2023. The coefficients were estimated using OLS. In columns (2) and (4) the dependent variables represent the balances of overdue and written-off debt, all measured in December 2023. The table reports $exp(\hat{\beta}) - 1$, where the coefficients were estimated using Poisson regression. Overdue debt includes past-due loans, which are loans not repaid by the due date and recorded as overdue, and loans under judicial collection, which are in legal recovery processes. A written-off debt is a loan that the lender has classified as uncollectible and removed from its balance sheet. All regressions control for gender and age. Standard errors clustered at the school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for the family of outcomes formed by the four variables in this table.

Table 3. Average Treatment Effects on Formality and Income

	(1) Formality	(2) Business	(3) PFA Contributor	(4) Income
Treatment	0.001	-0.001	0.001	0.007
	(0.006)	(0.002)	(0.006)	(0.022)
Number of Observations	57,443	57,443	57,443	57,443
Number of Schools	300	300	300	300
Mean in Control	0.296	0.043	0.263	307.4

Notes: In Column (1), the variable equals 1 if the individual either contributed to their Pension Fund Administrator (PFA) in any month between January and December 2023, or if SUNAT reported that the individual owned a business in 2023. Column (2) is a dummy variable that takes the value 1 if SUNAT reported that the individual owned a business in 2023. Column (3) is a binary indicator that equals 1 if the individual reported income to their PFA in at least one month during 2023. The variable in column (4) is a continuous variable representing the annual average of monthly income reports provided to the PFA between January and December 2023. All regressions control for gender and age. Columns (1)–(3) are estimated using OLS, while Column (4) presents $exp(\hat{\beta}) - 1$, with coefficients estimated using Poisson regression. Standard errors clustered at the school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for the family of outcomes formed by the four variables in this table.

Table 4. Average Treatment Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions by Sex

	Total		MSE		Revolving		Non-Revolving	
	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male	(7) Female	(8) Male
Panel A: Probability of Holding Debt								
Treatment	0.012	0.001	0.007	0.002	-0.003	-0.005	0.005	0.003
	(0.009)	(0.008)	(0.008)	(0.007)	(0.005)	(0.005)	(0.005)	(0.006)
Number of Observations	57435	57435	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300	300	300
P-value (F)–(M)	0.316		0.552		0.868		0.850	
Mean in Control	0.377	0.385	0.229	0.163	0.080	0.100	0.174	0.223
Panel B: Debt Balance								
Treatment	0.040	0.096*	0.028	0.073	-0.198*	-0.061	0.128	0.152**†
	(0.054)	(0.053)	(0.058)	(0.065)	(0.094)	(0.086)	(0.105)	(0.074)
Number of Observations	57435	57435	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300	300	300
P-value (F)–(M)	0.469		0.605		0.246		0.866	
Mean in Control	1113.5	1512.7	544.2	460.4	113.0	141.5	276.7	702.7
Panel C: Average Loan Size								
Treatment	0.072	0.083*	0.049	0.070	-0.022	0.003	0.070	0.083
	(0.051)	(0.051)	(0.062)	(0.071)	(0.141)	(0.127)	(0.085)	(0.068)
Number of Observations	57435	57435	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300	300	300
P-value (F)–(M)	0.880		0.824		0.869		0.916	
Mean in Control	834.2	1034.9	598.2	532.3	102.4	125.3	289.1	533.1
Panel D: Median Interest Rate								
Treatment	-0.010*	-0.002	-0.015**	-0.005	-0.001	-0.006	0.004	0.008
	(0.005)	(0.006)	(0.007)	(0.009)	(0.008)	(0.009)	(0.007)	(0.007)
Number of Observations	16059	16059	8733	8733	2486	2486	7588	7588
Number of Schools	300	300	300	300	250	250	300	300
P-value (F)–(M)	0.364		0.382		0.698		0.685	
Mean in Control	0.712	0.648	0.696	0.575	0.851	0.828	0.677	0.654

Notes: See notes in Table 1 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator capturing sex of the student. Panels A and D report estimates obtained using OLS, while Panels B and C report estimates based on Poisson regressions, expressed as $exp(\hat{\beta}) - 1$, to facilitate interpretation as percentage changes. All regressions control for gender and age. Standard errors clustered at school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for each family of outcomes (probability of holding debt, debt balance, average loan size, and median interest rate) by type of debt and sex.

Table 5. Average Treatment Effects on Overdue and Written-Off Debt by Sex

	Overdue + Written-Off		Overdue		Written-Off	
	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male
Panel A: Probability of Holding Debt						
Treatment	-0.001	0.000	-0.002	-0.001	0.000	0.000
	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)
Number of Observations	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300
P-value (F)–(M)	0.889		0.740		0.975	
Mean in Control	0.144	0.146	0.097	0.103	0.099	0.095
Panel B: Debt Balance						
Treatment	-0.021	0.179*	-0.024	0.205	-0.019	0.168
	(0.087)	(0.102)	(0.172)	(0.168)	(0.089)	(0.120)
Number of Observations	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300
P-value (F)–(M)	0.155		0.421		0.219	
Mean in Control	225.8	245.7	72.1	75.3	153.8	170.4

Notes: See notes in Table 2 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator capturing sex of the student. Panels A and D report estimates obtained using OLS, while Panels B and C report estimates based on Poisson regressions, expressed as $exp(\hat{\beta}) - 1$, to facilitate interpretation as percentage changes. All regressions control for gender and age. Standard errors clustered at school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for each family of outcomes (probability of holding overdue and written-off debt, probability of holding overdue debt, probability of holding written-off debt, overdue and written-off debt balance, overdue debt balance, and written-off debt balance) by sex.

Table 6. Average Treatment Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions by Academic Performance

	Total		MSE		Revolving		Non-Revolving	
	(1) Bottom	(2) Top	(3) Bottom	(4) Top	(5) Bottom	(6) Top	(7) Bottom	(8) Top
Panel A: Probability of Holding Debt								
Treatment	-0.010	0.022***††	-0.005	0.015**	-0.007	-0.001	-0.006	0.012**††
	(0.009)	(0.008)	(0.008)	(0.007)	(0.005)	(0.005)	(0.006)	(0.005)
Number of Observations	52849	52849	52849	52849	52849	52849	52849	52849
Number of Schools	300	300	300	300	300	300	300	300
P-value (B)–(A)	0.003		0.030		0.225		0.017	
Mean in Control	0.412	0.349	0.222	0.174	0.083	0.092	0.216	0.180
Panel B: Debt Balance								
Treatment	-0.011	0.192***†††	0.026	0.122*	-0.110	-0.121	-0.029	0.410***†††
	(0.054)	(0.059)	(0.061)	(0.067)	(0.096)	(0.075)	(0.083)	(0.117)
Number of Observations	52849	52849	52849	52849	52849	52849	52849	52849
Number of Schools	300	300	300	300	300	300	300	300
P-value (B)–(A)	0.015		0.278		0.910		0.006	
Mean in Control	1363.0	1216.9	555.0	459.8	97.7	144.3	509.6	429.3
Panel C: Average Loan Size								
Treatment	-0.012	0.165***†††	0.028	0.079	-0.051	0.025	-0.037	0.221***††
	(0.048)	(0.053)	(0.065)	(0.065)	(0.156)	(0.129)	(0.075)	(0.089)
Number of Observations	52849	52849	52849	52849	52849	52849	52849	52849
Number of Schools	300	300	300	300	300	300	300	300
P-value (B)–(A)	0.019		0.585		0.661		0.057	
Mean in Control	979.9	879.4	612.2	525.3	84.8	130.4	422.7	382.6
Panel D: Median Interest Rate								
Treatment	-0.006	-0.007	-0.012*	-0.007	0.006	-0.007	0.006	0.003
	(0.005)	(0.005)	(0.007)	(0.007)	(0.009)	(0.008)	(0.007)	(0.007)
Number of Observations	14754	14754	8051	8051	2229	2229	6957	6957
Number of Schools	300	300	299	299	245	245	300	300
P-value (B)–(A)	0.892		0.557		0.267		0.759	
Mean in Control	0.683	0.678	0.655	0.638	0.842	0.836	0.672	0.660

Notes: See notes in Table 1 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator that is equal to 1 if student's GPA falls above the median within their school and 0 otherwise. Panels A and D report estimates obtained using OLS, while Panels B and C report estimates based on Poisson regressions, expressed as $exp(\hat{\beta}) - 1$, to facilitate interpretation as percentage changes. All regressions control for gender and age. Standard errors clustered at school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for each family of outcomes (probability of holding debt, debt balance, average loan size, and median interest rate) by type of debt and academic performance.

Table 7. Average Treatment Effects on Overdue and Written-Off Debt by Academic Performance

	Overdue + Written-Off		Overdue		Written-Off	
	(1)	(2)	(3)	(4)	(5)	(6)
	Bottom	Top	Bottom	Top	Bottom	Top
Panel A: Probability of Holding Debt						
Treatment	0.003	-0.002	-0.005	0.001	0.003	-0.002
	(0.006)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)
Number of Observations	52849	52849	52849	52849	52849	52849
Number of Schools	300	300	300	300	300	300
P-value (B)–(A)	0.468		0.281		0.403	
Mean in Control	0.178	0.110	0.121	0.076	0.122	0.071
Panel B: Debt Balance						
Treatment	0.064	0.158	0.119	0.071	0.039	0.202
	(0.083)	(0.123)	(0.155)	(0.191)	(0.087)	(0.145)
Number of Observations	52849	52849	52849	52849	52849	52849
Number of Schools	300	300	300	300	300	300
P-value (B)–(A)	0.529		0.862		0.320	
Mean in Control	290.8	169.8	91.9	57.5	198.9	112.3

Notes: See notes in Table 2 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator that is equal to 1 if student's GPA falls above the median within their school and 0 otherwise. Panels A and D report estimates obtained using OLS, while Panels B and C report estimates based on Poisson regressions, expressed as $exp(\hat{\beta}) - 1$, to facilitate interpretation as percentage changes. All regressions control for gender and age. Standard errors clustered at school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for each family of outcomes (probability of holding overdue and written-off debt, probability of holding overdue debt, probability of holding written-off debt, overdue and written-off debt balance, overdue debt balance, and written-off debt balance) by academic performance.

Table 8. Average Treatment Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions by Socioeconomic Level

	Total		MSE		Revolving		Non-Revolving	
	(1) Non-poor	(2) Poor	(3) Non-poor	(4) Poor	(5) Non-poor	(6) Poor	(7) Non-poor	(8) Poor
Panel A: Probability of Holding Debt								
Treatment	0.004	0.026	0.002	0.027	-0.005	0.003	0.004	0.010
	(0.007)	(0.018)	(0.007)	(0.016)	(0.006)	(0.003)	(0.005)	(0.010)
Number of Observations	57435	57435	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300	300	300
P-value (NP)–(P)	0.275		0.185		0.297		0.596	
Mean in Control	0.388	0.352	0.190	0.218	0.106	0.029	0.202	0.181
Panel B: Debt Balance								
Treatment	0.059	0.128	0.044	0.085	-0.121	-0.083	0.103*	0.316**
	(0.044)	(0.107)	(0.056)	(0.095)	(0.070)	(0.188)	(0.059)	(0.165)
Number of Observations	57435	57435	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300	300	300
P-value (NP)–(P)	0.567		0.733		0.855		0.211	
Mean in Control	1304.7	1345.2	470.1	633.2	153.1	22.2	503.9	429.8
Panel C: Average Loan Size								
Treatment	0.079*	0.068	0.074	0.034	0.003	-0.221	0.066	0.095
	(0.042)	(0.079)	(0.061)	(0.102)	(0.117)	(0.173)	(0.052)	(0.116)
Number of Observations	57435	57435	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300	300	300
P-value (NP)–(P)	0.912		0.761		0.351		0.826	
Mean in Control	910.0	1032.9	524.4	731.5	135.1	27.8	409.6	416.0
Panel D: Median Interest Rate								
Treatment	-0.003	-0.018*	-0.009	-0.013	-0.004	0.003	0.009*	-0.005
	(0.004)	(0.010)	(0.006)	(0.012)	(0.006)	(0.014)	(0.005)	(0.013)
Number of Observations	16059	16059	8733	8733	2486	2486	7588	7588
Number of Schools	300	300	300	300	250	250	300	300
P-value (NP)–(P)	0.185		0.778		0.669		0.362	
Mean in Control	0.698	0.605	0.666	0.582	0.838	0.869	0.673	0.623

Notes: See notes in Table 1 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator that is equal to 1 if the student's school is in a district classified as poor and 0 otherwise. Panels A and D report estimates obtained using OLS, while Panels B and C report estimates based on Poisson regressions, expressed as $exp(\hat{\beta}) - 1$, to facilitate interpretation as percentage changes. All regressions control for gender and age. Standard errors clustered at school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for each family of outcomes (probability of holding debt, debt balance, average loan size, and median interest rate) by type of debt and socioeconomic level.

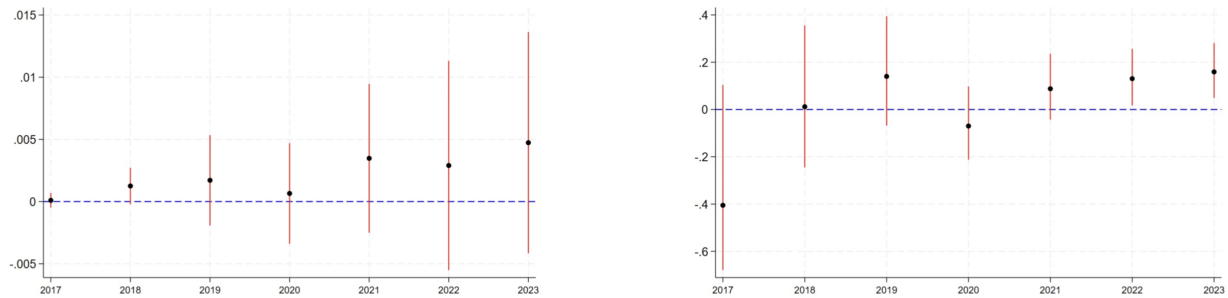
Table 9. Average Treatment Effects on Overdue and Written-Off Debt by Socioeconomic Level

	Overdue + Written-Off		Overdue		Written-Off	
	(1)	(2)	(3)	(4)	(5)	(6)
	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor
Panel A: Probability of Holding Debt						
Treatment	0.000	0.010	-0.001	0.005	-0.000	0.011*
	(0.004)	(0.008)	(0.003)	(0.006)	(0.004)	(0.006)
Number of Observations	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300
P-value (NP)–(P)	0.343		0.359		0.140	
Mean in Control	0.154	0.109	0.105	0.077	0.104	0.069
Panel B: Debt Balance						
Treatment	0.050	0.397**	0.069	0.284	0.040	0.445**
	(0.068)	(0.208)	(0.111)	(0.304)	(0.075)	(0.214)
Number of Observations	57435	57435	57435	57435	57435	57435
Number of Schools	300	300	300	300	300	300
P-value (NP)–(P)	0.094		0.505		0.059	
Mean in Control	253.3	164.5	76.1	63.8	177.2	100.6

Notes: See notes in Table 2 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator that is equal to 1 if the student's school is in a district classified as poor and 0 otherwise. Panels A and D report estimates obtained using OLS, while Panels B and C report estimates based on Poisson regressions, expressed as $exp(\hat{\beta})-1$, to facilitate interpretation as percentage changes. All regressions control for gender and age. Standard errors clustered at school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, ††† 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for each family of outcomes (probability of holding overdue and written-off debt, probability of holding overdue debt, probability of holding written-off debt, overdue and written-off debt balance, overdue debt balance, and written-off debt balance) by socioeconomic level.

A Appendix: Additional Figures and Tables

Figure A.1: Dynamic Effects on Debt Held in Banks



(a) Pr(Holding Debt) (b) Debt Balances

Note: The figure in Panel (a) plots the estimated effect of the treatment by year on the probability of having direct debt in private and public banks. 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 direct debt in private and public banks at any point from January to December of each year. All regressions control for gender and age and are estimated using OLS. The figure in Panel (b) displays the estimated effect of the treatment by year on the direct debt balance in private and public banks. The definition of the variable is consistent with the dependent variables used in Table 1: it is a continuous measure of the direct debt balance in private and public banks, measured in December of each year. The figure reports $exp(\hat{\beta}) - 1$, based on Poisson regressions that control for gender and age. Vertical bars indicate 95% confidence intervals. Standard errors are clustered at the school level.

Table A.1. Balance check

Variable	Control mean	Treatment-Control	N
Male	0.499	0.023	57443
	[0.500]	[0.015]	
Age	22.612	0.013	57443
	[1.095]	[0.013]	
GPA 2015 (0–20)	13.674	–0.070	52856
	[1.491]	[0.038]*	

Note: Test for joint covariates orthogonality p-value = 0.7497. Significance levels (* 10%; ** 5%; *** 1%) captured through OLS estimation accounting for clustered (school) standard errors. Standard errors (deviations) of coefficients (control means) are in brackets.

Table A.2. Average Treatment Effects on the Probability of Holding Debt, Debt Balances, and Borrowing Conditions by Type of Institution

	(1) Private and Public Banks	(2) Microfinance	(3) Others
	Panel A: Probability of Holding Debt		
Treatment	0.005	0.000	0.002
	(0.005)	(0.006)	(0.005)
Number of Observations	57443	57443	57443
Number of schools	300	300	300
Mean in Control	0.190	0.182	0.112
	Panel B: Debt Balance		
Treatment	0.121**	0.052	-0.003
	(0.062)	(0.049)	(0.056)
Number of Observations	57,435	57,435	57,435
Number of Schools	300	300	300
Mean in Control	517.1	584.0	211.5
	Panel C: Average Loan Size		
Treatment	0.161***†	0.078*	-0.033
	(0.065)	(0.042)	(0.052)
Number of Observations	57435	57435	57435
Number of Schools	300	300	300
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	6995	7258	4937
Number of schools	297	299	296
Mean in Control	0.774	0.502	0.813

Notes: In Panel A, the dependent variables are binary indicators measured between January and December 2023, based on monthly debt balance reports by type of institution. They take a value of 1 if, in any month between January and December 2023, the individual holds a positive debt balance in the corresponding institution. Panel B reports debt balances in December 2023 by type of institution. Panel C reports average loan sizes, calculated as the annual average of monthly values between January and December 2023. In Panel D, the outcomes represent the annual median interest rate, calculated using monthly reports from the same period. Column 1 refers to debt in a private or public bank. Column 2 corresponds to debt in a Municipal Savings and Credit Bank (CMAC), a Rural Savings and Credit Bank (CRAC), or a Credit Company. Column 3 refers to debt in a *Financiera*. Panels A and D report estimates obtained using OLS, while Panels B and C report estimates based on Poisson regressions, expressed as $exp(\hat{\beta}) - 1$, to facilitate interpretation as percentage changes. All regressions control for gender. Standard errors clustered at the school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for each family of outcomes (probability of holding debt, debt balance, average loan size, and median interest rate) by type of institution.

Table A.3. Average Treatment Effects on Formality and Income by Sex

	Formality		Business		PFA Contributor		Income	
	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male	(7) Female	(8) Male
Treatment	0.003	-0.001	0.002	-0.004*	0.000	0.001	-0.020	0.024
	(0.008)	(0.008)	(0.003)	(0.002)	(0.008)	(0.008)	(0.040)	(0.030)
Number of Observations	57443	57443	57443	57443	57443	57443	57443	57443
Number of Schools	300	300	300	300	300	300	300	300
P-value (F)–(M)	0.738		0.085		0.946		0.434	
Mean in Control	0.256	0.336	0.043	0.043	0.222	0.304	242.0	373.1

Notes: See notes in Table 3 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator capturing sex of the student. All regressions control for gender and age. Columns (1)–(6) are estimated using OLS, while Columns (7-8) presents $exp(\hat{\beta})-1$, with coefficients estimated using Poisson regression. Standard errors clustered at the school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for the family of outcomes formed by the four variables in this table by sex.

Table A.4. Average Treatment Effects on Formality and Income by Academic Performance

	Formality		Business		PFA Contributor		Income	
	(1) Bottom	(2) Top	(3) Bottom	(4) Top	(5) Bottom	(6) Top	(7) Bottom	(8) Top
Treatment	-0.005	0.006	-0.002	-0.001	-0.004	0.007	-0.011	0.024
	(0.008)	(0.006)	(0.003)	(0.002)	(0.008)	(0.006)	(0.028)	(0.028)
Number of Observations	52856	52856	52856	52856	52856	52856	52856	52856
Number of Schools	300	300	300	300	300	300	300	300
P-value (B)–(A)	0.164		0.790		0.158		0.291	
Mean in Control	0.300	0.288	0.041	0.044	0.269	0.254	310.7	302.5

Notes: See notes in Table 3 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator that is equal to 1 if student's GPA falls above the median within their school and 0 otherwise. All regressions control for gender and age. Columns (1)–(6) are estimated using OLS, while Columns (7-8) presents $exp(\hat{\beta})-1$, with coefficients estimated using Poisson regression. Standard errors clustered at the school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for the family of outcomes formed by the four variables in this table by academic performance.

Table A.5. Average Treatment Effects on Formality and Income by Socioeconomic Level

	Formality		Business		PFA Contributor		Income	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor
Treatment	-0.002	0.013	-0.002	0.002	-0.002	0.011	-0.008	0.095
	(0.006)	(0.018)	(0.002)	(0.003)	(0.006)	(0.017)	(0.021)	(0.095)
Number of Observations	57443	57443	57443	57443	57443	57443	57443	57443
Number of Schools	300	300	300	300	300	300	300	300
P-value (NP)–(P)	0.441		0.321		0.468		0.274	
Mean in Control	0.311	0.235	0.046	0.030	0.276	0.210	325.1	235.5

Notes: See notes in Table 3 for definitions of dependent variables. Heterogeneous treatment effects are obtained using a pooled regression including an interaction with a binary indicator that is equal to 1 if the student's school is in a district classified as poor and 0 otherwise. All regressions control for gender and age. Columns (1)–(6) are estimated using OLS, while Columns (7-8) presents $exp(\hat{\beta}) - 1$, with coefficients estimated using Poisson regression. Standard errors clustered at the school level are in parentheses. Stars denote significance levels (* 10%; ** 5%; *** 1%) based on unadjusted p-values. Daggers denote significance levels based on the Romano-Wolf adjusted p-values († 10%, †† 5%, † † † 1%) resulting from 1,000 bootstrap replications. Correction for multiple testing implemented for the family of outcomes formed by the four variables in this table by socioeconomic level.