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

Miriam Bruhn, Gabriel Garber, Sergio Koyama, and Bilal Zia*

**Abstract**

In 2011, we studied the impact of a comprehensive high school financial education program through a randomized control trial with 892 schools in six Brazilian states (Bruhn et al 2016). Using administrative data from multiple sources, this paper follows about 16,000 students for the next nine years and finds two main results. First, contrary to the short-term findings of students using expensive credit, the long-term results show treated students are significantly less likely to borrow from expensive sources than control students. Second, treated students are less likely to be formally employed and more likely to own microenterprises than control students.

**JEL Codes:** D14, D91, I25, O12
**Keywords:** Financial Education, Financial Literacy, Youth Economic Development, Brazil

*The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the Central Bank of Brazil or of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.*

* Bruhn and Zia are from the World Bank, mbruhn@worldbank.org and bzia@worldbank.org, respectively. Garber and Koyama are from the Central Bank of Brazil, gabriel.garber@bcb.gov.br and sergio.koyama@bcb.gov.br, respectively. We thank Jose Alexandre Vasco and Rogerio Vicentin Ferraz de Oliveira from CVM, Claudia Forte and Thiago Nascimento from AEF, Caio Piza from the World Bank and Cristina Oliveira Roriz, Mirena Gentilezza de Figueiredo Brito, Elisangela Batista Vieira Salgueiro Dias, Fabio de Almeida Lopes Araujo, Paulo Jose de Castro Jatahy, Rodrigo Augusto Silva de Andrade and Thiago Almeida Franco from BCB for their support and comments. Laís Camargo Bartalotti provided excellent research assistance.

## 1. Introduction

At least 64 countries around the world are implementing or planning national strategies for financial education (OECD 2015). 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 2021; Lührmann et al. 2018). 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).

While the literature on school financial education has expanded during recent years, we still know very little about the effects of these programs on long-term financial behavior. A meta-analysis of 37 studies measuring the effect of school financial education programs for children and youth concludes that the programs had 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 of financial education, less than a year following the intervention. In fact, the metanalysis suggests that the effects of school financial education decline over time (in a timeframe of up to 80 weeks), which may be because people forget what they were taught (see also Fernandes et al. 2014).[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).

This paper starts to fill the evidence gap by studying the long-term effects of a comprehensive high-school financial education program in Brazil. 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 control trial spanning 868 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 control trial graduated 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) showed increased financial knowledge, as well as positive effects on savings attitudes, self-reported saving up for purchases, money management, and budgeting. On the flip side, 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

[1] An exception is Frisancho (2021), 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.

actively discourage their use. 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.

We now study the effects of this same high-school financial education program on students' financial and labor market outcomes, up to nine years after the program ended (and students graduated high school).

Based on students' name and age, we obtained the taxpayer identification number (CPF) for nearly 16,000 students in the original sample, which included close to 35,000 students in total (although not more than 25,000 students in any given survey). The percentage of students for whom we found a CPF is 44.27 percent in the treatment group and 46.02 percent in the control group. The small difference in percentage of CPFs found across the treatment and control group is statistically significant at the 10 percent level. Nevertheless, the baseline characteristics from the 2010 survey are balanced across the treatment and control groups in the sample of 16,000 student with a CPF. We then use the CPF to consult administrative data housed at the Central Bank of Brazil (BCB). This data includes bank account ownership (but not account balances), use of various credit products, as well as information on formal employment status and formal microenterprise ownership. We follow students until February 2020, just before the COVID-19 pandemic hit Brazil.[2] 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 control trial, and to jointly analyze all the administrative datasets used.

Relying on the comparison of the randomized treatment and control group, we find no effect of the high-school financial education program on long-run bank account ownership, but a high percentage of students (85 percent) have a bank account after graduating high school. However, treatment students are significantly less likely to use credit than control students, particularly in the most expensive credit categories: credit cards and overdrafts. Treatment students are 1.4 percentage point less likely to have credit card debt and 0.9 percentage points 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 five to seven years after the program.

Interestingly, the long-run effect on using expensive sources of credit is the opposite as the short-run effect found in Bruhn et al (2016). That is, in the short-run treated students were more likely to use expensive sources of credit than control students, while treated students are less likely than control students to use these sources in the long-run. It could be that students experimented with expensive credit early and then realized that this was not a sound financial decision. It could also be 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. Finally, while we use administrative data on credit here, Bruhn et al (2016) relied on self-reported data, which may have been subject to bias.

[2] 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 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, which could potentially explain their lower use of credit. We do not believe that the difference in formal job holding is driving the credit results since the results are similar when we restrict the sample to those who have a formal job. The credit results may thus be driven by improved budget planning skills or control over consumption impulses.

The difference in formal job holdings could imply that treatment students are more likely to be business owners. Alternatively, treatment students may be more likely to be studying in university instead of working. We do not have data on university enrollment. However, nine years after graduating high school, most students would have entered the labor market, even if they went to university. In Brazil, students tend to start university one or two years after graduating high school (because students frequently use this time to study for entrance exams) and take between four and eight years to graduate.

We have data on one type of formal business owners: those with up to one employee in a special tax regime (MEI – individual microentrepreneurs), which covers about 42 percent of all registered businesses. 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 likely in the labor market. Here, we find that treatment students were not more likely to be microentrepreneurs than control students one to seven years after graduating, but eight to nine years after graduating, treatment students were about 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 a MEI).

We also examine a proxy for informal employment derived from data on a COVID-19 pandemic transfer distributed 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).

We thus observe that the high-school financial education program had effects on both long-run financial behavior, i.e., credit use, and employment outcomes. The observed 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 six 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

This paper proceeds as follows. Section 2 summarizes the short-term impact evaluation design and results. Section 3 discusses the rationale for a long-term impact evaluation, as well as the long-term sample, data sources, and timeline. Section 5 presents the long-term impact evaluation results. Section 6 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 $11^{th}$ and $12^{th}$ 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 who are primed to absorb, recall, and apply learning on a regular basis. Finally, well-informed students can modify not only their own financial choices, but also to act as agents of change in their households' financial decisions.

We tested short-term answers to these questions through a randomized control trial of a comprehensive financial education program for public high school students in Brazil. The program spanned a 17-month period across the 2010 and 2011 academic years, and was integrated in classroom curricula of Mathematics, Science, History, and Portuguese. The instruction used new textbooks with interactive and repeated 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 economy, and the world economy. Appendix 1 in Bruhn et al. (2016) provides a detailed list of topics within each theme.

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 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 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:

$$y_{i,s,f} = \alpha + \beta \text{Treatment}_{i,s} + \sum \gamma_s d_s + \delta y_{i,s,b} + \eta f_{i,s} + \varepsilon_{i,s} \quad (1)$$

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 $\text{Treatment}_{i,s}$ 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 a 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. Treated 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, treated 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, our short-term results 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.

For the long-term impact evaluation, we rely on administrative data sources, housed at 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 2012). Additionally, the administrative data are provided by authorized third parties, meaning that they do not suffer from self-reporting bias.

### *3.b. Selecting the Sample 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 BCB, we need their CPF (the 11-digit Brazilian taxpayer identification number).

Student 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 currently 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 collected CPFs for 10,846 students. From these, 8,093 are valid numbers, in that they are consistent with the algorithm for generating a CPF.

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 and since the CPFs collected during the short-term impact evaluation are missing for 77 percent of the sample, we implemented an algorithm to find 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 hold a CPF and are therefore present in the SRF registry. The number is necessary to open a bank account, have a formal job, pay taxes, etc. Even relatively poor individuals commonly have a CPF if they are 18 years or older.[3]

Two main caveats arise from attempting to obtain a CPF based on collected 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 (referring to the last update) 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 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. Out of these 15,940 students, 3,657 had reported a CPF during the short-run impact evaluation. Comparing these CPFs with the

[3] The Cadastro Único registry for December 2019, which includes poorer families representing around a third of the Brazilian population, lists a CPF for 96.2% of individuals who are age 18 years or older.

one found by the algorithm shows that they are almost always the same (only 2.68 percent are different), suggesting that the algorithm does a good job at identifying correct CPFs.[4]

### *3.c. Attrition and Balance of Baseline Characteristics*

The percentage of students in the starting sample for whom the algorithm found a CPF in the SRF data is 44.27 percent in the treatment group and 46.02 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.

We follow Bruhn et al. (2016) in examining the balance of student and school characteristics across the treatment and control groups, using the baseline data from the short-term impact evaluation (table 1). As in Bruhn et al. (2016) we find that the percentage of female students is statistically significantly higher in the treatment group than 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 "Receives income," but this difference is only significant at the 10 percent level.

In table A1, we check if the identified sample shows similar short-run effects of the intervention as the original sample. The effect of treatment on financial proficiency scores is positive and statistically significant in both samples. The point estimates are slightly higher in the long-term impact evaluation sample, but just like in the short-term sample they decrease moderately between follow-up 1 and 2 and as more controls are included in the regressions.

### *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[5] administrative datasets housed at the BCB. This section briefly describes the information available in each of these datasets. A caveat is that most of the administrative data cover only formal financial and employment relationships. Section 4.c 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

[4] 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 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 the students uniquely identified through name and age matching in the analysis.

[5] All individually identified information was handled exclusively by BCB staff.

in Brazil age 15 or older had a bank account.[6] 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) receives monthly information about every financial transaction from clients who expose the lender to a loss greater than a given amount. This threshold was of BRL 5,000 from 2003 until 2012, when it fell to BRL 1,000. It was reduced again in June 2016, to BRL 200 (currently corresponding to around USD 42). 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. In December 2019, the SCR contained information on over 127 million individuals (around 60 percent of the Brazilian population).

Third, we use data on formal employment from RAIS,[7] 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 BCB, starting in 2013 and ending in 2019. In December 2019, RAIS included nearly 51 million employees.

Fourth, we build a dataset 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.[8] 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.

### *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

[6] Available in Portuguese at https://www.bcb.gov.br/content/cidadaniafinanceira/Documents/RIF/Relatorio%20Cidadania%20Financeira_BCB_16jan_2019.pdf

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

[8] 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.

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:

$$y_{i,s,r,t} = \alpha + \beta \text{Treatment}_{i,s,r} + \sum \gamma_{s,r} d_{s,r} + \eta f_{i,s,r} + \sum \theta_{rt} m_{rt} + \varepsilon_{i,s,r,t} \quad (2)$$

where $y_{i,s,t}$ is a financial or labor market outcome of student $i$ in school pair $s$, located in state $r$, in month $t$. The variables $\text{Treatment}_{i,s,r}$, $d_{s,r}$, $f_{i,s,r}$, and $\varepsilon_{i,s,r,t}$ 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, $d_{s,r}$, and (iii) 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, which corresponds to the years when students may still be in university, and (ii) 2019 to 2020, when most students have entered the labor market.

### *4.a. Financial Inclusion and Behavior*

Table 3 presents the effect of the financial education program on holding accounts at financial institutions. Since account ownership is the only variable for which we have comprehensive pre-intervention data, we also include a regression using only pre-intervention years to test for baseline balance in the administrative data (column 1). Only 6.9 percent of students had an account at a financial institution in the pre-intervention period and this number was not statistically significantly different across the treatment and control group. In the post intervention period, account ownership was much higher, at 84.7 percent, but it was still not statistically different across the treatment and control groups (column 2). Figure 1 plots the proportion of students holding accounts over time.

In table 4, we examine the effect of the financial education program on credit usage. Column 1 shows 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).[9] For credit cards, we consider both having any positive credit card balance not yielding interest[10] (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). Panel D in table 4 shows that 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. Figure 2 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. Revolving credit card loans or overdrafts from the largest institutions carry between a 170 and 420 annual percent rate.[11] Treatment students are 5 percent less likely to have credit cards purchases than control students (a 1.75 percentage point decrease compared to the control group mean of 34.4 percent).

The results in the following section (4.b) show that treatment group students are less likely to hold a formal job, i.e., a job with a written contract, which could potentially explain their lower use of credit. However, we do not believe this to be the case since the results in table 4 are similar when we consider only the sample of students who have a formal job, according to RAIS, as shown in table A2 in the appendix. In column 8 of table A2, 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.

### *4.b. Employment*

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. Table 5 shows the effects of the program on two variables: (i) a variable that indicates that the individual is formally employed and (ii) a variable that indicates that the individual owns a MEI firm. The treatment group has a statistically significant 1.2 percentage point 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. A reason for this result could be that treatment students are more likely to go to university than control students. We do not have data on university enrollment to test this hypothesis. However, eight to nine years after graduating high school, most students would have entered the labor market, even if they went to university. In Brazil, students tend to start university

[9] Garber et al. (2019) provide stylized facts on the use of credit by individuals in Brazil.
[10] 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.
[11] BCB, Relatório de taxa de juros, March 2019.

one or two years after graduating high school (because students frequently use this time to study for entrance exams) and take between 4 and 8 years to graduate. Thus, while increased university enrollment may explain the negative effect of the financial education program during the first few years, it is unlikely to explain this effect 8 or 9 years after the program ended. Moreover, figure 3 suggests that the difference in formal employment emerges from 2016 onwards, five years after students graduated high school, when those who went to university would likely have been in university for several years already and some were already finishing their studies.

The negative effect on formal employment could also be due to students setting up their own business instead of becoming employees. Column 2 of table 5 shows that the intervention had no statistically significant effect on owning a MEI during the first seven years after the intervention. However, eight and nine years after the after the intervention, when students had likely graduated university, treatment students were statistically significantly more likely to own a 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 a MEI. Figure 4 also shows that the difference between the proportion of treatment and control students with a MEI increased over time.

Another reason for the lower probability of formal employment could be that treatment students are more likely than control students to be self-employed in 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 examine informal employment in the following subsection, using data from a COVID-19 pandemic transfer program. In Brazil, nearly two-thirds of businesses are informal (Ulyssea 2018) and around 42 percent of all firms in the SRF registry are MEIs. Between the MEI data examined in this subsection and the informal employment proxy in the following section, or data thus covers about 80 percent of businesses in Brazil.

### *4.c. The Covid-19 pandemic and a proxy of informal employment*

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.[12] 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.[13] 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 3 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

[12] Law 13.982 from April 2, 2020, article 2.
http://www.planalto.gov.br/ccivil_03/_ato2019-2022/2020/lei/l13982.htm#view
[13] Individuals who received unemployment insurance were not eligible for the transfer.

those that 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)[14] since these low-income individuals may be out of the labor force. The proxy thus includes informal employees (employees without a written contract who are not in RAIS), informal business owners (who did not register their business with the government) and the self-employed (individuals who do not have a registered business but file taxes under a self-employment regime).

Table 6 shows the effect of the financial education intervention on receiving the pandemic aid and on the informal proxy. Unlike the previous tables, the analysis here is done in a cross section, not a panel. The results indicate that once all controls are included, treatment students were 1.4 percentage points more likely to access the pandemic aid transfer than control students. About 38 percent of control students received pandemic aid transfers. Treatment students were also 1.1 percentage points more likely 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. Unfortunately, the data do not allow us to disentangle whether this increase comes from informal business owners or employees who are not registered with the government. However, given that the program lowered the share of students working as formal employees and increased the share of students running a formal microenterprise (MEI), we suspect that the increase is due to students running informal businesses.

## 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 close to 900 public high schools and administrative data on about 16,000 former students. Unlike the previous literature, including a paper on the short-run effects of the same program (Bruhn et al 2016), which has studied the effects of school financial education for up to one or two years after the programs ended, we follow students for up to nine years after leaving high school.

While some have argued 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 use of expensive sources of credit goes from being positive in the short-run to being negative in the long-

[14] 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.

run. That is, in the long-run treated students are less likely to use expensive sources of credit than control students, although the opposite was true in the short-run.

We also observe long-run effects on labor market outcomes. Students are less likely to work as formal employees and more likely to have formal microenterprises. These effects may be attributed to the fact that the financial education program was comprehensive and included modules on work and entrepreneurship.

**References**


Bruhn, M., Ibarra, G.L., and McKenzie, D. 2014. "The Minimal Impact of a Large-scale Financial Education Program in Mexico City." *Journal of Development Economics* 108: 184–189.

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

Chioda L, Contreras-Loya, D., Gertler, P., and Carney, D. 2021. "Making Entrepreneurs: Returns to Training Youth in Hard Versus Soft Business Skills." NBER Working Paper No. 28845.

Entorf, H. and Hou, J. 2018. "Financial Education for the Disadvantaged? A Review." SAFE Working Paper No. 205

Fernandes, D., Lynch, Jr., J.G., and Netemeyer, R.G. 2014. "Financial Literacy, Financial Education, and Downstream Financial Behaviors." *Management Science* 60(8): 1861–1883.

Frisancho, V. 2021. "Is School-Based Financial Education Effective? Immediate and Long-Lasting Impacts on Students and Teachers." Mimeo. Inter-American Development Bank.

Garber, G., Mian, A., Ponticelli, J. and A. Sufi. 2019. "Household Debt and Recession in Brazil". *Handbook of U.S. Consumer Economics*.

Kaiser, T. and Menkoff, L. 2020. "Financial Education in Schools: A Meta-Analysis of Experimental Studies." *Economics of Education Review* 78.

Lusardi, A., Mitchell, O. S., and Curto, V. 2010. "Financial Literacy among the Young." *Journal of Consumer Affairs* 44(2): 358–380.

Lührmann, M., Serra-Garcia, M., and Winter, J. 2018. "The Impact of Financial Education on Adolescents' Intertemporal Choices." *American Economic Journal: Economic Policy* 10(3), 309–332.

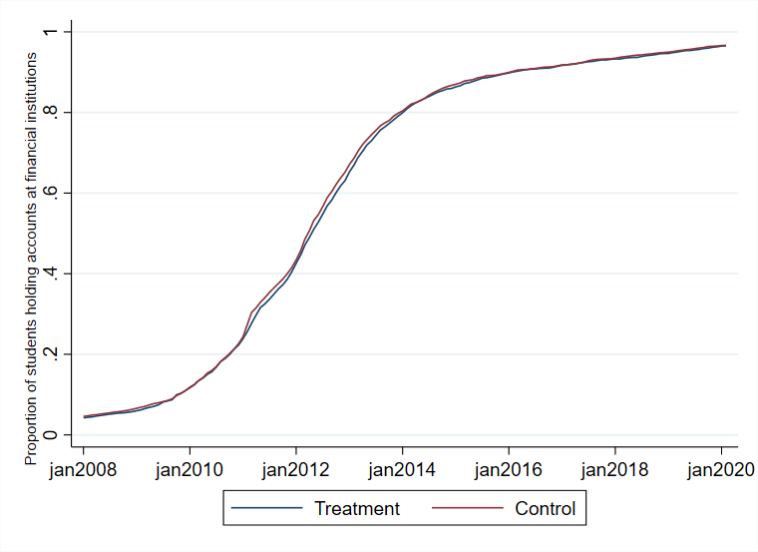
OECD. 2015. National Strategies for Financial Education. OECD/INFE Policy Handbook, https://www.oecd.org/finance/National-Strategies-Financial-Education-Policy-Handbook.pdf.

Ogden, Timothy. 2019. "More states are forcing students to study personal finance. It's a waste of time." The Washington Post. April 29.

https://www.washingtonpost.com/outlook/2019/04/23/more-states-are-forcing-students-study-personal-finance-its-waste-time/?noredirect=on&utm_term=.bf42822750ba

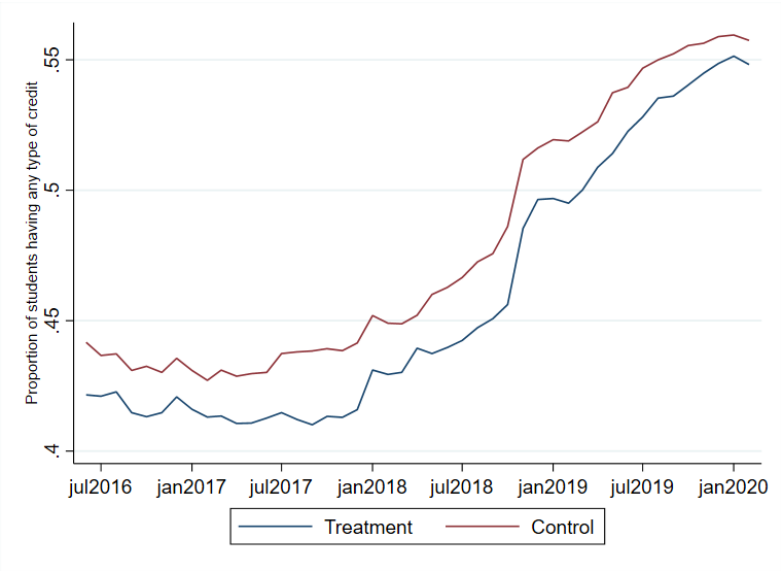
Ulyssea, Gabriel. 2018. “Firms, Informality, and Development: Theory and Evidence from Brazil.” *American Economic Review* 108(8): 2015–2047.

**Figure 1: Students Holding Accounts at Financial Institutions over Time**



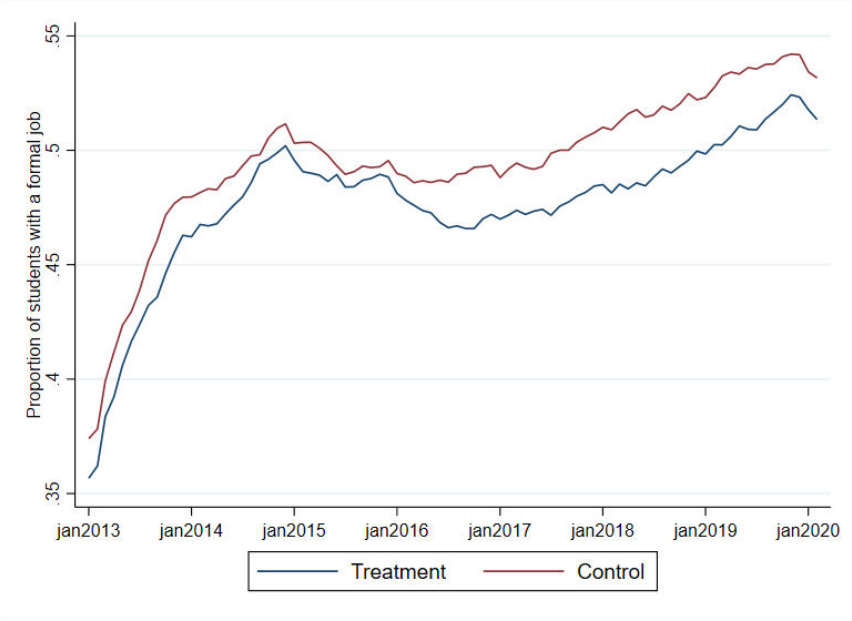

Notes: This figure shows the proportion of students in our sample that have an account with a financial institution for 2008 to 2020, using monthly administrative data from the Registry of Clients of the Financial System (CCS), housed at the Central Bank of Brazil (BCB).

**Figure 2: Credit Usage over Time**



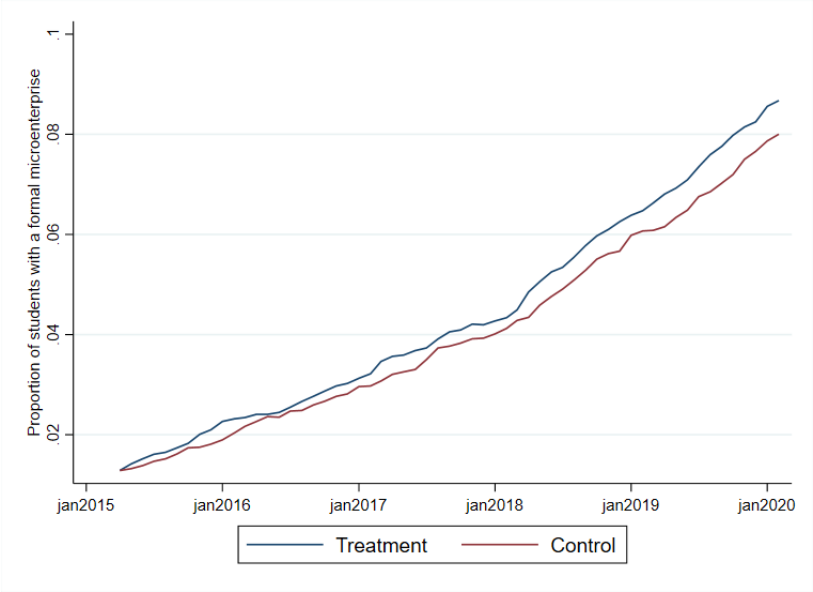

Notes: This figure shows the proportion of students in our sample that have positive credit balances, using monthly administrative data from the Credit Registry System (SCR), housed at the Central Bank of Brazil (BCB).

**Figure 3: 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 4: 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 a MEI in each group.

**Table 1 - Baseline Summary Statistics**

| | 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 transfer | 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 unemployed | 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 |

Note: 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* | -0.00458 | |
| Treatment school | (0.00298) | -0.00271 |
| | | (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 Brazilian Central Bank (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 |
| 2012 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 Formal Employment and Microenterprise Ownership**

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

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 has a formal employment relationship according to RAIS (column 1); an indicator variable equal to 1 if the student owns an individual microentrepreneur firm (MEI) according to the SRF registry of firms (column 2). 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 6 - Long-Term Effects on Informal Employment**

| | Pandemic aid (2020) | Informal proxy (2019) |
|---|---|---|
| | (1) | (2) |
| *Panel A - No controls* | | |
| Treatment school | 0.0185** | 0.0156** |
| | (0.00873) | (0.00756) |
| $R^2$ | 0.010 | 0.003 |
| *Panel B - With school pair dummies* | | |
| Treatment school | 0.0155** | 0.0112* |
| | (0.00649) | (0.00586) |
| $R^2$ | 0.046 | 0.035 |
| *Panel C - With school pair dummies and student gender* | | |
| Treatment school | 0.0137** | 0.0106* |
| | (0.00650) | (0.00588) |
| $R^2$ | 0.057 | 0.036 |
| Observations (students) | 15,940 | 15,940 |
| Number of schools | 886 | 886 |
| Dependent variable mean in control group | 0.383 | 0.255 |

Notes: This table presents cross-sectional OLS regression results for the impact of the financial education program on a proxy for being informally employed. The outcome variables are: an indicator variable equal to 1 if the student received a government pandemic aid transfer (Auxílio Emergencial) in 2020; and an indicator variable equal to 1 if the student is classified as informally employed in 2019 using a proxy calculation that sets the pandemic aid transfer indicator variable to zero if the student owned a formal microenterprise (MEI), was formally employed according to RAIS, or was a beneficiary of PBF cash transfers, indicating the student may have been out of the labor force (column 2). Panel A presents panel regressions of outcome on a treatment dummy, controlling for state fixed effects. Panel B controls additionally for school pair dummies, and panel C controls further for student gender. 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**

| | Financial Proficiency Score | | | |
|---|---|---|---|---|
| | Short-term IE sample | | Long-term IE sample | |
| | Follow-up 1 | Follow-up 2 | Follow-up 1 | Follow-up 2 |
| | (1) | (2) | (3) | (4) |
| *Panel A. No controls* | | | | |
| Treatment school | 4.266*** | 3.294*** | 4.744*** | 4.295*** |
| | (0.574) | (0.574) | (0.619) | (0.688) |
| $R^2$ | 0.021 | 0.012 | 0.026 | 0.020 |
| *Panel B. With school pair dummies* | | | | |
| Treatment school | 4.173*** | 3.237*** | 4.645*** | 4.134*** |
| | (0.396) | (0.394) | (0.433) | (0.525) |
| $R^2$ | 0.182 | 0.171 | 0.201 | 0.190 |
| *Panel C. With school pair dummies, baseline dependent variable, and student gender* | | | | |
| Treatment school | 3.793*** | 3.049*** | 4.173*** | 3.770*** |
| | (0.299) | (0.352) | (0.320) | (0.432) |
| $R^2$ | 0.449 | 0.318 | 0.494 | 0.436 |
| N | 18,276 | 18,953 | 10,776 | 7,859 |
| Number of schools | 852 | 847 | 841 | 783 |
| Dependent variable mean in control group | 56.050 | 59.045 | 57.195 | 59.915 |
| Dependent variable SD in control group | 14.808 | 14.866 | 15.022 | 15.374 |

Notes: This table presents OLS regression results for the impact of the financial education program on student financial proficiency. Columns 1 and 2 replicate the results found in the original sample used in Bruhn et al. (2016). Columns 3 and 4 use 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 variable is a student financial proficiency score, which aggregates financial knowledge questions included in a survey on a 0-100 scale. Panel A presents cross-sectional regressions of outcome on a treatment dummy with no controls. Panel B controls for school pair dummies, and panel C additionally controls for 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 Usage and Wages - Formally Employed Students**

| | Any type of credit | Credit card purchases | Credit card debt | Overdrafts | Non-payroll | Auto | Payroll | log(wage) |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Panel A - No controls* | | | | | | | | |
| Treatment school | -0.0162** | -0.0130 | -0.0136* | -0.0105** | -0.00202 | -0.00225 | -0.00267 | -0.0116 |
| | (0.00790) | (0.00825) | (0.00698) | (0.00517) | (0.00427) | (0.00452) | (0.00448) | (0.0109) |
| $R^2$ | 0.019 | 0.019 | 0.004 | 0.015 | 0.003 | 0.006 | 0.004 | 0.083 |
| *Panel B - With school pair dummies* | | | | | | | | |
| Treatment school | -0.0178*** | -0.0151** | -0.0143*** | -0.0115*** | -0.00295 | -0.00490 | -0.000574 | -0.00761 |
| | (0.00602) | (0.00635) | (0.00508) | (0.00378) | (0.00328) | (0.00343) | (0.00332) | (0.00758) |
| $R^2$ | 0.047 | 0.047 | 0.032 | 0.039 | 0.027 | 0.037 | 0.041 | 0.139 |
| *Panel C - With school pair dummies and student gender* | | | | | | | | |
| Treatment school | -0.0173*** | -0.0148** | -0.0144*** | -0.0111*** | -0.00278 | -0.00436 | -0.000418 | -0.00532 |
| | (0.00605) | (0.00637) | (0.00508) | (0.00378) | (0.00328) | (0.00343) | (0.00332) | (0.00761) |
| $R^2$ | 0.048 | 0.048 | 0.032 | 0.040 | 0.027 | 0.043 | 0.042 | 0.162 |
| *Panel D - With school pair dummies, student gender and time varying treatment effects* | | | | | | | | |
| Treatment school x (2016 to 2018) | -0.0174** | -0.0143** | -0.0136** | -0.0109** | -0.000915 | -0.00625 | -0.00241 | -0.00420 |
| | (0.00690) | (0.00700) | (0.00586) | (0.00427) | (0.00366) | (0.00388) | (0.00345) | (0.00753) |
| Treatment school x (2019 to 2020) | -0.0169** | -0.0159* | -0.0162** | -0.0113* | -0.00665 | -0.000413 | 0.00373 | -0.00786 |
| | (0.00818) | (0.00883) | (0.00655) | (0.00581) | (0.00485) | (0.00469) | (0.00459) | (0.0106) |
| R2 | 0.048 | 0.048 | 0.032 | 0.040 | 0.027 | 0.043 | 0.042 | 0.162 |
| F-test p-value (effect equal in both periods) | 0.954 | 0.856 | 0.713 | 0.957 | 0.272 | 0.246 | 0.142 | 0.668 |
| Observations (student x month) | 359,656 | 359,656 | 359,656 | 359,656 | 359,656 | 359,656 | 359,656 | 326,487 |
| Number of students | 11,848 | 11,848 | 11,848 | 11,848 | 11,848 | 11,848 | 11,848 | 11,553 |
| Number of months | 45 | 45 | 45 | 45 | 45 | 45 | 45 | 45 |
| Number of schools | 885 | 885 | 885 | 885 | 885 | 885 | 885 | 885 |
| Dependent variable mean in control group | | | | | | | | |
| Full sample | 0.601 | 0.446 | 0.279 | 0.154 | 0.089 | 0.077 | 0.065 | 0.586 |
| 2016 to 2018 | 0.570 | 0.414 | 0.275 | 0.141 | 0.082 | 0.075 | 0.060 | 0.545 |
| 2019 to 2020 | 0.664 | 0.512 | 0.288 | 0.181 | 0.103 | 0.081 | 0.076 | 0.679 |

Notes: This table presents OLS regression results for the impact of the financial education program on using credit and on log wages. It includes only students who are 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 has fewer observations because some wages are missing or reported as zero. 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.