The Long-Term Impact of High School Financial Education: Evidence from Brazil

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Abstract

As the financial system expands to new clients and services, countries are promoting financial education, with unknown long-run returns. In 2011, we studied the short-run impact of a comprehensive financial education program through a randomized controlled trial with 892 high schools in Brazil. This paper uses administrative data for 16,000 students over the next nine years to measure the program's long-term impact. We find that treatment students are less likely to borrow from expensive sources or to make delayed loan repayments than control students. The program also caused students to shift from formal jobs to microenterprise ownership.

JEL Codes: D14, D91, I25, O12

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1. Introduction

Financial literacy mediates economic decisions in an increasingly complex environment. Among the main decisions that can be improved by financial literacy are wealth accumulation, investment choices, preparation for retirement and use of credit (Lusardi and Mitchell 2014). As the financial system expands to include new clients and new services, the threat of bad choices resulting in welfare loss makes financial literacy even more salient.

Financial skills were included in the Vision Statement of United Nations Secretary-General for the Transforming Education Summit in 2022. As of 2015, at least 64 countries around the world were implementing or planning national strategies for financial education (OECD 2015) to promote financial literacy. Many of these strategies involve teaching financial education in schools. Proponents of this approach argue that school financial education guarantees broad coverage and reaches individuals at a stage in life where they are particularly receptive to learning and forming behavioral patterns (e.g., Bruhn et al. 2014; Lusardi et al. 2010; Frisancho 2023; Lührmann et al. 2018). On the other hand, opponents claim that "requiring schools to spend time and money teaching financial literacy is a worse financial decision than any that those high-schoolers are likely to make anytime soon," pointing out that effects on financial behavior seem to be small and are uncertain in the long run (Ogden 2019).

Our paper contributes to this discussion by providing evidence of the long-run effects of high school financial education. A meta-analysis of 37 studies recently concluded that school financial education programs have strong effects on financial knowledge and weaker but still statistically significant effects on financial behavior (Kaiser and Menkoff 2020). However, most studies measure only short-term effects, less than a year following the intervention.[1] Another literature review likewise concludes that financial education for students has "a positive effect on short-term financial knowledge and awareness of the young, but there is no proven evidence on long-term behavior" (Entorf and Hou 2018).

Studying the long-run effects of school financial education programs is important since the meta-analysis by Kaiser and Menkoff (2020) suggests that effects may decline over time (in a timeframe of up to 80 weeks), perhaps because people forget what they were taught (see also Fernandes et al. 2014). In addition, the financial decisions taken while in school are typically more limited in scope and involve lower stakes than the decisions that students may take later in life. Finally, students may experiment with financial products in the short run as part of the learning process, leading to different behavior later (see Kolb, 1984, on experiential learning).

We measure the long-term effects of a comprehensive high-school financial education program in Brazil on financial behavior and labor market outcomes, for up to nine years after the program ended (and students graduated from high school). The program was implemented during a 17-month period across the 2010 and 2011 academic years. Bruhn et al. (2016) measured the short-term effects of the program in a randomized controlled trial, spanning 892 public high schools in six Brazilian states and approximately 25,000 students, with a baseline and two follow-up surveys conducted in 2010 and 2011. Half the schools were randomly selected to receive teacher training and financial education textbooks, which they integrated into the existing curriculum during the two last years of high school, i.e., the second and third years. Control group schools did not receive training or materials but participated in the surveys. Students in the randomized controlled trial graduated

[1] An exception is Frisancho (2023), who examines credit bureau data for up to three years after a financial literacy intervention started in Peruvian high schools (corresponding to one to two years after the students graduated). The intervention led to less borrowing for students in some subgroups.

from high school at the end of the program and the control group was never exposed to the financial education program.

The short-term effects measured in Bruhn et al. (2016) show increased financial knowledge, as well as positive effects on savings attitudes, self-reported saving for purchases, money management, and budgeting. By contrast, the program also led to significantly greater use of expensive financial products such as credit cards, and a higher likelihood of being behind on credit repayments, likely because the program tried to inform students about these products but did not actively discourage their use, potentially inducing experimentation. Finally, administrative data showed that the program also increased the grade passing rate, i.e., led fewer students to have to repeat a school year.

To measure the long-term effects of the program, we obtained taxpayer identification numbers (CPFs) for nearly 16,000 students in the original sample by matching students' name and age to a tax registry list covering the entire Brazilian population. School and student baseline characteristics collected by Bruhn et al. (2016) are balanced across the treatment and control groups in this sample of 16,000 students with a CPF. We then use the CPF to consult administrative data housed at the Central Bank of Brazil (BCB). These data include bank account ownership, use of various credit products, as well as information on formal employment status and formal microenterprise ownership.[2] We follow students until February 2020, just before the COVID-19 pandemic hit Brazil.[3] To the best of our knowledge, ours is the first study to present long-term evidence on the effectiveness of financial education via a large, randomized controlled trial, and to jointly analyze a diverse set of administrative records on long-term outcomes.

[2] We tried to request data on university enrollment from the Ministry of Education but were not able to obtain and merge this data with our sample due to confidentiality reasons.
[3] We do not include data from the post-COVID-19 period in our analysis since the crisis likely altered the typical functioning of the credit and labor markets.

We first document that a high percentage of students (96 percent) have a bank account at the end of our study period. Most students open bank accounts during or right after high school, with account ownership rates increasing from 6 percent before to 79 percent two years after high school. This pattern suggests that high school is a relevant teachable moment for providing financial education, also since account ownership may come with access to other financial products, including credit.

Relying on the comparison of the randomized treatment and control groups, we find no effect of the high-school financial education program on bank account ownership, which could be because the program did not emphasize specific financial products. Instead, it focused on the importance of financial planning, saving, and using credit responsibly. Our administrative data does not include account balances, so that we cannot measure the effect of the program on savings. However, we examine whether the program led to more responsible credit use.

Like many other countries, Brazil has seen an increase in household debt, as new clients have gained access to the financial system (Garber et al. 2019). The most readily available types of credit, credit cards and overdrafts, are also the most expensive ones, with annual percentage rates (APRs) of up to 328 percent. The BCB has had an interest in reducing the use of credit card debt and overdrafts for financial consumer protection purposes.[4] Kim and Lee (2018) suggest that financial literacy may be particularly important for avoiding expensive types of credit.

Indeed, our results show that treatment students are significantly less likely to use credit than control students, particularly in the most expensive credit

[4] Credit card debt and overdrafts are often used by borrowers with low financial education, who may not be able to assess the true cost of these credit lines. These types of credit tend to have high default rates and a low elasticity of demand to the interest rate. In 2020, BCB capped the APR for overdrafts at 152 percent, while allowing lenders to charge an availability fee (capped at 3 percent per year for unused credit lines exceeding BRL 500). The BCB is currently evaluating a cap on interest rates for credit card debt.

categories: credit cards and overdrafts. Treatment students are 1.4 percentage points less likely to have credit card debt[5] and 0.9 percentage point less likely to use overdrafts, compared to 23 percent of control students with credit card debt and 11 percent of control students with overdrafts. Both effects persist over time, that is, they are equally strong eight to nine years after the financial education program ended as they were five to seven years after the program.

Interestingly, the long-run effect on using expensive sources of credit is the opposite of the short-run effect found in Bruhn et al. (2016). That is, in the short run, treatment students were more likely to use expensive sources of credit than control students, while treatment students are less likely than control students to use these sources in the long run. While we do not have further data to analyze mechanisms, we hypothesize that in the short-run, students experimented with expensive credit early and realized that this was not a sound financial decision. It is also likely that the credit they used while still in high school was only for small purchases, whereas in the long term the stakes are higher, and students avoid larger amounts of debt.

In the short-run follow-up survey, treatment students were more likely to report than control student that they were behind on credit repayments. Consistent with the self-reported data, in the administrative data, we find treatment students have more losses from loans taken out close to the time of the intervention than control students. In contrast, in the long run, the program improved repayment behavior. Treatment students have a lower likelihood of having more recent loans with repayment delays, by about 0.9 percentage point, compared to 15 percent of control students who have loans with repayment delays.

To study the effects of the program on entrepreneurship, we use data on formal business ownership that is available in the BCB administrative records. The

[5] Revolving debt or installment plans to pay outstanding balances.

MEI (individual microentrepreneur) data set covers about 42 percent of all registered businesses in Brazil. We examine the effect on formal microenterprise ownership for two post-program periods: (i) one to seven years after graduating, when students may still have been in university; and (ii) eight to nine years after graduating, when most students were most likely in the labor market. We find that treatment group students are not more likely to be microentrepreneurs than control students one to seven years after graduating, but eight to nine years after graduating, treatment students are 10 percent more likely to own a formal microenterprise than control students (a 0.69 percentage point increase relative to 6.9 percent of control students with an MEI). We also find that treatment group students are 1.2 percentage points less likely to hold a formal job, i.e., a job with a written contract, relative to 49.5 percent of control students with a formal job, suggesting that the financial education program caused them to switch occupations from being employees to being business owners.

These effects on employment outcomes may be attributed to the fact that the program was comprehensive and included modules on work and entrepreneurship. A related study by Chioda et al. (2021) examines the labor market effects of a 3-week entrepreneurship program for high school students in Uganda. Data from a follow-up survey, conducted 3.5 years after the program ended, shows that the program increased the probability of having a business by about 6 percentage points, relative to 33.6 percent of control students who reported owning a business. The percentage of students with a business is much smaller in our sample (6.9 percent), in part because we use administrative data and thus focus on businesses that are registered with the government. The percentage increase in students with a business found by Chioda et al. (2021), about 18 percent, is larger than the 10 percent increase in formal business ownership we observe in our data. A reason for the smaller effect in our study could be that the financial education program in Brazil dedicated two out of nine modules to work and entrepreneurship, while the program in Uganda focused entirely on entrepreneurship.

We also examine a proxy for informal employment derived from data on a COVID-19 pandemic government transfer distribution in 2020. This proxy provides weak evidence that the financial education intervention increased the share of students in the informal sector. We cannot disentangle whether these students are owners of informal firms or employees who are not registered with the government. However, we suspect that the effect is driven by informal business owners, not employees, since our other results show that the financial education program increased formal microenterprise ownership. Some students who started their own business likely started informal businesses since nearly two-thirds of businesses in Brazil are informal (Ulyssea 2018).

Tying these results together, the high-school financial education program had lasting effects on long-run credit use and employment outcomes. The finding that financial education in schools affects long-run economic behavior adds to the existing literature, which hypothesized that the effects of financial education may decline over time and fizzle out in the long run (Kaiser and Menkoff 2020).

Even though the magnitude of the effects is not large, the program in Brazil has contributed to the financial consumer protection goal of reducing the use of credit in the most expensive categories. It also shifted employment from formal sector jobs to entrepreneurship, which may be a desirable effect even if some of the businesses created due to the program are informal. Maloney (2004) reports that, according to the Brazilian National Household Sample Survey (PNAD), most informally self-employed in Brazil prefer their jobs over being a formal sector employee. We examine earnings data from the 2019 PNAD, which suggests that switching from being formally employed to being business owners allows students to earn the same or more and comes with greater upward potential in income.

This paper proceeds as follows. Section 2 summarizes the short-term impact evaluation design and its results. Section 3 discusses the rationale for a long-term impact evaluation, as well as the long-term sample, data sources, and timeline. Section 4 presents the long-term impact evaluation results. Section 5 concludes.

2. The Short-Term Impact Evaluation (2010-2011)

2.a. Context and Research Design

The original study of the impact of financial education in Brazil focused on youth in the 11th and 12th grades of high school. This focus on youth was attractive and relevant for several reasons. First, good financial habits formed at an early age are likely to benefit schooling, employment, and standards of living throughout adulthood. Second, the focus on youth leverages their learning capacity, as students are primed to absorb, recall, and apply learning on a regular basis, in contrast to adults, who are typically less engaged in this learning routine. Finally, well-informed students can modify not only their own financial choices, but also act as agents of change in their households' financial decisions.

In Bruhn et al. (2016), we tested short-term answers to these questions through a randomized controlled trial of a comprehensive financial education program for public high school students in Brazil. The program was developed and implemented as part of the National Strategy for Financial Education (ENEF). It spanned a 17-month period across the 2010 and 2011 academic years, and was integrated into classroom curricula of Mathematics, Science, History, and Portuguese. The program did thus not add extra hours of instruction. It used new textbooks with interactive classroom exercises on financial education themes, take-home exercises such as creating household budgets with parents, and role-play assignments. The textbooks covered nine themes: everyday family life, social life, personal property, work, entrepreneurship, large expenditures, public goods, the country's economy, and the world economy. Appendix 1 in Bruhn et al. (2016) provides a detailed list of topics within each theme.[6]

The curriculum was complemented by teacher training, web learning tools, and instructor handbooks. Furthermore, the learning was continuous throughout the school year, which stands in contrast to typical workshop-based financial education programs that are delivered in one shot and vary in length from 90 minutes to a few hours. As such, the intensity of treatment of this program was much stronger than typical one-off financial education workshops. To date, this study is the largest randomized evaluation in the financial education literature, covering 892 public high schools in six Brazilian states and approximately 25,000 students.

As part of the original study, schools in the study sample were assigned their treatment status through stratified and matched randomization, with schools within each state matched into pairs based on their pre-existing school- and community-level characteristics. Within each pair, one school was then randomly assigned as treatment while the other served as control.

Treated schools received financial education material and teacher training. Control schools did not receive any material or training but participated in surveys and testing in the same manner as the treated schools. One eleventh grade class in each school participated in our study. Treated classes received the first semester of financial education during the second semester of eleventh grade (Fall 2010) and the second and third semesters of financial education throughout twelfth grade (Spring 2011 and Fall 2011), the last year of high school. Most students in the sample were between 15 and 17 years of age at the start of the intervention. The rationale for choosing this age group was to engage with students who were already making some personal financial decisions, for example, when purchasing consumer products. Many students in this cohort also worked and earned income so there was

[6] The textbooks are available on ENEF's website: https://www.vidaedinheiro.gov.br/en/livros-ensino-medio/

an opportunity to apply newly learned financial concepts to their concurrent decisions.

The short-term analysis was based on three rounds of data collection: baseline (August 2010), follow-up 1 (December 2010), and follow-up 2 (December 2011). In addition, complementary administrative data on school graduation and dropout rates was compiled for the study period. Finally, teachers and principals were interviewed for feedback on the program.

2.b. Short-Term Results

The empirical analysis of short-term impacts used the three surveys described above and applied the following intent-to-treat OLS specification:

$$y_{i,s,f} = \beta \mathrm{Treatment}_{i,s} + \sum \gamma_s d_s + \delta y_{i,s,b} + \eta f_{i,s} + \varepsilon_{i,s}, \tag{1}$$

where $y_{i,s,f}$ is a measure of the financial knowledge, attitude, or behavior, of student *i* in school pair *s* at follow-up *f*. The variable $\mathrm{Treatment}_{i,s}$ indicates whether an individual is in a school that was randomized into treatment or not and is thus equal to one for the treatment group and equal to zero for the control group. Equation 1 includes a set of dummy variables, d_s, for the school pairs formed prior to randomization, the baseline outcome variable, $y_{i,s,b}$, and a dummy for whether student *i* is female, $f_{i,s}$. The standard errors, $\varepsilon_{i,s}$, are clustered at the school level.

This original analysis found unambiguous positive treatment effects on student financial proficiency and graduation rates, but the effects on student financial behavior, especially their use of credit, were mixed with some cautionary results. Specifically, we found the financial education program caused a quarter of a standard deviation improvement in student test scores on an SAT-like financial proficiency test, with a distributional shift to the right for students at all levels of initial capability. We also found a 9 percent lower failure rate and significantly higher passing rate in treated schools compared to control schools, with no effect on the drop-out rate.

On financial behavior, the program led to positive treatment effects on some key areas of focus, namely saving up for purchases, money management, and budgeting. Treatment students were 12.5 percent more likely to save on the extensive margin and to save significantly higher amounts than the control group. These students were also 21 percent more likely to list monthly expenses in a budget and 4 percent more likely to negotiate prices when buying consumer products. In addition, treatment students scored significantly higher on two psychology-based indices on intentions to save and financial autonomy that (i) identified preferences over hypothetical savings and spending scenarios, and (ii) measured whether students felt empowered, confident, and capable of making independent financial decisions.

By contrast, the findings on real purchasing decisions and use of credit were mixed. Specifically, we found a significantly higher likelihood of borrowing by students in treated schools and greater likelihood of purchasing consumer items such as electronics, shoes, and clothing. In fact, we found a significantly greater use of expensive financial products such as credit cards, and a higher likelihood of being behind on credit repayments.

This mix of positive and perverse results serves as strong motivation for a study of longer-term impacts of the financial education program.

3. The Long-Term Impact Evaluation (2012-2020)

3.a. Rationale for a Long-Term Follow-Up

The concluding assessment of the short-term study called for a longer-term follow-up to assess the overall welfare implications of the financial education program. Specifically, we wrote:

"…our findings offer mixed evidence on the impact of financial education in schools at least in the short-term. On the one hand, we find clear and positive impacts of financial education on some key outcomes such as financial proficiency, graduation, savings... On the other hand, we find some perverse results on purchasing behavior with greater use of expensive credit and evidence on being behind on some repayments. We acknowledge that it is difficult to draw overall welfare conclusions at this stage and longer term follow-up data on students would be needed to help identify whether the use of expensive credit for consumer purchases was sustained and what effects it had on long-term repayment rates and other financial and real outcomes."

This study picks up where the previous study concluded and addresses the outstanding question of longer-term impacts. One hypothesis we previously posed for the mixed results was related to a multi-tasking problem. While the financial education curriculum urged students to save, budget, and spend wisely, it did so simultaneously, and the concurrent emphasis may have been overbearing. Furthermore, a closer examination of the course books suggests that while the curriculum offered very clear direction on actions such as saving and budgeting (both are good), there was no such indication when it came to purchasing items on expensive credit cards or installment plans. The course instead urged greater awareness and understanding of the pros and cons of spending choices but did not outright discourage the use of credit. Hence, it is likely that while students keenly followed the directions to save and budget, they did less well when such clear direction was not provided.

This type of multi-tasking problem is linked to the literature in psychology on willpower depletion. Moreover, while the school-based intervention provided the opportunity for repeated instruction and exercises that allow for sustained learning, short-term results from Bruhn et al. (2016) suggested that students tended to follow clear directions for some topics (savings, budgeting), but wavered in other aspects where direction was less clear (spending and borrowing).

In the long term, such multi-tasking problems can potentially smooth out with students having more time and opportunity to absorb and experience financial decisions, and subsequently apply learnings from the curriculum to adjust course from financial mistakes.

In this study, we test this new hypothesis on longer-term impacts by following students from the original study for the next nine years, from 2012 to 2020. Here, we rely on administrative data sources, housed at the BCB. Using administrative data was the most obvious choice since we do not have students' contact information or addresses. All data for the short-term impact evaluation was collected in schools and the students would have left those schools by now (most of them would have graduated in 2011). Additionally, the administrative data are provided by authorized third parties, meaning that they do not suffer from self-reporting bias.

3.b. Sample Selection for the Long-Term Impact Evaluation

As a starting point, we take the full sample of 35,346 students who participated in any of the surveys conducted during the short-term impact evaluation. Any given survey conducted during the short-term impact evaluation covered at most 24,473 students, but the composition of students changed throughout the study due to students rotating and repeating a year, which is why the full sample includes over 35,000 students. To match these students with the long-term follow-up data from the administrative sources housed at the BCB, we need their CPFs (the 11-digit Brazilian taxpayer identification number).

Students' CPFs were collected on three occasions during the short-term impact evaluation. First, at parent workshops that were organized in some treatment schools. Second, on lists of enrolled students that classroom teachers submitted before the final follow-up survey, which made them eligible to enter a lottery with prizes. Third, during the final follow-up survey. This resulted in CPFs for 10,846 students. Among these, 8,093 are valid numbers.

In Brazil it is common for young individuals to use the CPF of one of their parents when they do not have one for themselves, implying that some of the valid CPFs may be from parents instead of the students. For this reason, CPFs collected during the short-term impact evaluation may be wrong. Since they are also missing for 77 percent of the sample, we implemented an algorithm to find the CPFs based on the names and ages students provided on the surveys for the short-term impact evaluation.

We match students' names with the registry of names from the Federal Government Revenue Service (SRF), the Brazilian agency in charge of federal tax collection. This registry contains all CPFs that ever existed in Brazil, along with name, gender, mother´s name, date of birth and last updated location. Most adults in Brazil, even those who are relatively poor, have a CPF and are therefore present in the SRF registry. The CPF is necessary to open a bank account, hold a formal job, pay taxes, etc. In December 2019, the Cadastro Único registry, which includes poorer families representing around a third of the Brazilian population, lists a CPF for 96.2 percent of individuals aged 18 years or older.

Two main caveats arise from attempting to obtain a CPF based on students' names. The first is the possibility of misspelling of the reported names, and the second is the potential existence of homonyms. To address misspelling, we take advantage of the recurrent collection of names during the experiment. Students or their parents provided student names on nine occasions. We consider all different spellings for each name as possibly right in the matching exercise and compare all of them with the SRF registry.

In addition to the CPF, we extract from the SRF registry the name, date of birth, gender, and municipality of every individual born from 1988 to 1999. We match this data to the list of student names for all students, including those for whom we had valid CPFs from the short-term impact evaluation data.

After matching by name, we drop any matches that are not age compatible to reduce the incidence of homonyms or individuals whose name matched misspelled student names. We use the dates of birth from the SRF to compute the students' age in August 2010, when the data on age was collected during the baseline survey for the short-term impact evaluation. We then compare this calculated age with the answers given in the August 2010 survey. The possible answers to that survey were: (i) 13 years old or younger, (ii) 14 years old, (iii) 15 years old, (iv) 16 years old, (v) 17 years old, and (vi) 18 years old or older. If students selected "13 years old or younger," we considered them to be a match if their SRF computed age was between 10 and 13 years old. If students selected "18 years old or older", we considered them a match if their SRF computed age was between 18 and 22 years old. A big caveat here is that not all students in the short-term impact evaluation took the baseline survey (i.e., the students who joined the classes in the study after the school year when the baseline took place), so that we only have 18,796 students who reported their age in August 2010.

We drop any students who are not uniquely identified after matching by name and age, yielding 15,940 students for whom the algorithm found a CPF in the SRF data. Of these, 3,657 had reported a CPF during the short-run impact evaluation. Comparing these CPFs with the ones found by the algorithm shows that they are almost always the same (only 2.68 percent are different), suggesting that the algorithm does an accurate job of identifying CPFs.[7]

[7] For students with more than one age-compatible homonym, we could further try to identify the correct CPF in the SRF data using geographic information. However, the SFR contains only the most recent location and not location of birth, so that using this information could lead to false matches if students have moved. The location in the SRF registry is updated when individuals file their tax reports or when the individual requests an update. Imposing that the student location from the short-term impact evaluation matches the one in the SFR gives 2,130 additional unique matches. Out of these, 440 had reported a CPF in the short-term impact evaluation and 6.14 percent of these

3.c. Attrition and Balance of Baseline Characteristics

The students for whom the algorithm found a CPF in the SRF data form our sample for measuring the long-term effects of the financial education program. The long-term impact evaluation sample includes 45.1 percent of the students present in any of the survey rounds of the short-term impact evaluation (44.3 percent in the treatment group and 46.0 percent in the control group). The difference in the percentage of students with a CPF across the treatment and control group is thus small, although it is statistically significant at the 10 percent level.

Table 1 shows baseline characteristics for the treatment and control groups in the long-term impact evaluation sample. As in Bruhn et al. (2016), we find that the percentage of female students is statistically significantly higher in the treatment group than in the control group (56 percent vs. 54 percent) and hence we control for gender in our regressions. The only other variable that shows a statistically significant difference across the treatment and control groups is the one indicating that the student "Receives income," but this difference is only significant at the 10 percent level.

In Appendix Table A1, we check if the long-term impact evaluation sample yields similar short-run effects of the financial education program as the original sample. Here, we focus on the main outcome variables from the short-term impact evaluation: financial proficiency and financial behavior (saving, borrowing, and being behind on payments). Panel A in Table A1 replicates the findings from the short-term impact evaluation reported in Bruhn et al. (2016). Panel B shows the corresponding short-term effects in the long-term impact evaluation sample. For all variables, the treatment effects in the long-term sample are close in magnitude and

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CPFs are not the same as those identified by the algorithm. Given that this mismatch rate is higher than in the sample that does not use location to identify matches, we use only the 15,940 students uniquely identified through name and age matching in the analysis.

statistical significance to those in the short-term sample. This similarity suggests that the effects we measure in the long-term sample are representative of the original sample.

3.d. Data Sources

To capture the effects of the financial education program on students' financial life and employment outcomes, we combine four individually identified administrative data sets housed at the BCB. [8] This section briefly describes the information available in each of these data sets. A caveat is that most of the administrative data cover only formal financial and employment relationships. Section 4.d describes how we use data on pandemic aid transfers made during 2020 to construct a proxy measure of informal employment.

First, we obtained data on account ownership, from the Registry of Clients of the Financial System (CCS). The CCS includes information about every account held in financial institutions since 2001, including checking, savings, payments, and investment accounts. According to the Financial Citizenship Report 2018, information contained in CCS implied that 86.5 percent of the residents in Brazil aged 15 or older had a bank account.[9] The original data contains opening and closing dates for all accounts, which we turn into a monthly panel. Unfortunately, the CCS does not provide account balances, fees, and transactions.

Second, we have detailed information on credit. Set up in 2003 to monitor risk, the Credit Registry System (SCR) collects from lenders monthly information about every financial transaction conducted by clients who can cause the lender a loss greater than a given amount. This threshold was BRL 5,000 from 2003 until

[8] All individually identified information was handled exclusively by the BCB staff.
[9] Available in Portuguese at https://www.bcb.gov.br/content/cidadaniafinanceira/Documents/RIF/Relatorio%20Cidadania%20Financeira_BCB_16jan_2019.pdf

2012, when it fell to BRL 1,000. It was reduced again in June 2016 to BRL 200 (corresponding to around USD 46[10]). We use data from 2016 onwards, since the SCR includes relatively few transactions for our sample before the threshold reduction to BRL 200. For each transaction, the SCR records the amount, credit category, interest rate, due date, and amount in delay or classified as a loss. With clients' authorization (current or prospective), financial institutions can request this information from the credit registry. In December 2019, the SCR contained information on over 127 million individuals (around 60 percent of the Brazilian population).

Third, we build a data set of individual microentrepreneurs (MEI) using the registry of firms from the Brazilian tax authority (SRF) for 2012 through 2020. MEI is a type of firm with a simplified registration process, created in 2008 by Complementary Law 128. The only tax MEIs need to pay is a flat monthly fee below USD 15, whose major component is a contribution to the public pension system. A MEI is subject to a revenue cap of BRL 81,000 (about USD 17,000) per year and can hire at most one employee. Registration as a MEI (as opposed to operating without registration) has the advantage that it allows entrepreneurs to provide receipts to their customers, increasing the pool of partners with whom they can conduct business. The name of a MEI is automatically generated as the name of its owner concatenated with the CPF, allowing us to match the MEI database with our sample via the CPF. At the end of 2019, the SRF registry of firms included 12.8 million MEIs, representing about 42 percent of all firms in the SRF registry.[11] We do not use data on other types of firms in our analysis since they have identification numbers that are not clearly linked to the CPF of the owner.

[10] Using the exchange rate of 4.3410 BRL/USD, of February 2020 (free exchange rate - period average).
[11] For the pre-2018 period, we drop 1.37 million MEIs that were determined to be inactive in a 2018 audit. Including these MEIs in the analysis does not change our findings.

Fourth, we use data on formal employment from RAIS,[12] a database maintained by the Ministry of Labor. In RAIS, all employers in Brazil are required to report their employees who have a written contract. RAIS also includes information on wages, education, gender, sector, and type of occupation. However, RAIS is not designed to capture business owners or the self-employed. We use a monthly panel of RAIS created at the BCB, starting in 2013 and ending in 2019. In December 2019, RAIS included nearly 51 million employees.

3.e. Timeline

Table 2 provides a timeline of the study. It first summarizes when the financial education intervention happened in high schools (2010 and 2011). It then shows the years for which we have data from the sources described above. The table also lists the corresponding average student age for each year. At the time of the intervention, students were 16 and 17 years old on average. In this study, we use data that follows the students until they were up to 26 years old on average. Most of the administrative data is only available for the post-intervention period, except for account ownership, which we have for 2008 and 2009.

4. Long-Term Results

When analyzing the long-term effects of the financial education program, we use a monthly panel and estimate the following intent-to-treat OLS specification:

$$\mathrm{y}_{\mathrm{i,s,r,t}} = \beta \mathrm{Treatment}_{\mathrm{i,s,r}} + \sum \gamma_{\mathrm{s,r}} \mathrm{d}_{\mathrm{s,r}} + \eta \mathrm{f}_{\mathrm{i,s,r}} + \sum \theta_{rt} \mathrm{m}_{\mathrm{rt}} + \varepsilon_{\mathrm{i,s,r,t}}, \qquad (2)$$

where $\mathrm{y}_{\mathrm{i,s,t}}$ is a financial or labor market outcome of student i in school pair s, located in state r, in month t. The variables $\mathrm{Treatment}_{\mathrm{i,s,r}}$, $\mathrm{d}_{\mathrm{s,r}}$, $\mathrm{f}_{\mathrm{i,s,r}}$, and $\varepsilon_{\mathrm{i,s,r,t}}$ are defined as in equation 1. Following Bruhn et al. (2016), we show results for three different specifications: (i) no controls, (ii) with school pair dummies, $\mathrm{d}_{\mathrm{s,r}}$, and (iii)

[12] RAIS is the Annual Report of Social Information (Relação Anual de Informações Sociais).

with school pair dummies and gender, $f_{i,s,r}$. We add fixed effects for state r in month t, m_{rt}, to all specifications to account for regional trends in dependent variables.

We also estimate a specification with time-varying treatment effects. Here, we split the post-intervention years into two separate time periods: (i) 2012 to 2018,[13] which corresponds to the years when students may still be in university, and (ii) 2019 to 2020, when most students would have entered the labor market. In Brazil, students tend to start university one or two years after graduating from high school (because students frequently use this time to study for entrance exams) and take between four and eight years to graduate.

4.a. Financial Accounts and High School as a Teachable Moment

This section examines account holdings at financial intuitions. As explained above, this variable is the only outcome from the administrative data that is available both before and after the financial education program was implemented. Figure 1 shows that only about 6 percent of students had an account at a financial institution before high school, which is perhaps not surprising since students were only 14 or 15 years old on average. While the program was being implemented, during the last two years of high school, account holdings increased to 41 percent of students (35 additional percentage points). An additional 38 percent open an account during the following two years. Seven years after high school, almost all students in our sample have an account.

Possible reasons why account ownership increases steeply during and after high school are that 1) students' financial life becomes more independent of their parents, 2) more students start to enter the labor market, 3) students start to use more electronic payment methods, and 4) students need a minimum level of

[13]When looking at credit use (Tables 4 and 5), we include data since June 2016, when the reporting threshold of credit contracts fell significantly. For formal employment (Table 6), the first time period starts in 2013 due to data availability.

knowledge to understand the benefits of owning an account and to successfully open an account.

The pattern of account ownership over time suggests that high school is a relevant teachable moment for providing financial education as most students appear to join the formal financial system in or shortly after high school. Offering a financial education program during a teachable moment can increase its success (Kaiser and Menkhoff 2017).

Figure 1 also shows that account holdings are similar for treatment and control students, both before and after the financial education program. This lack of impact is confirmed by Table 3, which measures the effect of the financial education program on holding accounts at financial institutions by estimating equation 2. One reason why the program had no effect on account holdings could be that the program did not emphasize or endorse specific financial products (in part to avoid conflicts of interest since the steering group of the pilot included representatives from financial institutions). Instead, the program focused on the importance of financial planning, saving, and using credit in a responsible manner. The administrative data we use does not include account balances, implying that we cannot measure the effect of the program on savings. However, in the following section, we examine whether the program led to more responsible credit use.

4.b. Credit Usage

Lusardi and Mitchell (2014) argue that financial literacy may play an important role in helping consumers make sound borrowing decisions. For Brazil, Garber et al. (2021) find that those with low financial literacy tend to take out debt that increases consumption variability instead of helping to smooth it. Here, we examine whether financial education has a causal effect on borrowing decisions.

Table 4 shows the effect of the financial education program on credit usage. Column 1 includes any type of credit from a financial institution, while the remaining columns contain credit in the most common categories: credit cards, checking account overdrafts, non-payroll loans (general purpose loans, typically without any collateral), auto loans, and payroll loans (general purpose loans which use future wages as collateral).[14] For credit cards, we consider both having any positive credit card balance not yielding interest[15] (Column 2) and revolving debt or installment plans to pay outstanding balances (Column 3).

The financial education program lowered the probability of having credit in the post-intervention period by about 4 percent (a 1.96 percentage point decrease compared to 47.8 percent of control students with credit). The size of the effect is similar in the earlier years (2016 and 2018) and the later years (2019 and 2020), suggesting the effect persists through the medium to the long run (Panel D in Table 4). This persistent effect is also visible in Figure 2, which shows the proportion of students with a positive credit balance for each year from 2016 to 2020.

The effect of the financial education program on credit usage is concentrated in the most frequently used categories: credit card purchases, credit card debt, and checking accounts overdrafts (column 2 to 4 in Table 4). The latter two categories are also the most expensive ones in Brazil. For loans granted between June 2016 and February 2020, the APR across months was 328 percent for credit card debt and 265 percent for overdrafts. Nonpayroll loans carried an APR of 123 percent, auto loans of 23 percent, and payroll loans of 26 percent.[16]

Appendix Table A2 shows the effect on credit using levels instead of dummy variables as the outcomes. Panel A uses log credit balances, thus dropping all observations that do not have credit in the respective categories. Panel B uses

[14] Garber et al. (2019) provide stylized facts on the use of credit by individuals in Brazil.
[15] In Brazil, new purchases only yield interest rates if they are not paid in full at the due date of the bill, even if there is an outstanding revolving balance resulting from preceding billing cycles. There are also purchases made in installments directly at shops, who receive their payment as the installments become due.
[16] During this period inflation measured by IPCA, the Extender National Consumer Price Index, from IBGE, the National Institute of Geography and Statistics, averaged 3.6%.

log (1+balance) to keep these observations. The results suggest that the decrease in credit is driven by the extensive margin, not the intensive margin. We only see the negative and statistically significant effects on credit when including the observations that do not have credit. Conditional on having credit, treatment students do not have lower balances than control students, on average (Panel A in Table A4).

Overall, the findings suggest that the program helped students make more sound borrowing decisions in the long run, by causing them to avoid the most expensive sources of credit. In contrast, in the short run, Bruhn et al. (2016) found that the financial education program increased the probability of borrowing money, particularly from expensive sources, such as via installment plans. One reason for the different in short- and long-run effects could be experiential learning. The field of education has long recognized the central role of personal experience in the learning process. For instance, Kolb (1984) systematizes previously existing models of experiential learning, showing how initial experience affects the way new knowledge is absorbed and that knowledge can mediate how new experiences are understood, influencing subsequent choices. New information about credit may thus result in students deciding to experiment with it.

Another potential reason for the differences in short- and long-run effects on credit could be that short-run outcomes were self-reported, whereas the long-run outcomes are form administrative data. As explained above, we do not have administrative data for the short-run period (2010 and 2011) since the SCR had a high reporting threshold until 2016. However, our analysis of credit losses in Section 4.c, suggests students had higher credit losses coming from short-run credit, consistent with self-reported data. Unlike current credit contracts, we can see credit from before 2016 that resulted in losses, since loss amounts stay in the system, and many started to be reported when the SCR threshold dropped in 2016.

4.c. Repayment Behavior

In Table 5, we examine the effect of the financial education program on having credit contracts in default. Column 1 shows that treatment students are less likely to have credit contracts with repayment delays than control students (see also Figure 3). Here, delays are defined as being between one day and one year late on a credit payment. If payments are more than one year late, the credit contract is written off as a loss. Column 2 shows that treatment students are more likely to have losses than control students. However, since losses stay in the system, the difference between treatment and control students here may reflect the finding from the short-term impact evaluation that treatment students had a higher likelihood of being behind on credit repayments than control students. Following the Consumer Protection Law (Código de Defesa do Consumidor), after five years in delay, a credit contract ceases to appear in reports from the credit registry. However, the losses continue to appear in the SCR data available to BCB staff and can also be used internally by financial institutions that have this information.

In Columns 3 and 4 of Table 5, we code the outcome variables so that they exclude any credit contracts that were already classified as a loss at the beginning of the period for which we have comprehensive data (June 2016). When excluding old credit contracts, we do not find that treatment students are more likely to have credit losses than control students (Column 4 of Table 5). To the contrary, Panel D in Table 5 shows weak evidence that treatment students were less likely to have credit losses than control students at the end of the period, which we also observe in Figure 5.

We thus conclude that the financial education program caused students to have more credit losses in the short run, probably because they experimented with expensive sources of credit, as documented in Bruhn et al. (2016). In contrast, in the long run, the program lowered the likelihood that students have loans in default, by about 0.9 percentage points, relative to 15 percent of students with loans in default in the control group.

4.d. Work and Entrepreneurship

The financial education program could have affected occupational choice, particularly since two out of the nine themes covered in the financial education textbooks were work and entrepreneurship. Columns 1 and 2 of Table 6 show the effects of the program on two employment outcomes: (i) a variable that indicates that the individual owns an MEI firm; and (ii) a variable that indicates that the individual is formally employed.

The financial education program had no statistically significant effect on owning an MEI during the first seven years after the intervention, when students may still have been in university (Column 1 of Table 6). However, eight and nine years after the after the intervention, when students had likely graduated from university, treatment students were statistically significantly more likely to own an MEI than control students. The effect size of 0.69 percentage points corresponds to an increase of 10 percent relative to the control group mean of 6.9 percent of students with an MEI. Figure 5 also illustrates that the difference between the proportion of treatment and control students with an MEI increased over time.

Column 2 of Table 6 shows that the treatment group has a statistically significant 1.2 percentage points lower probability of being formally employed than the control group. The magnitude of this effect corresponds to a 2.4 percent decrease in formal employment, relative to the control group mean of 49.5 percent. Figure 6 also shows a persistently lower rate of formal job holdings among the treatment group than the control group, a difference which emerged from 2016 onwards, five years after students graduated from high school.

Our findings thus suggest that the comprehensive financial education program made students more likely to set up their own formal business instead of becoming formal employees. The effect on owning an MEI is smaller in absolute magnitude than the effect on formal employment (0.69 vs. 1.2 percentage points, respectively), suggesting that some treatment students may have started businesses that are not MEIs.

Treatment students could either operate registered businesses that are larger than MEIs or informal businesses, i.e., without registering with the government. We do not have reliable data on businesses larger than MEIs, but we can construct a proxy of informal employment, using data from a COVID-19 pandemic government transfer program. Although data from this transfer program only provides a proxy of informal businesses, we believe it is important to add it to our analysis since nearly two-thirds of businesses in Brazil are informal (Ulyssea 2018).

The COVID-19 pandemic hit Brazil in March 2020. To mitigate the economic fallout of the pandemic, the federal government instituted a transfer program called Auxílio Emergencial.[17] This program was targeted at individuals over the age of 18 years without a formal employment relationship who were either MEIs, self-employed, informal business owners, informal workers, or not working.[18] To be eligible for the program, the individuals had to have 2018 taxable income below BRL 28,500 and monthly household income per capita below half a minimum wage (about BRL 500) or total household income below three minimum wages.

We have data from the Citizenship Ministry on all Auxílio Emergencial transfers made in 2020, covering 67.8 million individuals. Assuming time persistence of occupations between 2019 and 2020, we use this data to construct a proxy of individuals working in the informal sector in 2019. We compute this proxy by taking all individuals who received the transfer in 2020 and excluding those that

[17] Law 13.982, from April 2, 2020, article 2.
http://www.planalto.gov.br/ccivil_03/_ato2019-2022/2020/lei/l13982.htm#view

[18] Individuals who received unemployment insurance were not eligible for the transfer.

belonged in 2019 to occupation categories that we can track directly through data housed at the BCB, as follows:

$$I_{i,2019} = \begin{cases} 1\ if\ recepient\ of\ 2020\ Auxílio\ Emergencial\ and\ not \begin{cases} MEI\ in\ 2019 \\ In\ RAIS\ in\ 2019 \\ PBF\ beneficiary\ in\ 2019 \end{cases} \\ 0\ otherwise \end{cases}$$

We exclude MEIs since they are formally registered with the government. We exclude individuals in RAIS since these formal employees may not have been eligible for the Auxílio Emergencial transfer in the first place. Finally, we exclude beneficiaries of Programa Bolsa Família (PBF)[19] since these low-income individuals may be out of the labor force. The proxy thus includes informal business owners (who did not register their business with the government), the self-employed (individuals who do not have a registered business but file taxes under a self-employment regime), and informal employees (employees without a written contract who are not in RAIS).

Column 3 of Table 6 shows the effect of the financial education intervention on the informal proxy. Unlike the previous columns, the analysis here is done in a cross section, not a panel, which is why we have included it only in Panel D for the time that is most similar (2019). Treatment students were 1.1 percentage points more likely to be informal (a 4.2 percent increase relative to the 25.5 percent of the control group proxied to be informal), but this coefficient is only statistically significant at the 10 percent level.

We thus find weak evidence that the financial education intervention increased the share of students in the informal sector. The data do not allow us to disentangle whether this increase comes from informal business owners or

[19] PBF is a cash transfer program for families with monthly per capita income up to BRL 89.00 (extreme poverty), and between BRL 89.01 and BRL 178.00 (poverty) if they had children under 18 years old.

employees who are not registered with the government. However, given that the program increased the share of students running a formal microenterprise (MEI) and lowered the share of students working as formal employees, we suspect that the increase is due to students running informal businesses.

Taken together, the 2019-2020 results in Panel D of Table 6 trace the effects of the financial education program on students' occupational choices almost fully. The program caused a 0.69 percentage point increase in formal microenterprise (MEI) ownership and a 1.1 percentage point increase in informal employment, adding up to 1.8 percentage points, which is almost identical to the drop in formal employment in the same period (1.7 percentage points).

To get a better sense of the welfare effects of moving from formal employment to running formal and informal businesses, we examine income reported in Brazil's Continuous National Household Sample Survey (PNAD). This survey monitors fluctuations and evolution of the labor force, covering about 211,000 households each quarter. It is representative for states, state capitals, and metropolitan areas.

Figure 7 displays average monthly earnings in the fourth quarter of 2019, for formal and informal business owners, self-employed, and employees. Formal business owners and self-employed have the highest earnings. Formal employees earn about the same as informal business owners, with slightly higher averages and medians, but lower maximum earnings. The groups with lowest earnings are the informally self-employed and informal employees.

The PNAD data thus suggests that switching from being formally employed to being business owners mostly allows students to earn the same or more and comes with greater upward potential in income.

4.e. Robustness Checks

Section 4.c concludes that the program caused students to have more credit losses in the short run, which could influence their credit use in the longer run. For example, the lower use of credit by treatment students found in Section 4.b could be due to them having lower credit scores. To explore this channel further, we check whether the effect of the program on credit use is different for the approximately 19 percent of students who had old losses (defined as credit contracts that were already a loss when they appeared in the SCR system in June 2016). Appendix Table A3 shows that having an old loss is negatively associated with credit use (between June 2016 and February 2020). However, the effect of the program on credit use is the same for those who had old losses as for those who did not, suggesting that old losses are not driving the results in Section 4.b.

Another concern is that the results in Section 4.d, showing that treatment students are less likely to hold a formal job, could potentially drive their lower use of credit found in Section 4.b. That is, students may be less likely to receive credit if they do not have a steady source of income. We do not believe this channel is behind the findings in Section 4.b. The results in Table 4 are not statistically different across students who have a formal job, according to RAIS, and those who don't have a formal job, as shown in Appendix Table A4. In Column 8 of Table A7, we use RAIS data to examine whether the financial education program affected the wages of those who were formally employed. We do not find any statistically significant effect on wages.

5. Conclusion

This paper measures the long-run effects of a high-school financial education program in Brazil on financial behavior and employment outcomes using a randomized control trial with 892 public high schools and administrative data on about 16,000 former students. Unlike previous literature that focuses on short-term effects over one to two years, we follow students for up to nine years after leaving high school.

While some literature has hypothesized that the long-run effects of high-school education would be declining or zero as students forget what they have learned, we find lasting effects. In fact, the effect on the use of expensive sources of credit goes from being positive in the short run to being negative in the long run. That is, in the long run, treatment students are less likely to use expensive sources of credit than control students, even though the opposite was true in the short run. Similarly, in the short run treatment students reported more loans with repayment delays than control students, but we find the opposite result in the long run: treatment students are less likely to have loans with repayment delays than control students. The reduction in use of expensive sources of credit is desirable from the financial consumer protection perspective since the APRs in Brazil are so high that they can make debt double or quadruple within a year.

We also observe long-run effects on labor market outcomes. Students are more likely to have formal microenterprises and less likely to work as formal employees. These effects may be attributed to the fact that the financial education program was comprehensive and included modules on entrepreneurship. Entrepreneurship can be a more desirable option than a formal job, in terms of income, independence, or flexibility (Maloney 2004). Moreover, household survey data shows that entrepreneurship yields similar or higher earnings than being an employee and has more upward potential in income. Switching from being an employee to entrepreneurship can thus be welfare enhancing.

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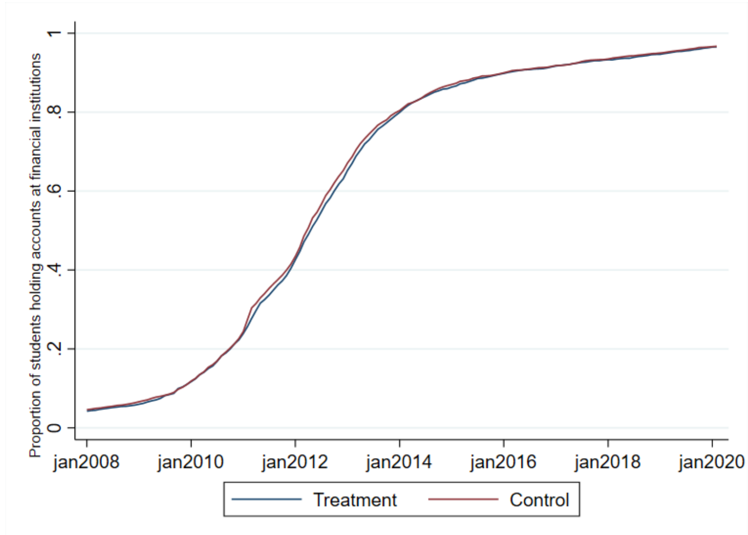
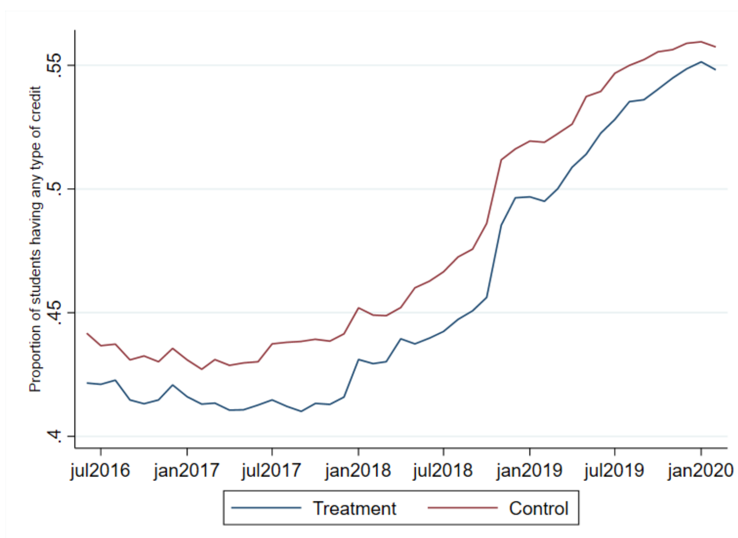
Figure 1: Students Holding Accounts at Financial Institutions over Time



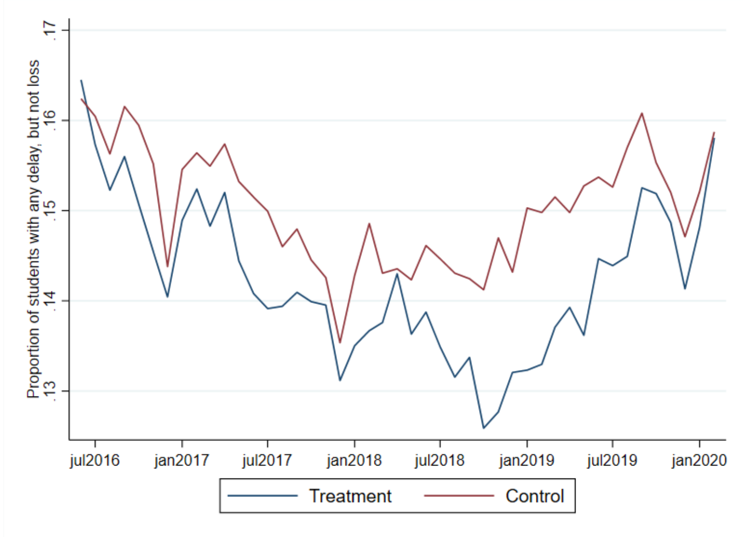
Notes: This figure shows the proportion of students in our sample that have an account with a financial institution for 2008 to 2020, using monthly administrative data from the Registry of Clients of the Financial System (CCS), housed at the Central Bank of Brazil (BCB).

Figure 2: Credit Usage over Time



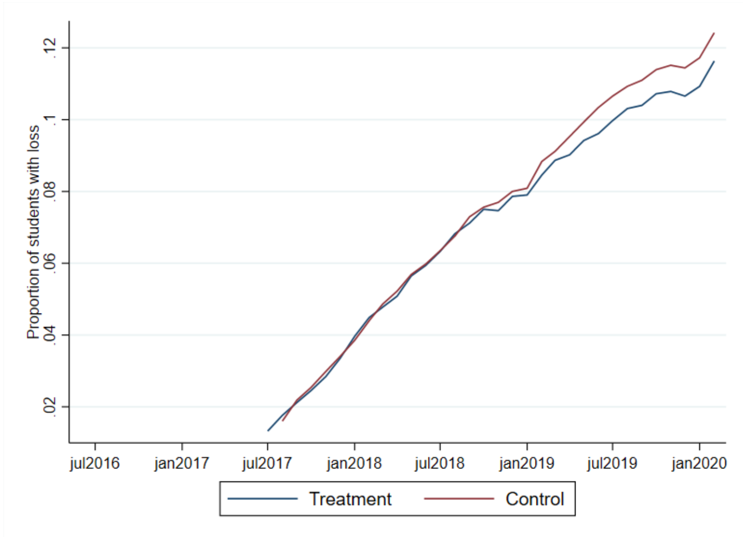
Notes: This figure shows the proportion of students in our sample that have positive credit balances, using monthly administrative data from the Credit Registry System (SCR), housed at the Central Bank of Brazil (BCB).

Figure 3: Credit Repayment Delays over Time



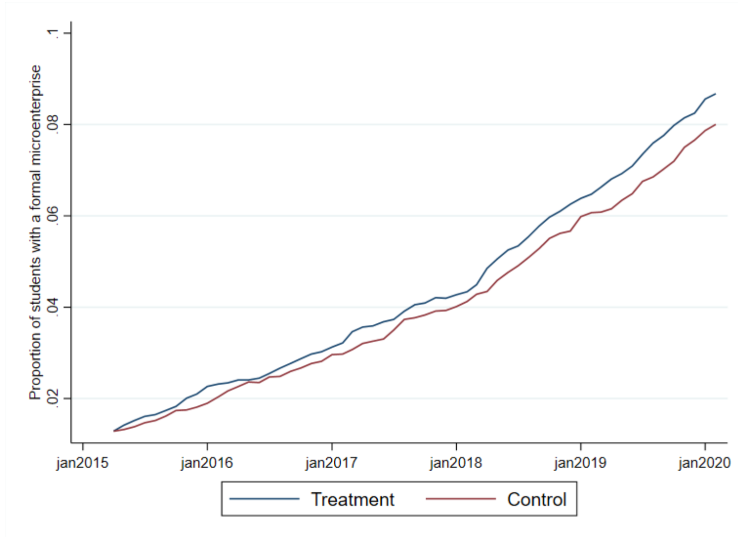
Notes: This figure shows the proportion of students in our sample in from whom the longest repayment delay is between one day and one year, using monthly administrative data from the Credit Registry System (SCR), housed at the Central Bank of Brazil (BCB). Here, we compute the longest repayment delay after dropping credit contracts that were already a loss in June 2016.

Figure 4: Credit Losses over Time



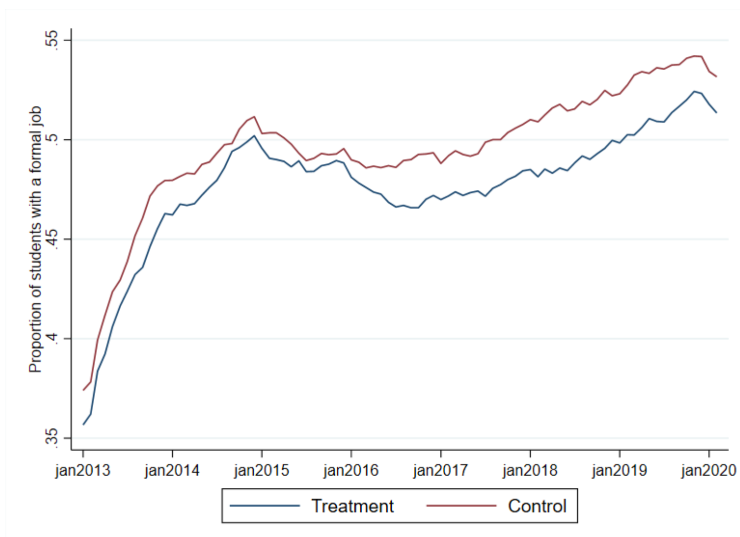
Notes: This figure shows the proportion of students in our sample for whom the longest repayment delay is greater than a year, when credit contracts get written off as a loss, using monthly administrative data from the Credit Registry System (SCR), housed at the Central Bank of Brazil (BCB). Here, we compute the longest repayment delay after dropping credit contracts that were already a loss in June 2016.

Figure 5: Students with Formal Microenterprises over Time



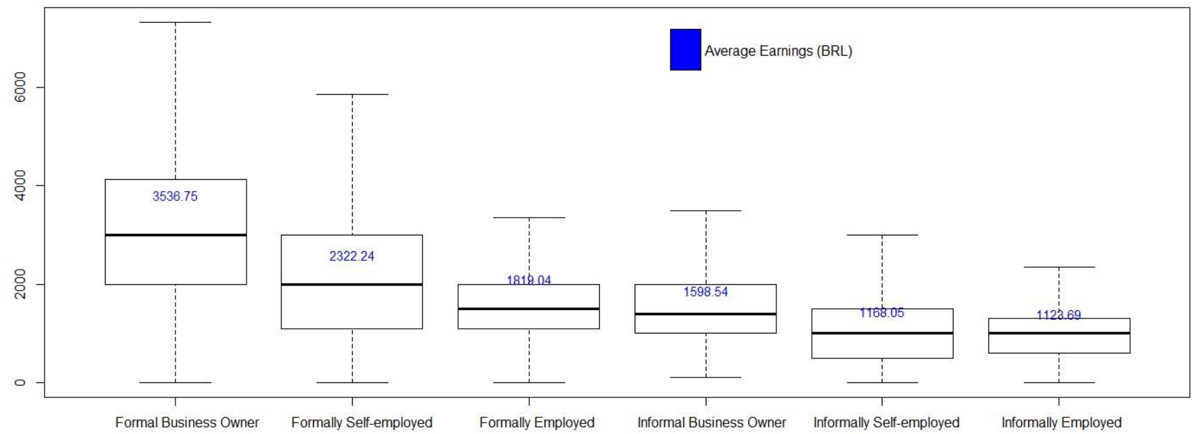
Notes: This figure shows the proportion of students in our sample who own an individual microentrepreneur firm (MEI), using monthly administrative data from the SRF registry of firms, housed at the Central Bank of Brazil (BCB). Data before April 2015 is omitted from the graph for confidentiality reasons since it contains less than 100 individuals with an MEI in each group.

Figure 6: Formally Employed Students over Time



Notes: This figure shows the proportion of students in our sample that have a formal employment relationship, using monthly administrative data from RAIS, housed at the Central Bank of Brazil (BCB).

Figure 7: Earnings of Formal and Informal Business Owners and Employees



Notes: This figure shows monthly earnings in the fourth quarter of 2019 from the Continuous National Household Sample Survey (PNAD), for individuals aged 23 to 25 with at least high school education (corresponding to the characteristics of the students in our sample). The bold line represents the median income for each occupation group and the box lines represent the 25th and 75th percentiles. The lower and upper lines are the minimum and maximum earnings. The minimum is truncated at 0 and the maximum is computed as the 75th percentile plus 1.5 times the interquartile distance (outliers are dropped).

Table 1 - Baseline Summary Statistics for Long-Term IE Sample

	Number of schools	Number of students	Control Mean	Control SD	Treatment Mean	Treatment SD	Difference in Means Test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: School-level variables (administrative data)							
Number of students in school (2008)	886		642.59	462.08	680.92	515.91	0.245
Number of teachers in school (2008)	764		37.53	24.15	38.40	25.80	0.633
Grade-level dropout rate (2009)	876		11.08	11.23	11.71	11.74	0.420
Grade-level passing rate (2009)	876		68.02	15.98	67.86	15.94	0.878
Panel B: 2010 baseline survey data							
Student is female	886	15,925	0.54		0.56		0.034 **
Mother attended secondary school	886	15,710	0.47		0.46		0.413
Father attended secondary school	884	15,641	0.43		0.43		0.697
Student has failed at least one school year	886	15,667	0.27		0.29		0.283
Student's family receives *Bolsa Familia* cash transfe	886	15,828	0.31		0.34		0.157
Student has computer with Internet at home	886	15,704	0.50		0.51		0.423
Student has some form of income	883	14,969	0.64		0.66		0.077 *
Student is not working	883	14,966	0.32		0.32		0.720
Financial proficiency score	886	15,939	50.73	15.02	51.25	14.74	0.277
Saves money for future purchases	883	14,788	0.15		0.15		0.632
Intention to save index	883	14,096	48.86	18.80	48.67	18.39	0.606
Makes a list of expenses every month	883	14,900	0.10		0.09		0.909
Negotiates prices or payment methods	883	14,780	0.76		0.76		0.898
Financial autonomy index	883	14,064	48.90	19.61	48.88	19.22	0.952

Notes: This table presents summary statistics from 2008 or 2009 administrative data and the 2010 baseline survey, for the long-term impact evaluation sample. This sample includes only students for whom we found a CPF (taxpayer identification number) based on name and age matching with administrative data from the Brazilian tax authority (SRF). *** p<0.01, ** p<0.05, * p<0.1.

Table 2: Timeline

Year	'08	'09	'10	'11	'12	'13	'14	'15	'16	'17	'18	'19	'20
Intervention			X	X									
Baseline			X										
Follow-up 1			X										
Follow-up 2				X									
CCS financial accounts data	X	X	X	X	X	X	X	X	X	X	X	X	X
SCR credit data									X	X	X	X	X
RAIS employment data						X	X	X	X	X	X	X	X
MEI entrepreneurship data					X	X	X	X	X	X	X	X	X
Average student age	14	15	16	17	18	19	20	21	22	23	24	25	26

Notes: Baseline, follow-up 1 and follow-up 2 refer to the short-term impact evaluation.

Table 3 - Long-Term Effects on Holding Accounts at Financial Institutions

	Has an account at a financial institution	
	Pre-intervention (2008 and 2009)	Post-intervention (2012 to 2020)
	(1)	(2)
Panel A - No controls		
Treatment school	-0.00344	-0.00615
	(0.00403)	(0.00580)
R^2	0.014	0.154
Panel B - With school pair dummies		
Treatment school	-0.00458	-0.00271
	(0.00298)	(0.00378)
R^2	0.044	0.186
Panel C - With school pair dummies and student gender		
Treatment school	-0.00417	-0.00183
	(0.00298)	(0.00378)
R^2	0.046	0.191
Panel D - With school pair dummies, student gender and time varying treatment effects		
Treatment school x (2012 to 2018)		-0.00243
		(0.00411)
Treatment school x (2019 to 2020)		0.00179
		(0.00429)
R2		0.191
F-test p-value (effect equal in both periods)		0.3820
Observations (students x month)	382,560	1,562,120
Number of students	15,940	15,940
Number of months	24	98
Number of schools	886	886
Dependent variable mean in control group		
Full sample	0.069	0.847
2012 to 2018		0.828
2019 to 2020		0.959

Notes: This table presents OLS regression results for the impact of the financial education program on holding accounts at financial institutions. Column 1 shows results for the pre-intervention period (2008 and 2009). Column 2 shows results for the post-intervention period (2012 to 2020). The outcome variable is an indicator variable equal to 1 if the student has an account at a financial institution, according to administrative data from the Registry of Clients of the Financial System (CCS), housed at the Central Bank of Brazil (BCB). Panel A presents regressions of the outcome on a treatment dummy, controlling for state by month fixed effects. Panel B controls additionally for school pair dummies, and panel C controls further for student gender. Panel D breaks the treatment effects into two time periods: 2012 to 2018, when students may still be in university, and 2019 and 2020, when most students have entered the labor market, using all controls. 2020 data includes only January and February (pre COVID-19 pandemic in Brazil). Robust standard errors, clustered at the school level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 4 - Long-Term Effects on Credit Usage

	Any type of credit	Credit card purchases	Credit card debt	Overdrafts	Non-payroll	Auto	Payroll
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A - No controls							
Treatment school	-0.0187***	-0.0163**	-0.0135**	-0.00948***	-0.00271	-0.00161	-0.00272
	(0.00670)	(0.00656)	(0.00526)	(0.00346)	(0.00264)	(0.00286)	(0.00244)
R^2	0.020	0.018	0.005	0.015	0.003	0.006	0.003
Panel B - With school pair dummies							
Treatment school	-0.0206***	-0.0181***	-0.0142***	-0.00964***	-0.00321	-0.00279	-0.00191
	(0.00493)	(0.00488)	(0.00350)	(0.00248)	(0.00203)	(0.00218)	(0.00174)
R^2	0.040	0.039	0.025	0.030	0.015	0.023	0.024
Panel C - With school pair dummies and student gender							
Treatment school	-0.0196***	-0.0175***	-0.0142***	-0.00900***	-0.00294	-0.00215	-0.00172
	(0.00494)	(0.00490)	(0.00351)	(0.00247)	(0.00203)	(0.00218)	(0.00174)
R^2	0.044	0.041	0.025	0.034	0.017	0.030	0.025
Panel D - With school pair dummies, student gender and time varying treatment effects							
Treatment school x (2016 to 2018)	-0.0208***	-0.0200***	-0.0143***	-0.00874***	-0.00175	-0.00243	-0.00231
	(0.00527)	(0.00510)	(0.00398)	(0.00269)	(0.00218)	(0.00242)	(0.00179)
Treatment school x (2019 to 2020)	-0.0171**	-0.0121*	-0.0139***	-0.00957**	-0.00556*	-0.00153	-0.000413
	(0.00673)	(0.00676)	(0.00465)	(0.00382)	(0.00310)	(0.00286)	(0.00243)
R2	0.044	0.041	0.025	0.034	0.017	0.030	0.025
F-test p-value (effect equal in both periods)	0.562	0.205	0.924	0.834	0.229	0.760	0.382
Observations (student x month)	717,300	717,300	717,300	717,300	717,300	717,300	717,300
Number of students	15,940	15,940	15,940	15,940	15,940	15,940	15,940
Number of months	45	45	45	45	45	45	45
Number of schools	886	886	886	886	886	886	886
Dependent variable mean in control group							
Full sample	0.478	0.344	0.230	0.111	0.061	0.054	0.036
2016 to 2018	0.449	0.315	0.225	0.101	0.055	0.051	0.033
2019 to 2020	0.543	0.406	0.242	0.133	0.074	0.061	0.043

Notes: This table presents OLS regression results for the impact of the financial education program on using credit. The outcome variables come from administrative data from the Credit Registry System (SCR), housed at the Brazilian Central Bank (BCB), and are defined as follows: an indicator variable equal to 1 if the student has a positive balance for any type of credit (column 1); an indicator variable equal to 1 if the student has a positive balance in credit card purchases (column 2); an indicator variable equal to 1 if the student has a positive balance in revolving credit card credit or interest paying installment plans (column 3); an indicator variable equal to 1 if the student has a positive balance in overdrafts (column 4); an indicator variable equal to 1 if the student has a positive balance in non-payroll loans (column 5); an indicator variable equal to 1 if the student has a positive balance in auto loans (column 6); and an indicator variable equal to 1 if the student has a positive balance in payroll loans (column 7). Panel A presents regressions of the outcome on a treatment dummy, controlling for state by month fixed effects. Panel B controls additionally for school pair dummies, and panel C controls further for student gender. Panel D breaks the treatment effects into two time periods: 2016 to 2018, when students may still be in university, and 2019 and 2020, when most students have entered the labor market, using all controls. 2020 data includes only January and February (pre COVID-19 pandemic in Brazil). Robust standard errors, clustered at the school level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 5 - Long-Term Effects on Default

	All credit contracts		Without credit contracts that were a loss in June 2016	
	Any delay, but not loss	Any loss	Any delay, but not loss	Any loss
	(1)	(2)	(3)	(4)
Panel A - No controls				
Treatment school	-0.00785***	0.0105	-0.00762**	-0.00174
	(0.00289)	(0.00736)	(0.00315)	(0.00229)
R^2	0.002	0.005	0.003	0.036
Panel B - With school pair dummies				
Treatment school	-0.00874***	0.0134**	-0.00850***	-0.00210
	(0.00219)	(0.00543)	(0.00238)	(0.00169)
R^2	0.010	0.039	0.011	0.048
Panel C - With school pair dummies and student gender				
Treatment school	-0.00850***	0.0130**	-0.00824***	-0.00200
	(0.00218)	(0.00545)	(0.00237)	(0.00169)
R^2	0.010	0.040	0.012	0.048
Panel D - With school pair dummies, student gender and time varying treatment effects				
Treatment school x (2016 to 2018)	-0.00739***	0.0149**	-0.00756***	-0.000146
	(0.00263)	(0.00577)	(0.00288)	(0.00155)
Treatment school x (2019 to 2020)	-0.0110***	0.00903	-0.00974***	-0.00611*
	(0.00340)	(0.00606)	(0.00368)	(0.00361)
R2	0.010	0.040	0.012	0.048
	0.38	0.212	0.625	0.103
F-test p-value (effect equal in both periods)				
Observations (student x month)	717,300	717,300	717,300	717,300
Number of students	15,940	15,940	15,940	15,940
Number of months	45	45	45	45
Number of schools	886	886	886	886
Dependent variable mean in control group				
Full sample	0.130	0.232	0.150	0.053
2016 to 2018	0.128	0.225	0.149	0.029
2019 to 2020	0.137	0.247	0.153	0.105

Notes: This table presents OLS regression results for the impact of the financial education program on repayment delays and credit losses. The outcome variables come from administrative data from the Credit Registry System (SCR), housed at the Brazilian Central Bank (BCB), and are defined as follows: an indicator variable equal to 1 if the student's longest repayment delay is between one day and one year (columns 1 and 3); an indicator variable equal to 1 if the student's longest repayment delay is greater than a year, when credit contracts get written off as a loss (columns 2 and 4). Columns 1 and 2 consider all credit contracts in the system since June 2016, including previously realized losses which stay in the system. For columns 3 and 4, we re-compute the outcome variables after dropping credit contracts that were already a loss in June 2016. Panel A presents regressions of the outcome on a treatment dummy, controlling for state by month fixed effects. Panel B controls additionally for school pair dummies, and panel C controls further for student gender. Panel D breaks the treatment effects into two time periods: 2016 to 2018, when students may still be in university, and 2019 and 2020, when most students have entered the labor market, using all controls. 2020 data includes only January and February (pre COVID-19 pandemic in Brazil). Robust standard errors, clustered at the school level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 6 - Long-Term Effects on Microenterprise Ownership, Formal Employment and Informality Proxy

	Owns microenterprise (MEI)	Formally employed	Informal proxy
	(1)	(2)	(3)
Panel A - No controls			
Treatment school	0.00171	-0.0177**	
	(0.00170)	(0.00711)	
R^2	0.022	0.016	
Panel B - With school pair dummies			
Treatment school	0.00171	-0.0140***	
	(0.00135)	(0.00475)	
R^2	0.034	0.041	
Panel C - With school pair dummies and student gender			
Treatment school	0.00181	-0.0122**	
	(0.00135)	(0.00479)	
R^2	0.034	0.052	
Panel D - With school pair dummies, student gender and time varying treatment effects			
Treatment school x (2012 to 2018)	0.000970	-0.0112**	
	(0.00126)	(0.00495)	
Treatment school x (2019 to 2020)	0.00689**	-0.0173***	0.0106*
	(0.00349)	(0.00659)	(0.00588)
R^2	0.034	0.052	0.036
F-test p-value (effect equal in both periods)	0.065	0.3030	
Observations (student x month)	1,562,120	1,370,840	15,940
Number of students	15,940	15,940	15,940
Number of months	98	86	
Number of schools	886	886	886
Dependent variable mean in control group			
Full sample	0.027	0.495	
2012 to 2018	0.020	0.488	
2019 to 2020	0.069	0.535	0.255

Notes: This table presents OLS regression results for the impact of the financial education program on labor market outcomes. The outcome variables come from administrative data housed at the BCB, and are defined as follows: an indicator variable equal to 1 if the student owns an individual microentrepreneur firm (MEI) according to the SRF registry of firms (column 1); an indicator variable equal to 1 if the student has a formal employment relationship according to RAIS (column 2); and an indicator variable equal to 1 if the student is classified as informally occupied using the proxy calculation (column 3) . Panel A presents regressions of the outcome on a treatment dummy, controlling for state by month fixed effects. Panel B controls additionally for school pair dummies, and panel C controls further for student gender. Panel D breaks the treatment effects into two time periods: 2012 to 2018, when students may still be in university, and 2019 and 2020, when most students have entered the labor market, using all controls. 2020 data includes only January and February (pre COVID-19 pandemic in Brazil). For the informal proxy, only a cross-section, based on 2019 and 2020 data is available. Robust standard errors, clustered at the school level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Table A1 - Short-Term Effects on Students' Financial Proficiency by Sample

Table A1 - Short-Term Effects on Students' Financial Proficiency by Sample

	Financial proficiency score		Saves at least some of their disposable money		Has borrowed money from any source		Is behind on any repayments	
	Follow-up 1	Follow-up 2	Follow-up 1	Follow-up 2	Follow-up 1	Follow-up 2	Follow-up 1	Follow-up 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Short-term IE sample								
Treatment school	3.793***	3.049***	0.050***	0.052***	0.037***	0.029***	0.006	0.005
	(0.299)	(0.352)	(0.006)	(0.007)	(0.006)	(0.006)	(0.004)	(0.005)
R^2	0.449	0.318	0.212	0.110	0.210	0.099	0.164	0.068
N	18,276	18,953	16,660	17,843	16,503	17,135	16,336	17,959
Number of schools	852	847	845	845	844	845	844	845
Dep. var. mean in control group	56.050	59.045	0.440	0.403	0.304	0.288	0.094	0.121
Dep.var. SD in control group	14,808	14,866	0.496	0.490	0.460	0.453	0.292	0.326
Panel B. Long-term IE sample								
Treatment school	4.173***	3.770***	0.064***	0.062***	0.034***	0.020**	0.007	0.003
	(0.320)	(0.432)	(0.008)	(0.011)	(0.007)	(0.009)	(0.005)	(0.006)
R^2	0.494	0.436	0.235	0.158	0.233	0.143	0.158	0.092
N	10,776	7,859	9,909	7,417	9,809	7,116	9,717	7,456
Number of schools	841	783	828	782	828	784	828	784
Dep. var. mean in control group	57.195	59.915	0.433	0.411	0.296	0.271	0.079	0.109
Dep. Var. SD in control group	15,022	15,374	0.496	0.492	0.456	0.444	0.270	0.311

Notes: This table presents OLS regression results for the impact of the financial education program on student outcomes collected in the short-term follow-up surveys. Panel A replicates the results found in the original sample used in Bruhn et al. (2016). Panel B uses the sample of students for whom we found a CPF (taxpayer identification number) based on name and age matching with administrative data from the Brazilian tax authority (SRF). The number of students and schools included in each sample fluctuate across waves because of student turnover. The outcome variables are: a student financial proficiency score, which aggregates financial knowledge questions included in a survey on a 0-100 scale (columns 1 and 2), an indicator variable equal to 1 if the student saves at least some of their disposable money (columns 3 and 4), an indicator variable equal to 1 if the student has borrowed money from any source (columns 5 and 6); and an indicator variable equal to 1 if the student in behind on any repayments (columns 7 and 8). All regressions are cross-sectional and control for school pair dummies, the baseline value of the dependent variable as well as student gender. When baseline outcomes have missing values, they are replaced by zero and a dummy variable indicating such missing values is included. Robust standard errors, clustered at the school level, are in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A2 - Long-Term Effects on Credit Balance

	Any type of credit	Credit card purchases	Credit card debt	Overdrafts	Non-payroll	Auto	Payroll
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A - log(balance)							
Treatment school	-0.00980	-0.0238	-0.0178	-0.0177	-0.0531	0.0264	-0.0953*
	(0.0209)	(0.0212)	(0.0286)	(0.0386)	(0.0336)	(0.0307)	(0.0576)
R^2	0.075	0.073	0.036	0.058	0.182	0.223	0.306
Observations (student x month)	336,110	240,621	160,245	75,827	42,991	38,393	24,763
Number of students	12,539	10,555	9,475	5,575	3,217	1,796	1,275
Number of months	45	45	45	45	45	45	45
Number of schools	885	882	884	865	828	712	622
Dependent variable mean in control group	7,595	6,477	5,679	5,068	7,187	9,180	8,303
Panel B - log(1+balance)							
Treatment school	-0.150***	-0.119***	-0.0825***	-0.0445***	-0.0234	-0.0193	-0.0169
	(0.0410)	(0.0349)	(0.0218)	(0.0136)	(0.0149)	(0.0200)	(0.0147)
R^2	0.055	0.050	0.025	0.032	0.017	0.030	0.026
Observations (student x month)	717,300	717,300	717,300	717,300	717,300	717,300	717,300
Number of students	15,940	15,940	15,940	15,940	15,940	15,940	15,940
Number of months	45	45	45	45	45	45	45
Number of schools	886	886	886	886	886	886	886
Dependent variable mean in control group	3,632	2,233	1,325	.572	.440	.499	.298

Notes: This table presents OLS regression results for the impact of the financial education program on credit balances. The outcome variables come from administrative data from the Credit Registry System (SCR), housed at the Brazilian Central Bank (BCB) and are defined as follows: balance for any type of credit (column 1); balance in credit card purchases (column 2); balance in revolving credit card credit or interest paying installment plans (column 3); balance in overdrafts (column 4); balance in non-payroll loans (column 5); balance in auto loans (column 6); balance in payroll loans (column 7). Panel A presents regressions using the balances in logs, defined only for students with a positive balance. Panel B present the logs of balances plus 1 BRL, to measure the overall effect, including the extensive margin. Balances are inflation adjusted to December 2019 using the Broad Consumer Price Index from the National Institute of Statistics. Regression include state by month fixed effects. 2020 data includes only January and February (pre COVID-19 pandemic in Brazil). Robust standard errors, clustered at the school level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A3 - Robustness: Long-Term Effects on Credit Usage by Credit Losses in June 2016

	Any type of credit (1)	Credit card purchases (2)	Credit card debt (3)	Overdrafts (4)	Non-payroll (5)	Auto (6)	Payroll (7)
Treatment school	-0.0176***	-0.0149***	-0.0130***	-0.00848***	-0.00317	-0.00222	-0.00156
	(0.00550)	(0.00547)	(0.00422)	(0.00292)	(0.00239)	(0.00269)	(0.00216)
Loss in June 2016	-0.266***	-0.281***	-0.135***	-0.0407***	-0.0266***	-0.0456***	-0.00699*
	(0.00910)	(0.00729)	(0.00624)	(0.00525)	(0.00375)	(0.00382)	(0.00386)
Loss in June 2016 X treatment	0.00633	0.00448	0.00215	-3.61e-05	0.00279	0.00314	-0.000336
	(0.0129)	(0.0103)	(0.00900)	(0.00706)	(0.00550)	(0.00517)	(0.00553)
R^2	0.085	0.093	0.040	0.036	0.018	0.035	0.026
Observations (student x month)	717,300	717,300	717,300	717,300	717,300	717,300	717,300
Number of students	15940	15940	15940	15940	15940	15940	15940
Number of months	45	45	45	45	45	45	45
Number of schools	886	886	886	886	886	886	886
Dependent variable mean in control group	0.478	0.344	0.230	0.111	0.061	0.054	0.036

Notes: This table presents OLS regression results for the impact of the financial education program on using credit, with heterogeneous effects according to having losses in June 2016, the first year of our credit data. Those losses stem from operations granted in the previous years. The outcome variables come from administrative data from the Credit Registry System (SCR), housed at the Brazilian Central Bank (BCB), and are defined as follows: an indicator variable equal to 1 if the student has a positive balance for any type of credit (column 1); an indicator variable equal to 1 if the student has a positive balance in credit card purchases (column 2); an indicator variable equal to 1 if the student has a positive balance in revolving credit card credit or interest paying installment plans (column 3); an indicator variable equal to 1 if the student has a positive balance in overdrafts (column 4); an indicator variable equal to 1 if the student has a positive balance in non-payroll loans (column 5); an indicator variable equal to 1 if the student has a positive balance in auto loans (column 6); and an indicator variable equal to 1 if the student has a positive balance in payroll loans (column 7). The table presents regressions of the outcome on a treatment dummy, controlling for state by month fixed effects, school pair dummies and student gender. Robust standard errors, clustered at the school level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A4 - Robustness: Long-Term Effects on Credit Usage and Wages by Formally Employed Status

	Any type of credit (1)	Credit card purchases (2)	Credit card debt (3)	Overdrafts (4)	Non-payroll (5)	Auto (6)	Payroll (7)	log(wage) (8)
Treatment school	-0.0182***	-0.0158**	-0.0150***	-0.0104**	-0.00258	-0.00320	-0.00143	-0.00532
	(0.00672)	(0.00690)	(0.00546)	(0.00412)	(0.00349)	(0.00369)	(0.00350)	(0.00761)
Not formally employed	-0.235***	-0.198***	-0.0954***	-0.0767***	-0.0547***	-0.0402***	-0.0566***	
	(0.00662)	(0.00629)	(0.00532)	(0.00381)	(0.00305)	(0.00344)	(0.00283)	
Not formally employed X treatment	0.00563	0.00374	0.00500	0.00551	0.00127	0.00351	0.00147	
	(0.00958)	(0.00928)	(0.00763)	(0.00550)	(0.00450)	(0.00488)	(0.00437)	
R^2	0.095	0.082	0.037	0.048	0.029	0.037	0.048	0.162
Observations (student x month)	717,300	717,300	717,300	717,300	717,300	717,300	717,300	326,487
Number of students	15940	15940	15940	15940	15940	15940	15940	11,553
Number of months	45	45	45	45	45	45	45	45
Number of schools	886	886	886	886	886	886	886	885
Dependent variable mean in control group	0.478	0.344	0.230	0.111	0.061	0.054	0.036	0.586

Notes: This table presents OLS regression results for the impact of the financial education program on using credit and on log wages. It includes heterogeneous effects for students not formally employed (according to RAIS). The outcome variables come from administrative data from the Credit Registry System (SCR), housed at the Brazilian Central Bank (BCB), and from RAIS, and are defined as follows: an indicator variable equal to 1 if the student has a positive balance for any type of credit (column 1); an indicator variable equal to 1 if the student has a positive balance in credit card purchases (column 2); an indicator variable equal to 1 if the student has a positive balance in revolving credit card credit or interest paying installment plans (column 3); an indicator variable equal to 1 if the student has a positive balance in overdrafts (column 4); an indicator variable equal to 1 if the student has a positive balance in non-payroll loans (column 5); an indicator variable equal to 1 if the student has a positive balance in auto loans (column 6); and an indicator variable equal to 1 if the student has a positive balance in payroll loans (column 7); the logarithm of the student´s wage in minimum wages (column 8). Column 8 has fewer observations because only formally employed students are included. Also, for less than 10 percent of the observations, wages are missing or reported as zero. The table presents regressions of the outcome on a treatment dummy, controlling for state by month fixed effects, for school pair dummies, and for student gender. 2020 data includes only January and February (pre COVID-19 pandemic in Brazil). Robust standard errors, clustered at the school level, are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.