The Impact of High School Financial Education: Evidence from a Large-Scale Evaluation in Brazil[†]

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We study the impact of a comprehensive high school financial education program spanning 6 states, 892 schools, and approximately 25,000 students in Brazil through a randomized control trial. The program increased student financial proficiency by a quarter of a standard deviation and raised grade-level passing rates. Short-term financial behaviors, however, show mixed results with significant improvements in students' savings and budgeting as well as positive spillovers to parents, but also an increase in students' use of expensive credit to make consumer purchases. (JEL D14, I21, O12, O16)

The fast rate of financial development around the world has made financial products and services widely available, yet such proliferation has consistently outpaced the capacity of individuals and households to make informed financial choices (Gerardi, Goette, Meier 2010; Klapper, Lusardi, Panos 2013). The policy response to this trend has been to introduce consumer protection measures on the supply side, and to promote financial education programs aimed at adults on the demand side. Yet despite their popularity, rigorous evidence on the impact of such programs shows only marginal improvements in financial behavior (Cole, Sampson, Zia 2011; Collins 2013) and these effects tend to dissipate over time (Fernandes, Lynch, and Netemeyer 2014). Also, interest and participation in adult financial education workshops tends to be low, so they do not appear to be an effective way of reaching broad audiences (Bruhn, Ibarra, and McKenzie 2013).

An alternative approach to financial education is to introduce it early, in formative schooling years. Calls for such school based programs are widespread in both developed and developing countries.[1] This emphasis on youth is attractive and relevant for a number of reasons. First, good financial habits formed at an early age are likely to benefit schooling, employment, and standards of living throughout adulthood. The ability to control temptation and delay gratification among youth has been shown in the literature to be an important determinant of lifetime academic, economic, and social outcomes (Mischel, Shoda, and Rodriguez 1989; Duckworth and Seligman 2005; Sutter et al. 2013; Mullainathan and Shafir 2013). 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 have the opportunity to modify not only their own financial choices, but also to act as agents of change in their households' financial decisions.

At the same time, however, the focus on financial education in schools could have some unintended negative consequences. For instance, the emphasis on money and finances could distract from regular academic learning. Bjorvatn et al. (2015) find a financial education program on youth entrepreneurship in Tanzania in fact led to negative treatment effects on school performance and retention. Also, introducing financial concepts in classrooms could make students more aware of money and financial products, and eventually lead to higher take-up of such products and higher spending. In addition, students being primed to learn may work in the opposite direction as well with students prioritizing specific messages of the curriculum and potentially demoting other financial decisions not emphasized in the classroom. This effect could be even more pronounced if students have to exert self-discipline to improve certain financial choices, hence leaving them more vulnerable over other monetary decisions. An important literature in psychology identifies willpower and self-control as a muscle that can become stronger with repeated practice but can also be depleted when applied over a larger set of decisions (Baumeister and Heatherton 1996; Baumeister et al. 1998; Muraven and Baumeister 2000; Baumeister, Vohs, and Tice 2007).

Discerning between these potential impacts is thus an empirical question. However, so far we simply do not know whether and on which dimensions financial education in schools is effective. The few existing empirical studies of school-based programs show either conflicting results, are too narrowly focused, or suffer from important identification concerns.[2]

This paper addresses these shortcomings by using a randomized control trial to study the impact of a comprehensive financial education program for public high school students in Brazil. The program spanned 17 months and was integrated into regular classroom curricula with interactive in-school and take-home exercises. To date, our study is the largest randomized evaluation in the financial education

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[1] The US Consumer Financial Protection Bureau now officially recommends introducing and building key financial education concepts throughout the K–12 school years (CFPB 2013). The UK government has mandated financial education to become part of the national secondary school curriculum since September 2014, and the Australian government has voted for the same. Many developing countries including Brazil, Colombia, India, Indonesia, and Uganda are introducing financial education in their school systems as well.

[2] See Section II of this paper for a detailed discussion of the existing literature.

literature, covering 892 public high schools in six Brazilian states and approximately 25,000 students.

We collected data in schools through three survey rounds consisting of 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.

Our main empirical analysis presents a complex set of findings. While we find unambiguously positive treatment effects on student financial proficiency, graduation rates, and even significant positive spillovers on parents, the effects on short-term student financial behavior are mixed with some fairly cautionary results.

On financial knowledge, we find the financial education program caused a quarter of a standard deviation improvement in student test scores on a SAT-like financial proficiency test. In fact, the entire distribution of scores shifted to the right with students at all levels of capability showing marked improvements in test scores. In addition, administrative data on school performance shows a statistically significant 9 percent reduction in failure rates and significantly higher passing rates in treated schools compared to control schools. Hence, we can rule out that the financial education curriculum negatively affected school performance and enrollment, in fact, our findings suggest the opposite, i.e., that the program complemented regular academic learning.

We also find significant intergenerational effects of the program on parents. Previous literature has studied how parents often learn from their children in areas such as technology adoption (Kiesler et al. 2000; Ito et al. 2009; Katz 2010; Correa 2014), but to the best of our knowledge our study is the first to identify reverse intergenerational knowledge transfers that affect financial decisions. We find that parents in treated schools scored higher on financial literacy questions, and also improved their financial behavior, with a 7 percent increase in savings, improvements in the likelihood of having a formal savings account, and maintaining a household budget.[3]

The evidence on short-term student financial behavior is based on self-reported survey measures and hence subject to concerns of demand effects, nevertheless our analysis uncovers some interesting findings. First, we find positive treatment effects on some key areas of focus of the financial education program, 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 identified preferences over hypothetical savings and spending scenarios, and measured whether students felt empowered, confident, and capable of making independent financial decisions.

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[3] We also offered adult financial education workshops for parents, and although the attendance was low we detect further improvements in the percentage of disposable money saved by students from families that attended.

In contrast, we find important cautionary evidence on the impact of the financial education program when we examine real purchasing decisions of students. Specifically, we find a statistically significant increase in borrowing by students in treated schools and greater likelihood of purchasing consumer items such as electronics, shoes, and clothing. In fact, we find a significantly greater use of expensive financial products, such as credit cards and installment plans, by students in treated schools to make these purchases. Furthermore, treated students report a significantly higher likelihood of being behind on some of their credit repayments. These are fairly perverse effects as the financial education curriculum sought to encourage responsible financial behavior, whereas installment plans and credit cards typically charge very high interest rates.

Closer examination of the curriculum as well as insights from theory can help reconcile these perverse findings. First, we consider the hypothesis that making students focus on savings and budgeting made them more aware of money, which in turn led them to spend more. Indeed, we do find significant treatment effects on work and employment outcomes with treated students more likely to be employed and earn an income. These results suggest that students became more aware of money by saving, budgeting, and employment, and then they spent their money on consumer goods.

But why did students choose to purchase on expensive installment and credit plans? To understand this choice, we investigate whether students faced a multitasking problem in that the financial education curriculum urged them to save, budget, and spend wisely all at the same time, and the simultaneous emphasis on all fronts was overbearing. In fact, closer examination of the course books suggest 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 multitasking 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 results suggest that students tended to follow clear directions in some aspects (savings, budgeting), but wavered in other aspects where direction was less clear (spending and borrowing).

Overall, 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, and spillovers to parents. 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 paper proceeds as follows. Section I summarizes the literature on financial education for youth and provides details on the Brazilian context and curriculum studied. Section II presents the research and sampling methodology including analysis of sample representativeness, as well as the study timeline. Section III describes program take-up and implementation, and Section IV presents summary statistics and survey participation analysis. Sections V and VI discuss the main results, and Section VII concludes.

I. Literature and Context

A. *Financial Education for Youth*

There is a growing literature on the impact of financial education among youth, but much of the work relies on correlations rather than causal analysis. More rigorous studies show mixed results at best and even many of them suffer from identification issues. For example, Bernheim, Garrett, and Maki (2001) employ a natural experiment to analyze the impact of state high school financial education mandates on savings behavior in the United States and find that mandates appear to effectively increase exposure to financial education and have a significant subsequent effect on future savings. However, Cole, Paulson, and Shastry (2014a, b) replicate and extend the analysis using a much larger sample from US census data and find, in contrast, no significant impact of high school financial education on future savings. Brown et al. (2014) use a similar empirical approach with even newer data from 1999 to 2012 with a younger cohort (19–29) and identify improved debt-related outcomes immediately after high school graduation. A number of other observational studies on financial education among youth in the United States likewise find fairly mixed evidence (Varcoe et al. 2005; Mandell and Klein 2009; Carlin and Robinson 2010; Walstad, Rebeck, and MacDonald 2010)

Outside the United States, Lührmann, Serra-Garcia, and Winter (2012) evaluate 90 minute financial education sessions delivered to 14–16-year-old students in lower stream German high schools. They compare 558 treatment students to 158 control students and find significant improvements in financial knowledge and a hypothetical savings scenario. However, schools were chosen to receive financial education sessions based on how busy teachers felt they were with students prior to the end of the academic year, which raises serious selection concerns since teachers in control schools may have also been dealing with relatively poorly performing students or other unobserved underlying student, class, or school characteristics.

Randomized evaluations of school-based financial education programs are scarce; in fact we are aware of only four other such studies. Berry, Karlan, and Pradhan (2015) conduct an evaluation of two programs offering voluntary after-school clubs in Ghana for primary and junior high students in 135 schools over a 10 month period. The study randomly assigns a group of fifth and seventh graders to a social and financial treatment and another to basic training. It is not clear how exposed students are to financial decision making at such a young age, and eliciting reliable and consistent responses in this age group is generally difficult; hence, there are some measurement concerns. The treatment is also short and participation voluntary. The

findings are unsurprisingly muted, while the study identifies some effects on savings, there are no improvements in financial knowledge, test scores, or social and psychological measures.

Another experiment among secondary school students in Tanzania by Bjorvatn et al. (2015) studies the effects of an edutainment TV show on entrepreneurship. The sample includes 43 secondary schools in Dar es Salaam, where the treatment group received a financial incentive to watch the edutainment show and the control group received the same incentive to watch a weekend movie.[4] The study relies on data from a lab experiment a few weeks after treatment and shows an improvement in students' entrepreneurial ambitions and willingness to take risk, but no improvement in business knowledge. In the longer run, there is an increase in the probability of starting a business, though the data is self-reported over the phone.

In Germany, Lührmann, Serra-Garcia, and Winter (2014) conduct an experiment with 994 students from 25 high schools (12 treatment, 13 control), aged 13 to 15 years old. Treated students received three 90 minute training modules on shopping, planning, and savings. The authors measure students' intertemporal choices using an incentivized experiment and structurally estimated individual preference parameters. They find that participation in the program increased time consistency, but they do not study the program's effect on financial knowledge or behavior.[5]

Finally, Becchetti, Caiazza, and Coviello (2011) conduct a study among 17–19-year-old high school students in Italy covering 944 students in 36 classes. The authors offered a 16-hour long course on finance over 3 months. Difference-in-differences estimates show no statistically significant effect on financial knowledge. Instead, both the treatment and control groups show improved knowledge over time, suggesting that students adapt to repeated financial literacy tests. Becchetti and Pisani (2011) extend the sample to 3,820 students in 118 classes. They now detect positive effects of the course on financial knowledge in a difference-and-difference analysis. Surprisingly, however, treatment group students already performed better on the financial knowledge test before the course, raising questions about the implementation of the random assignment. The paper does not examine effects on financial behavior or attitudes.

B. *Context and Curriculum*

Brazil is a large and growing economy, yet levels of financial awareness and financial literacy are fairly low. In response, the Brazilian government set up a working group of representatives from the financial sector and launched a National Strategy for Financial Education (ENEF) in 2010 with the goal of fostering a culture

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[4]This identification strategy has previously been used to study the impact of financial education through soap operas among South African adults (Berg and Zia forthcoming).

[5]In a related study, Alan and Ertac (2014) conduct a randomized experiment with 37 elementary schools in Turkey, where treated students, aged 9 to 10 years, received an educational program, which aimed to teach forward-looking behavior, over a period of 8 weeks, for 2 hours per week. The results show that treated students made more patient intertemporal choices in incentivized time preference elicitation tasks. Treated students were also less likely to receive a low behavioral grade (based on official school administrative records).

of financial education in the country.[6] The strategy's scope is national, targeting children, youth, and adults, and its goals ambitious, involving a large set of actors and a multiplicity of delivery mechanisms for financial education.[7]

One of the first pilot projects of the ENEF was a financial education program for public high schools. This program was developed in collaboration with the Federal Ministry of Education and State and Municipal Secretariats of Education, assisted by a team of education experts, psychologists, and sociologists. The curriculum encompasses innovative instructional material, where financial education themes are introduced through a cross-curricular approach based on 72 learning frameworks that can be integrated into regular school subjects such as Mathematics, Portuguese, Science, Geography, and History.

High school students in Brazil typically have 14 mandatory subjects, each with a set of predefined skills and content that must be taught. The cross-curricular approach is a teaching technology that allows any of these 14 subjects to use financial education to cover the predefined skills and content required for their courses. For example, students are expected to learn how to relate different measurements (length, mass, area, and volume) and these skills are typically taught in mathematics, physics, and biology. The financial education curriculum includes learning frameworks that allow teachers to use financial education as a theme to develop these same set of skills. One such exercise in the curriculum is a case study of a group of friends planning a barbeque who have had previous disagreements dividing up costs of food and drinks. So this time they plan in advance and calculate the amount of food and drinks they need to purchase and how to divide up these costs. This involves, for instance, calculating the number of soda bottles they should buy given the number of guests and expected consumption, and involves calculating volume. Apart from teaching the core concept, the financial skill that is simultaneously developed through this exercise is the tracking of expenses and budgeting. The learning is further enhanced by using a case study (i.e., planning a party) that is very relevant to the lives of young high school students. Similarly, when discussing the international economy, students are asked to identify the imported products they use in everyday life. Each learning framework concludes with a short outline of the skills and content that students are expected to learn.

The learning is also repeated and continuous throughout the school year. 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, the textbooks in our program contain material for between 72 and 144 hours of teaching (1–2 hours per

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[6]Participating public sector institutions in the working group included the Brazilian Securities and Exchange Commission (Comissão de Valores Mobiliários, CVM), the Central Bank, and pensions and insurance regulatory agencies (Superintendência Nacional de Previdência Complementar, PREVIC, and Superintendência de Seguros Privados, SUSEP). The group also included the association of financial institutions (Associação Brasileira das Entidades dos Mercados Financeiro e de Capitais, ANBIMA), BM&F BOVESPA (the stock exchange), and the federation of banks (Federação Brasileira de Bancos, FEBRABAN).

[7]After the working group fulfilled its purpose, a new committee, the National Committee on Financial Education (CONFEF—including Minister of Education, Minister of Justice, Ministério da Fazenda e Ministério da Previdência Social) was created to spearhead the financial education agenda.

topic). Further, the material includes exercises that students complete with their parents, such as household budgeting, or planning their professional careers.[8]

Finally, the curriculum includes detailed teaching guidelines and teacher training. The guidelines explain how to integrate the financial education framework into the regular curriculum, and teacher training provides an introductory two-day seminar, a reference DVD, and a training website with complementary material and a blog on teaching tools.

II. Research Design, Sampling, and Timeline

A. *Research Design*

We use a randomized control trial to measure the causal effects of the high school financial education program described in Section II. Treatment was assigned at the school level. 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. 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 work and earn income in Brazil so there would be an opportunity to apply newly learned financial concepts to their current decisions.

The choice of classroom was not random in either treatment or control schools, but importantly this choice was made prior to treatment assignment. Schools were required to identify a classroom when they expressed interest in the financial education pilot program. This choice was up to the school administration, but since the selection was made prior to treatment assignment, there is no reason to believe there are systematic differences in how these classrooms were chosen across treatment and control schools.[9]

In addition to the school financial education program, we study the impact of a complementary parent intervention in treatment schools.[10] During the second semester of financial education (spring 2011), parents of students in treatment schools were invited to participate in a school workshop. At the workshop, they were directed to watch either a financial education video (workshop treatment group) or a health education video (workshop control group) through random assignment. Parents learned which video they had been assigned to watch only *after* they arrived at the workshop. The purpose of the parents' workshop was to raise awareness

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[8] A detailed outline of the curriculum is provided in online Appendix 1.

[9] The empirical assessment of this choice would be to compare baseline graduation and dropout rates for selected classrooms versus other classrooms within the same school, but unfortunately we do not have this data available—graduation data is only available at the grade level and is not disaggregated by classrooms.

[10] Due to political constraints, we were unable to extend the parent intervention to control schools.

	Group 1 schools	Group 2 schools
Student intervention	Control	Treatment
Parent intervention	--	½ treatment, ½ control

Figure 1. Study Design

among parents about the importance of financial education and to encourage them to interact more with their children on financial matters, thereby leveraging and reinforcing the material students were taught through the school program. The health education video was shown to control for Hawthorne effects. Figure 1 summarizes the study design.

B. *Sample Selection and Representativeness*

Brazil is divided into 26 states and a Federal District. The Federal District and five states were part of the study, including three of the most populous and developed states (São Paulo, Minas Gerais, and Rio de Janeiro), and two states (Ceará and Tocantins) that represent less developed areas of Brazil. In April and May 2010, the Secretariat of Education in each state assembled a list of public high schools that volunteered to participate in the financial education pilot program, totaling 815 schools. We divided these schools into a treatment and control group through stratified and matched randomization as soon as we received each list, so that the teacher training for treatment schools could be organized and conducted before the mid-year school break in July. São Paulo being the state with the largest number of schools sent four separate lists on different dates. In addition, one of the project's partner institutions (Instituto Unibanco) provided us with an additional list of 101 public schools that they partnered with in Rio de Janeiro, São Paulo, Minas Gerais, and the Federal District.

Overall, our sample is fairly representative of public high schools in Brazil. According to data from the Federal Ministry of Education, there were 18,193 public high schools across Brazil in 2008, of which nearly 44 percent were located in our sample states—21 percent in Sao Paulo, 12 percent in Minas Gerais, 6 percent in Rio de Janeiro, 3 percent in Ceara, 1.4 percent in Tocantins, and 0.4 percent in Federal District. Our study sample drew predominantly from the larger States of Sao Paulo (42 percent of sample) and Rio de Janeiro (31 percent of sample). To assess representativeness within states, Appendix Table 1 compares school quality measures for in-sample and out-of-sample schools, where in columns 1 and 4 high school graduation and dropout rates are regressed on an indicator for in-sample schools and state dummies. Columns 2 and 5 add interactions with state dummies to assess state-by-state differences. Finally, columns 3 and 6 include even finer controls with municipality fixed effects. Standard errors are clustered at the municipality level and data is available for nearly 8,000 schools in 1,966 municipalities. The coefficients on all regressions and interactions with state dummies are statistically indistinguishable from zero, which suggests that schools in our study sample were similar to other schools in the country in terms of school quality.

For the schools in our study, we further obtained municipality-level economic indicators and first stratified by whether they were located in a municipality with above or below median number of financial institutions per capita. Within strata, we formed matched pairs of schools and randomly assigned one school in each pair to be in the treatment group and the other school to be in the control group. We matched on the following school and municipal variables obtained for 2008, to improve balance on these characteristics across the treatment and control groups: GDP per capita of the municipality, savings volume per capita of the municipality, school location, number of students in the school, number of teachers in the school, school dropout rate, and school graduation rate.

Randomization was done by us and through a computer, implying that any differences across the treatment and control group are due to pure chance. After the randomization was completed, we had to move three control schools to the treatment group manually since some states requested that at least one school in each school district participate in the program. We chose these schools at random among the schools in the school district. In addition, five schools had accidentally been listed twice (three were on both the state ministries' and Instituto Unibanco lists and two were duplicated on the state ministries' lists). We randomly chose which entry to drop.

Also, after the randomization was completed but before the program was implemented, we discovered that 12 schools indicated by the Instituto Unibanco decided not to participate in the pilot. Another six treatment group schools from São Paulo and one control school from Rio de Janeiro did not participate in any of our surveys for unknown reasons.[11] In the end, we are left with 892 schools for the analysis—440 in the treatment group and 452 in the control group. The majority of the schools are located in the most populous states, São Paulo, Rio de Janeiro, and Ceará.

C. *Sample Selection for Parent Workshops*

For the implementation of the parent workshops, treatment schools sent in a list of current students enrolled in the class receiving financial education during the spring of 2011. Only schools that sent the list were included in the study as this was taken as a signal that they were willing to implement the workshop. A total of 264 treatment schools provided a list, covering 8,534 students.

We matched the lists with data from the baseline parent survey on the basis of the student's name, and stratified and randomly assigned parents in each school into treatment and control groups by computer. We used the following strata: (i) no baseline information on parents; (ii) parent had low baseline financial literacy; and (iii) parent had high baseline financial literacy. We defined the level of baseline financial literacy based on the number of correct answers to the two standard financial

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[11] We conducted an "extreme" bounding exercise to address this noncompliance for three of our main outcome variables: financial proficiency, disposable money saved, and listing of monthly expenses. As part of the exercise, we assigned the minimum value of the outcome to all students in the 18 treatment schools that attrited and maximum value of the outcome to all students in the one control school that attrited. Our main results remain statistically significant at the 5 (or 1) percent level.

literacy questions on interest rates and inflation asked on the survey.[12] About 41 percent of parents did not answer either question correctly. We classify these parents as having low baseline financial literacy. Parents who answered one or two questions correctly are classified as having high financial literacy.

Each school that provided a list of their students received two separate lists in return: one with the names of the students whose parents would watch the financial education video during the school workshop (treatment group) and one with the names of students whose parents would watch the health video (control group). Schools were provided the financial education and health videos and parent exit questionnaires. Each school was responsible for organizing and implementing the workshop at a time of their choosing and administering the questionnaire at the end of the workshop.

D. *Empirical Specification*

Based on the random assignment, the impact of student financial education is measured as the difference in average outcomes in the treatment and control groups using the following intention-to-treat OLS regression.

$$(1) \qquad y_{i,s,f} = \alpha + \beta Treatment_{i,s} + \sum \gamma_s d_s + \delta y_{i,s,b} + \varepsilon_{i,s},$$

where $y_{i,s,f}$ is a measure of the financial knowledge, attitude, or behavior, of student or parent i in school pair s at follow-up f. The variable $Treatment_{i,s}$ 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. Standard errors are clustered at the school level.

All analysis tables have a standard reporting format where panel A reports cross-sectional regression results from estimating equation 1 without control variables. Panel B controls for school pair dummies to account for our method of randomization, as per Bruhn and McKenzie (2009). Following the suggestion in Glennerster and Takavarasha (2013), we include a dummy for each school pair we randomized, unless doing so would lead us to drop the pair (due to noncompliance of some schools or re-randomization as described in Section IVB). We instead include a common dummy for all schools that would have been dropped with a school pair dummy. Panel C additionally controls for baseline values of the dependent variable, as per McKenzie (2012), and for student gender (see Section VIC). When baseline values have missing observations, we replace these with zero and include a dummy variable indicating that the observation was missing. Finally, note that results from

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[12] These two questions are:

Question 1. Assume that you would like to get a loan for R$ 50,000. Bank A offers you a loan of R$ 50,000 that needs to be repaid after one year with an interest rate of 1 percent payable at the end of the loan. Bank B offers you a loan for the same amount, R$ 50,000, for which you will have to pay R$ 60,000 also after a period of one year. Which option do you prefer?

(A) Bank A, (B) Bank B, (C) Don't know.

Question 2. Assume that the interest rate on your savings account is 1 percent per year and the inflation rates 2 percent per year. After one year without making any transactions on our saving account, would you be able to purchase more, the same amount, or less than what you could purchase today with the money in the account?

(A) More than today, (B) The same as today, (C) Less than today, (D) Don't know.

the first and second follow-up surveys are not fully comparable due to changes in class composition from one year to the next, as discussed in Section IVC.

The analysis of the parent workshops is based on workshop attendance rather than invitation, hence, the treatment variable in specification (1) takes on the value of 1 if the parent watched the financial education video, and 0 if the parent watched the health education video. As mentioned before, these assignments were unknown to the parents prior to arriving at school, and both videos were shown simultaneously and in identical settings.

E. *Study Timeline*

The sample selection and randomization for the student intervention occurred in April–May 2010. A baseline survey was conducted in early August 2010 among students and parents in both treatment and control schools. The financial education program was rolled out immediately after. By mid-August 2010, teachers began using the financial education materials in the classroom. The program continued until November 2011 for a total of three school semesters.

Over the course of this study, two rounds of follow-up surveys were conducted. The first follow-up survey was implemented in early December 2010, four months after the program started. The results of this survey measure the short-term effects of the program. A second follow-up survey was implemented in December 2011 to assess the longer term impacts. The main reason for this timing of the follow-ups was logistical. The surveys were conducted in school rather than at student homes and the second follow-up was for students in their final year of high school. Hence, there would not be another opportunity to survey these students in a central location with low attrition.

The parent intervention was introduced in May 2011, and parent outcomes recorded through an exit survey and the December 2011 follow-up survey.

III. Program Take-Up and Implementation

A. *Student Program*

Following school assignment to treatment and control, the program distributed textbooks to treatment schools and organized training sessions for the teachers. The vast majority of treatment schools received the financial education textbooks and distributed them to students. In both rounds of the follow-up survey, we asked school principals as well as teachers and students a series of questions regarding implementation and usage of the financial education program. Follow-up 1 spans the first semester of the program, while follow-up 2 covers the second and third semesters.

Over 95 percent of treatment school principals report that they received the textbooks for the first semester, and 93 percent report receiving them for semesters 2 and/or 3. The large majority of teachers also say that students received the textbooks (i.e., the books were actually distributed to students), 94 percent and 92 percent in follow-up 1 and 2, respectively. On training, 78 percent of teachers report that they received training on how to use the financial education material in the first semester, and 65 percent report receiving it in the second/third semesters.

In terms of usage, 87 percent of students report that teachers actively used the financial education textbooks in classrooms in the first semester, though a drop-off occurs in semesters 2 and 3, with 74 percent of students reporting usage. However, the percentage of principals reporting that financial education was taught in school remained high throughout the study period (93 percent for all semesters).

Schools in the control group did not receive textbooks or teacher training through the financial education program studied in this paper, but they may have implemented other types of financial education. The principal and teacher questionnaires were only applied in treatment schools during the first follow-up survey, but during the second follow-up survey control schools also answered these questionnaires.[13] In this survey, 16.6 percent of control group principals stated that the school had a financial education program and 11 percent of control group teachers reported receiving some training related to financial education. We do not have detailed information on the financial education program implemented in control schools. However, only 5 percent of control group principals said that the school received a textbook with financial education material, suggesting that these programs may be less intensive than the one studied in this paper.

Data from the teacher questionnaires indicates that the financial education material was well received. Ninety percent of teachers interviewed stated that the financial education material complemented regular learning. More than 90 percent said it had a direct positive effect on their teaching of the curriculum. The focus groups also asked teachers to rate student interest in the material, more than 88 percent responded positively to this question and more than 90 percent said that students learned a lot from the material.

We supplemented this quantitative analysis with qualitative work in the form of teacher focus groups for treatment schools, which were organized with our Brazilian counterparts. The purpose of these meetings was to learn how teachers implemented the program, how material was integrated into the regular curriculum, and how the contents and/or delivery could be improved for future implementation. Six such meetings were held in September and October 2011, with teachers and educators from all six states. All meetings were conducted in facilities provided by the state education departments and meals were provided. The meetings were generally well attended (more than 200 educators attended in São Paulo), and lasted an average of four hours each.

In all states, the teachers and educators said that students liked the case studies and felt that they connected well with their daily life situations. For example, case studies in mathematics included budgeting and planning a class party instead of learning the same concepts through an abstract scenario (e.g., calculating distance between trains). The teachers also complemented the clarity with which concepts were conveyed in the books. One teacher commented, "What motivates me the most about this project are the books. The content is directly related to students' lives and helps to insert them in a highly-competitive market society. I learned a lot from the

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[13] The student questionnaire did not include questions on program implementation in control group schools in either follow-up survey.

material, and similarly to what happened to my students, it has helped me to plan better for the future."

In terms of implementation, most teachers employed work group strategies to teach the material, where students were divided into small groups and asked to work on different tasks. Examples of such tasks varied from identifying steps for opening a small firm to creating a school market. Several teachers reported assigning projects to students related to fundraising and organizing their own graduation party. Other teachers reported undertaking field trips to local markets, universities, and companies to learn how they operate. Others used various forms of media to explore the topics in the textbook, such as computer simulations and videos.

Overall, teachers and educators felt the financial education program was valuable, that the textbooks were extremely relevant, and that they as well as their students enjoyed the new learning opportunities afforded to them.

B. *Parent Workshops*

For the parent workshops, we asked schools to mail the filled-in parent exit questionnaires back to us after the workshop, and we determine which schools implemented the workshops based on our receipt of these questionnaires. We received 1,553 filled-in parent questionnaires from 109 treatment schools, implying that parent workshops did not take place in the other 153 treatment schools that had originally provided a current list of their students for the parent intervention (these schools either did not organize the workshop or parents did not attend the workshop). When planning the parent intervention, our counterparts had cautioned that attendance rates may be low since parents of public high school students in Brazil tend to not participate in school events.

Comparing the lists of current students we received from schools before the workshop to the filled-in exit questionnaires gives an average attendance rate of 46 percent across the 109 schools that returned questionnaires. The attendance rate does not differ across parents who were randomized into the treatment and control groups. Parents did not know whether they had been assigned to watch a financial education or health video until the video was screened at the workshop. That is, the decision to attend was independent from treatment status. Hence, in our impact analysis of the parents' workshop, we only keep students and parents if the parent attended a workshop. The interpretation of the results is applicable to families who are more interested in school events and respond to school invitations, which is arguably the most policy-relevant group of parents for this intervention. At the same time, these results are only valid for schools that elected to participate and actually organized the parent workshops.

IV. Survey Data and Summary Statistics

A. *Survey Data*

For the data collection, we partnered with the *Centro de Políticas Públicas e Avaliação da Educação* (CAEd/UFJF), a local education research and survey

institute with extensive experience in designing and implementing knowledge tests in Brazilian schools. CAEd has a wide network of supervisors and surveyors across Brazil and was able to implement simultaneous surveys in all schools in our sample.

Survey implementation took three days per school during each survey round (baseline and both follow-ups). On the first day, CAEd staff independently administered a financial knowledge test and distributed parent questionnaires to students. The students were instructed to take the parent questionnaire home, ask one of their parents to fill-out the questionnaire and return the questionnaire on one of the following days. On the second day, students filled out a self-administered questionnaire measuring financial attitudes and behavior. The third day provided an opportunity for any student who had missed one or both of the previous days to fill out the test and/or questionnaire. The student tests and questionnaires were administered in the classroom in the same way as a regular school exam, i.e., distributed to students, supervised by the surveyor and collected by the surveyor at the end of the allocated time. School teachers were not privy to the tests or questionnaires beforehand, and were not involved in proctoring them.

B. *Baseline Summary Statistics*

Table 1 shows preprogram summary statistics of school and student characteristics for treatment and control schools. The school-level variables were obtained from the Federal Ministry of Education, while the student-level variables were collected through the baseline survey that was conducted in August 2010. Overall, there is balance across treatment and control groups, barring student gender, which we control for in all regression analyses. Student background characteristics show that 56 percent of students participating in the study were female, 67 percent had some form of income (from work or from parents), and about 35 percent were unemployed at baseline. Additionally, 33 percent were beneficiaries of the *Bolsa Família* government cash transfer program, indicating that they belonged to low-income households. About 60 percent of the students' parents had less than high school education. In terms of financial characteristics, only 11 percent made a list of their expenses every month, but 76 percent negotiated the price or payment method when making purchases. There were no differences in baseline levels of financial proficiency, financial autonomy, or intention to save.

C. *Survey Participation and Attrition*

At baseline, 890 out of 892 schools in our study sample participated in the survey, although two of these schools did not implement the financial proficiency test or the parent questionnaire. In each follow-up survey, about 40 schools did not implement the survey. Reasons for non-implementation varied and were mostly related to scheduling difficulties. The schools that did not participate in the follow-up surveys were different in each round, so we have follow-up data for most schools from either follow-up 1 or follow-up 2.

After realizing that survey participation had dropped between baseline and follow-up 1, we provided incentives for survey completion during follow-up 2. Both

TABLE 1—BASELINE SUMMARY STATISTICS

	Number of schools (1)	Number of students (2)	Control		Treatment		Difference in means test (*p*-value) (7)
			Mean (3)	Standard deviation (4)	Mean (5)	Standard deviation (6)	
Panel A. School-level variables							
Number of students in school (2008 data)	892		642.84	461.7	680.89	514.62	0.246
Number of teachers in school (2008 data)	770		37.49	24.09	38.28	25.74	0.664
Grade-level dropout rate (2009 data)	882		11.04	11.22	11.74	11.82	0.365
Grade-level passing rate (2009 data)	882		68.05	16.03	67.74	15.94	0.776
Panel B. Student background characteristics							
Student is female	887	23,483	0.55		0.56		0.079
Mother attended secondary school	887	23,124	0.45		0.44		0.407
Father attended secondary school	887	23,003	0.41		0.4		0.435
Student has failed at least one school year	887	23,076	0.3		0.32		0.107
Student's family receives *Bolsa Familia* cash transfer	887	23,313	0.32		0.33		0.213
Student has computer with Internet at home	887	23,146	0.52		0.53		0.415
Student has some form of income	890	25,012	0.66		0.67		0.184
Student is unemployed	890	24,998	0.35		0.35		0.931
Panel C. Student financial characteristics							
Financial proficiency score	888	23,924	49.87	15.16	50.15	14.83	0.535
Saves money for future purchases	890	24,536	0.15		0.16		0.176
Intention to save index	890	23,464	48.36	18.91	48.18	18.56	0.558
Makes a list of expenses every month	890	24,766	0.1		0.11		0.664
Negotiates prices or payment methods	890	24,555	0.75		0.76		0.584
Financial autonomy index	890	23,450	49.08	19.81	48.98	19.58	0.788

Notes: This table presents baseline summary statistics as well as *p*-values for difference in means tests between students in treatment and control schools. The first four rows show school level variables from administrative data obtained from the Brazilian Ministry of Education. The subsequent rows in the table summarize student-level data from the baseline survey.

treatment and control schools where more than 75 percent students completed at least 80 percent of the survey questions were entered into a lottery for one of twenty five computers. Despite this incentive, we did not have a higher number of schools participate in follow-up 2 than in follow-up 1. However, a greater number of students within the participating schools answered the surveys in follow-up 2 than in follow-up 1. The number of students surveyed per school in follow-up 2 is still lower than in the baseline (about 22 versus 28 students per school). One reason for this decline in the number of students per school is that the drop-out rate is quite high in our sample (about 10 percent per year at baseline).

Note that student rotation is also common in our study sample. As shown in Table 1, 30 percent of students reported that they had repeated at least one school year at baseline. In part due to high drop-out and repetition rates, some schools reshuffle classrooms from one year to the next. In our sample, the majority of students in follow-up 1 were present in baseline (follow-up 1 was implemented in the same semester as the baseline, about four months apart). However, only about 60 percent of students in follow-up 2 were present at baseline (follow-up 2 took place about 16 months after baseline). For the financial education program, this high rotation implies that more than a third of the sample was not exposed to the material for a full three semesters, but rather for only one or two semesters.

With respect to the parent questionnaire, the participation was quite high especially considering that this questionnaire was self-administered at home. About

TABLE 2—SUMMARY STATISTICS OF THOSE WHO ARE PRESENT IN FOLLOW-UP SURVEYS

	Number of schools (1)	Number of students (2)	Control Mean (4)	Treatment Mean (5)	Difference in means test (*p*-value) (6)
Panel A. Present in follow-up 1					
Student is female	857	19,743	0.56	0.58	0.067
Parent attended secondary school	NA	NA	NA	NA	NA
Parent is an employee	830	13,540	0.48	0.48	0.831
Parent is self-employed	830	13,540	0.18	0.18	0.854
Parent's occupation is other (homemaker, retired, unemployed, other)	830	13,540	0.34	0.34	0.927
Panel B. Present in follow-up 2					
Student is female	851	20,723	0.57	0.59	0.056
Parent attended secondary school	838	14,073	0.46	0.47	0.363
Parent is an employee	838	13,710	0.48	0.47	0.703
Parent is self-employed	838	13,710	0.18	0.18	0.415
Parent's occupation is other (homemaker, retired, unemployed, other)	838	13,710	0.34	0.34	0.887

Notes: This table presents background summary statistics as well as *p*-values for difference in means tests for those present in treatment and control schools in the two follow-up surveys. Student gender is manually coded by student name and parent characteristics are reported in the parent questionnaires from each respective survey round. Panel A includes students present in follow-up 1, and panel B includes students present in follow-up 2.

88 percent of students returned parent questionnaires at baseline. In the follow-up surveys, this number dropped to about 76 percent.

Since the analysis sample changed over time in our study, Table 2 checks for balance in the background characteristics of follow-up survey participants. While we did not collect background information in the student follow-up surveys (only in the baseline, due to space considerations), we did do so for the parents in all survey rounds. When we compare parent background variables across the treatment and control groups, we do not see any statistically significant differences in the education levels or occupational status reported by parents in either of the follow-up surveys. For students, we manually coded their gender based on names and Table 2 shows a slightly higher ratio of females in treatment schools in both follow-ups. Note that a similar imbalance was present at baseline (see Table 1), therefore, we include a control for student gender in panel C of all subsequent analysis tables.[14]

Finally, since response rates varied slightly across questions in the surveys, each outcome regression in this paper includes the *p*-value from an *F*-test of joint significance from regressing the treatment indicator on the background characteristics listed in Table 2 for the analysis sample.

[14] Table A2 additionally presents the baseline characteristics of attriting students in both follow-up 1 and follow-up 2, for whom we have a full set of background variables. The table shows that these characteristics do not differ systematically across the treatment and control groups.

TABLE 3—STUDENT FINANCIAL PROFICIENCY

	Financial proficiency score	
	Follow-up 1 (1)	Follow-up 2 (2)
Panel A. No controls		
Treatment school	4.266	3.294
	(0.574)	(0.574)
R^2	0.021	0.012
Panel B. With school pair dummies		
Treatment school	4.173	3.237
	(0.396)	(0.394)
R^2	0.182	0.171
Panel C. With school pair dummies, baseline dependent variable, and student gender		
Treatment school	3.793	3.049
	(0.299)	(0.352)
R^2	0.449	0.318
Sample size (number of students)	18,276	18,953
Number of schools	852	847
Dependent variable mean in control group	56.050	59.045
Dependent variable standard deviation in control group	14.808	14.866
F-test *p*-value (treatment on background characteristics)	0.236	0.235

Notes: This table presents OLS regression results for the impact of the financial education program on student financial proficiency. The number of students and schools included in the sample fluctuate within a wave because not all students answered every question; and they fluctuate across waves because of student turnover. The outcome variable is a student financial proficiency score, which aggregates financial knowledge questions included in the 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. The last row in the table shows the *p*-value from an *F*-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

V. Analysis of Student Outcomes

A. *Student Financial Proficiency*

The analysis of treatment effects of the financial education program starts with studying the impact on financial proficiency in Table 3. The proficiency tests were tailored to the program's material and objectives, and the same tests were administered in both treatment and control schools. Tests were constructed using item response theory (which is also used to construct the GRE and SAT in the United States), and were comparable across students and across time. This implies that a series of equivalent questions were used to test the same concept leading to different combinations of questions on each test. The questions that each student received were thus likely to be different in each survey round and different from other students in class. This minimized the risk that students simply remembered correct answers from previous rounds without truly understanding the question and also reduced the
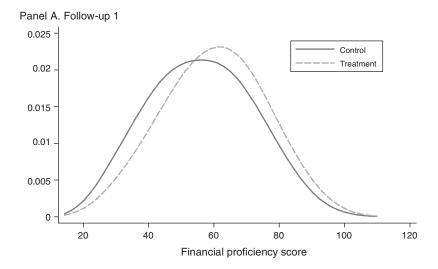
scope for cheating. Teachers were not privy to the tests at any point prior, after, or even on the day of the tests, and CAEd proctors were present in classrooms for the entire duration. No awards were given for performance on tests. These steps protect the analysis from concerns of cheating and teachers teaching to the test.

Student financial proficiency was calculated and scored on a scale of 0 to 100. Table 3 shows the average level of financial proficiency was significantly higher in the treatment group than in the control group in both follow-up 1 and in follow-up 2. Panel C of the table controls for school pair dummies, baseline dependent variable values, and student gender and identifies a difference of 3.8 points and 3.0 points, respectively, corresponding to a 5 to 7 percent increase in financial knowledge. These increases are equivalent to a quarter of a standard deviation improvement by follow-up 1 and a fifth of a standard deviation improvement by follow-up 2.

Further, as shown in Figure 2, test scores improved across their distribution benefiting low- and high-achieving students. Specifically, the proportion of students that performed exceptionally well increased by 28 percent and the proportion of students that performed exceptionally poorly decreased by 26 percent. This effect represents a rightward shift in the distribution of test scores for treated schools compared to control schools, a difference that is statistically significant, as per the Kolmogorov-Smirnov test of stochastic dominance, where we reject the null that the CDFs for treatment and control proficiency scores are the same. Also, in Appendix Table 3, we present quantile regressions for each decile of the proficiency distribution and find statistically significant treatment effects throughout. These distribution-wide effects indicate that the financial education program was suited for all types of students rather than being helpful for just a few.

Comparing these results with impacts in the previous literature is difficult because the small number of existing studies on financial education in schools is fraught with identification concerns, as discussed in Section IIA. But there are school-based studies outside the realm of financial education that are important for comparison purposes. The type of education interventions in secondary schools that have been tested through random control trials include the provision of monetary incentives to students and student tutoring. In general, these studies identify improvements in student learning during the periods studied, although the effects are not always statistically significant.

Perhaps the most well-known studies of monetary incentives for secondary school students are Angrist et al. (2002) and Angrist and Lavy (2009). In the first study, Israeli students were provided cash incentives to pass their graduation exams, and while the mean estimates are positive they are not statistically significant. In the second study, lotteries were used in Columbia to distribute vouchers to partially cover the cost of private secondary schooling for 1,600 students who maintained satisfactory academic progress. Three years after the lotteries, winners were about 10 percentage points more likely to have finished eighth grade, and scored 0.2 standard deviations higher on achievement tests, although the latter result is only marginally significant. Barrera-Osorio and Linden (2009) study the introduction of computers to supplement learning in Colombian schools and find negligible improvements in language and math test scores and limited use of the

Panel A. Follow-up 1

Panel B. Follow-up 2

FIGURE 2. DISTRIBUTION SHIFT IN FINANCIAL PROFICIENCY SCORES

Notes: This figure compares density plots for student financial proficiency scores in treatment and control schools. Follow-up 1 and follow-up 2 plots are presented separately. The Kolmogorov-Smirnov test rejects equality of distributions at the 1 percent level in both follow-ups.

computers for academic learning in higher grades despite the program's focus to do so. Compared to these studies, our finding of an increase of 0.2–0.24 standard deviations in financial proficiency lies in the top end of statistically significant improvements in test scores.[15]

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[15] There are also a number of papers that study classroom interventions in primary schools. These papers tend to find effects on test score between 0.15 and 0.47 standard deviations (for example, Banerjee et al. 2007; Cabezas, Paredes, Quezada 2011; Muralidharan and Sundararaman 2011; He, Linden, and MacLeod 2008; Linden 2008).

B. *Student Graduation*

Next, we examine the impact of the financial education program on student graduation rates. One hypothesis suggests a positive impact especially if the financial education material covered was engaging and interesting, with the new textbooks providing both teachers and students more motivation and interest in teaching and learning. At the same time, however, a greater emphasis on financial education topics and exercises may have come at the expense of neglecting aspects of the regular curriculum. In fact, Bjorvatn et al. (2015) find a financial education program on youth entrepreneurship in Tanzania led to negative treatment effects on school performance and retention. Hence, it is important to empirically investigate impacts on graduation rates.

The data on graduation comes from administrative records of the Brazilian Ministry of Education for 2009, the year prior to the intervention, and 2011, the year after the intervention. While we do not have access to individual student-level or class-level data, we were provided aggregated information at the grade level. The results are presented in Table 4. Column 1 of Table 4 presents the treatment coefficient on passing rates and finds a 1.2 percentage point improvement over the control group, a statistically significant effect at the 5 percent level in panel C after controlling for school pair dummies and baseline values of the dependent variable. Columns 2 and 3 identify treatment effects on failure and dropout rates. While we do not detect a statistically significant effect on the likelihood of dropping out of school in 2011, the effect on failing is negative and statistically significant at the 10 percent level and represents a 9 percent reduction in failure rate compared to the control group average.

An important point to note is that the analysis on graduation likely underestimates the true effect since the administrative data is at the grade level, whereas our financial education program was provided to a single classroom in each school. Moreover, the results allay concerns about the financial education program diverting attention and jeopardizing grades in the regular curriculum. The evidence, in fact, suggests a complementary role of financial education. Part of the explanation is that financial education was not offered as a separate course but rather integrated into the regular curriculum. It is important to highlight that there were no extra hours of instruction added, rather the program was designed to substitute existing curriculum case studies with ones with financial education themes. Moreover, the qualitative interviews of teachers and students discussed in Section VA suggest that the new material was more exciting and engaging than the regular textbooks.

C. *Financial Behavior and Attitudes*

Next we turn to analyzing changes in short-term financial behavior and attitudes. All of these measures were self-reported and are thus subject to the caveat that treatment group students may simply try to give the "right" answer to these survey questions. In order to minimize these potential demand effects, students were made well aware that their responses would not be used to assess their grades or influence any aspect of school performance, that their identities would be kept anonymous,

TABLE 4—STUDENT GRADUATION

	Grade-level passing rate (1)	Grade-level failing rate (2)	Grade-level dropout rate (3)
Panel A. No controls			
Treatment school	0.011	−0.009	−0.003
	(0.007)	(0.006)	(0.005)
R^2	0.003	0.003	0.000
Panel B. With school pair dummies			
Treatment school	0.012	−0.009	−0.003
	(0.006)	(0.005)	(0.004)
R^2	0.562	0.567	0.616
Panel C. With school pair dummies and baseline dependent variable			
Treatment school	0.012	−0.008	−0.004
	(0.006)	(0.005)	(0.004)
R^2	0.606	0.590	0.678
Number of schools	877	877	877
Dependent variable mean in control group	0.841	0.093	0.066
Dependent variable standard deviation in control group	0.102	0.081	0.068

Notes: This table presents OLS regression results for the impact of the financial education program on grade-level student graduation and retention rates. 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. The analysis uses administrative data from the Brazilian Ministry of Education for the 2011 school year for grade-level outcomes at the end of the school year. Baseline dependent variable values in Panel C correspond to the same grade-level outcomes for the 2009 school year. When baseline outcomes have missing values, they are replaced by zero and a dummy variable indicating such missing values is included. Passing rate, failing rate, and dropout rate sum to 1 for each school. Column 1 shows treatment effects on the graduation rate, while columns 2 and 3 identify treatment effects for students who did not pass, by whether they failed the grade (column 2) or dropped out of the grade during the school year (column 3). Robust standard errors are in parentheses.

and that school staff including their teachers would not be allowed to review any completed surveys.[16]

Savings Behavior and Attitudes.—Table 5 presents regression results on savings behaviors and attitudes. We measured savings in several ways: (i) by direct elicitation of saving patterns, (ii) by asking questions on attitudes towards saving and spending, and (iii) by a psychology-based index on intentions to save that aggregated a series of questions that identified preferences over hypothetical savings and spending scenarios.

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[16] Administrative data on financial behaviors and attitudes for high school-aged students in Brazil does not exist in a centrally accessible registry. Less than 30 percent of our sample had formal savings that are likely spread out across a wide array of financial institutions with little data sharing and central aggregating. Most of saving and spending among youth in Brazil is informal and cash-based, which makes it impossible to record administratively. As part of the study, we collected students' national ID card numbers, which we can track as students get older and enter the formal financial system more regularly. Furthermore, unlike the United States where school-based evaluations are widespread, this concept is fairly new in Brazil and our general interaction with students suggests that they were inclined to simply report the truth and that they did not face any kind of pressures from their teachers or parents. Finally, none of the students we spoke to in school visits intimated that they were trying to please the program evaluators in their responses.

TABLE 5—STUDENT SAVINGS BEHAVIOR AND ATTITUDES

	Says they are a saver		Saves at least some of their disposable money		Saves money for future purchases		Has formal savings		Percent of disposable money saved	Intention to save index	
	Follow-up 1	Follow-up 2	Follow-up 1	Follow-up 2	Follow-up 1	Follow-up 2	Follow-up 1	Follow-up 2	Follow-up 2	Follow-up 1	Follow-up 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A. No controls											
Treatment school	0.033	0.037	0.057	0.054	0.027	0.028	0.019	0.011	1.439	2.592	1.827
	(0.009)	(0.008)	(0.009)	(0.010)	(0.007)	(0.007)	(0.010)	(0.011)	(0.403)	(0.475)	(0.462)
R^2	0.001	0.002	0.003	0.003	0.001	0.001	0.001	0.000	0.001	0.004	0.002
Panel B. With school pair dummies											
Treatment school	0.036	0.039	0.052	0.051	0.027	0.025	0.011	0.002	1.365	2.519	1.658
	(0.007)	(0.006)	(0.007)	(0.007)	(0.005)	(0.005)	(0.006)	(0.007)	(0.308)	(0.361)	(0.348)
R^2	0.031	0.035	0.036	0.041	0.039	0.033	0.072	0.072	0.041	0.056	0.051
Panel C. With school pair dummies, baseline dependent variable, and student gender											
Treatment school	0.037	0.038	0.050	0.052	0.026	0.026	0.012	0.009	1.365	2.502	1.731
	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)	(0.005)	(0.004)	(0.006)	(0.308)	(0.295)	(0.326)
R^2	0.204	0.118	0.212	0.110	0.121	0.063	0.457	0.228	0.041	0.276	0.168
Sample size (number of students)	16,798	16,341	16,660	17,843	16,632	17,914	16,409	16,258	17,196	15,271	16,792
Number of schools	845	845	845	845	845	844	844	844	845	843	844
Dependent variable mean in control group	0.323	0.314	0.440	0.403	0.165	0.188	0.200	0.265	12.882	48.960	51.337
Dependent variable standard deviation in control group									18.971	21.157	21.232
F-test p-value (treatment on background characteristics)	0.312	0.520	0.367	0.314	0.293	0.302	0.388	0.565	0.274	0.315	0.279

Notes: This table presents OLS regression results for the impact of the financial education program on student savings behavior and attitudes. The number of students and schools included in the sample fluctuates within a wave because not all students answered every question; and they fluctuate across waves because of student turnover. The outcome variables in this table are: an indicator variable equal to 1 if the student says they consider themselves to be a saver versus spender (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 says they save money for future purchases (columns 5 and 6); an indicator variable equal to 1 if the student has a formal savings account (columns 7 and 8); the percentage of monthly disposable money that is saved (column 9); and an intention to save index that aggregates responses to questions on hypothetical savings and spending scenarios (columns 10 and 11). 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. The last row in the table shows the p-value from an F-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

Columns 1 and 2 report results for follow-ups 1 and 2, respectively, on a question that directly asked students whether they considered themselves to be savers or spenders. Students had to elect one of five options: (i) I'm a spender, (ii) I'm more or less a spender, (iii) I'm neither a spender nor a saver, (iv) I'm more or less a saver, (v) I'm a saver. We coded a dummy variable equal to one if students responded they were savers or more or less savers. Compared to 32 percent savers in the control group in either follow-up round, students in treated schools were 4 percentage points more likely to identify themselves as savers rather than spenders. This effect is robust to adding controls for school pair dummies, baseline dependent variables, and student gender.

Columns 3 and 4 then ask about saving at least some disposable money, and columns 5 and 6 ask about saving money for future purchases. Panel C regressions show that compared to 40 percent of students in the control group, the financial education program prompted an additional 5 percent of students in the treated schools to save some of their disposable money. This result is similar and highly significant (at the 1 percent level) in both follow-up rounds. Similar statistically significant results are reported for saving money for future purchases.

Columns 7 and 8 show treatment effects on formal savings and find a 1.2 percentage point greater likelihood of having a formal savings account over a control group mean of 20 percent. This result is only statistically significant in follow-up 1. However, in follow-up 2, we detect a higher percentage of money saved (14.3 percent of disposable money saved in the treatment group as compared to 12.9 percent in the control group as shown in column 9).[17] This 1.4 percentage point improvement represents a 0.07 standard deviation increase.

Finally, columns 10 and 11 report treatment effects on an intention to save index that aggregated a series of questions that identified preferences over hypothetical savings and spending scenarios. Students responded to questions on (i) attitudes toward financial behavior, such as "In my opinion, saving some money every month is extremely beneficial;" (ii) subjective norms and expectations, such as "My family has the habit of saving some money every month;" and (iii) perceptions of controlling one's behavior, such as "I believe I can save some money every month."[18] The results show that students in treated schools had a significantly higher measured intention to save (51) than those in control group (49) at follow-up 1, and 53 compared to 51 in follow-up 2. Normalizing by the standard deviations in the control group, these effect sizes represent 9–12 percent of a standard deviation.

Money Management Behavior and Attitudes.—Next, we study money management behavior in Table 6, and as with savings we measure outcomes in several ways: (i) by asking about listing of monthly expenses, (ii) by questions on negotiating prices for purchases and conducting market research before buying, and (iii) by a psychology-based financial autonomy index that aggregated a series of questions that measured whether students felt empowered, confident, and capable of making independent financial decisions and influencing the financial decisions of their households.

The results in panel C of columns 1 and 2 show that 16 percent of students in treated schools made a list of monthly expenses as part of a budgeting exercise compared to 13 percent in the control schools in follow-up 1. These numbers are 17 percent and 14 percent, respectively, in follow-up 2. Both treatment effects are statistically significant at the 1 percent level. Columns 3 and 4 show a 3.1–4.3 percentage point greater likelihood of negotiating price or payment method prior to purchases by students in treated schools compared to an average of 74 percent in

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[17] This question was not asked in follow-up 1.
[18] The intention to save index and the financial autonomy index described in the next section were both developed by CAEd using insights from sociology and child psychology for age-appropriate measurement (Micarello et al. 2012). As such, these measures aimed to signal preferences on current as well as future financial decision-making potential of students. The complete list of questions used to develop these measures is provided in the online Appendix.

TABLE 6—STUDENT MONEY MANAGEMENT BEHAVIOR AND ATTITUDES

	Lists monthly expenses in a budget		Negotiates prices or payment methods		Comparison shops before making purchase		Financial autonomy index	
	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. No controls								
Treatment school	0.026	0.032	0.044	0.033	0.010	0.014	1.872	1.807
	(0.007)	(0.007)	(0.010)	(0.010)	(0.005)	(0.005)	(0.444)	(0.488)
R^2	0.001	0.002	0.003	0.001	0.000	0.001	0.002	0.002
Panel B. With school pair dummies								
Treatment school	0.027	0.029	0.045	0.032	0.006	0.012	1.845	1.707
	(0.005)	(0.005)	(0.006)	(0.006)	(0.004)	(0.004)	(0.334)	(0.340)
R^2	0.046	0.047	0.067	0.061	0.037	0.031	0.051	0.061
Panel C. With school pair dummies, baseline dependent variable, and student gender								
Treatment school	0.025	0.027	0.043	0.031	0.005	0.010	1.826	1.743
	(0.005)	(0.005)	(0.005)	(0.006)	(0.003)	(0.004)	(0.234)	(0.298)
R^2	0.146	0.092	0.215	0.130	0.089	0.060	0.446	0.247
Sample size (number of students)	16,735	18,006	16,546	17,871	16,654	17,983	14,615	16,514
Number of schools	845	845	845	845	845	845	844	845
Dependent variable mean in control group	0.129	0.139	0.740	0.739	0.913	0.900	48.969	50.467
Dependent variable standard deviation in control group							19.792	20.718
F-test *p*-value (treatment on background characteristics)	0.331	0.307	0.326	0.301	0.327	0.293	0.581	0.306

Notes: This table presents OLS regression results for the impact of the financial education program on student money management behavior and attitudes. The number of students and schools included in the sample fluctuate within a wave because not all students answered every question; and they fluctuate across waves because of student turnover. The outcome variables in this table are: an indicator variable equal to 1 if the student makes a list of monthly expenses in a budget (columns 1 and 2); an indicator variable equal to 1 if the student negotiates the price or the payment method when making a purchase (columns 3 and 4); an indicator variable equal to 1 if the student comparison shops before making a purchase (columns 5 and 6); and a student financial autonomy index that aggregates questions on whether students feel empowered, confident, and capable of making independent financial decisions and influencing the financial decisions of their households (columns 7 and 8). 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. The last row in the table shows the *p*-value from an *F*-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

control schools. Column 6 finds a 1 percentage point greater likelihood of comparison shopping before making purchases compared to 90 percent in the control schools in follow-up 2.

Finally, columns 7 and 8 measure treatment effects on the financial autonomy index. Autonomy has previously been studied in the psychology and sociology literatures to understand and encompass attributes of independence, confidence, optimism, self-control, and conformity to parents and peers (Berndt 1976; Steinberg and Silverberg 1986; Reichert and Wagner 2007). Noom, Dekovic, and Meeus (2001) synthesize autonomy into attitudinal, emotional, and functional parts. Attitudinal autonomy refers to the ability to set goals, thinking before acting, and encompasses the notions of knowledge, consciousness, and responsibility. Emotional autonomy

relates to the perception of emotional independence in the face of parents, relatives, and peer groups, and the feeling of confidence in one's own choices. Lastly, functional autonomy refers to perceptions of competence, control, and responsibility in making decisions. The measure of financial autonomy used in this paper and developed in Micarello et al. (2012) most closely follows these definitions. Specifically, the autonomy measure was designed to capture students' confidence, independence, and willingness to participate and influence household financial decisions. For example, the survey asked students the extent to which they agree or disagree with statements on (i) reflexive/attitudinal autonomy, such as "I like to think carefully before deciding to buy something;" (ii) emotional autonomy, such as "I feel prepared to talk to my parents about money matters;" and (iii) functional autonomy, such as "I always try to save some money to do things I really like." Five questions were asked in each category, totaling 15 questions. Student responses to these questions were then aggregated into a summative scale, the financial autonomy index, which ranges from 0 to 100.

The results in panel C show that the average financial autonomy score increased from 49 in control schools to 51 in the treated schools by follow-up 1; and from 50 in control schools to 52 in treated schools by follow-up 2. These results are statistically significant at the 1 percent level and correspond to a 0.09 standard deviation improvement over the control group.

Purchasing and Repayment Behavior.—We study purchasing habits of students in Table 7 and repayment behavior in Table 8 and find some important cautionary results.

First, in Table 7 we study purchasing habits of students and find a greater likelihood of purchasing popular consumer items, such as electronic goods, shoes, and clothing. Interestingly, the way these purchases were made varied across the two follow-ups. In follow-up 1, students were significantly more likely than the control group to purchase consumer items with cash or on installments but not using credit cards. The result on using cash complements our finding in Table 5, which shows positive treatment effects on savings. However, the positive impact on purchasing on installments is a perverse result as the financial education curriculum sought to encourage responsible financial behavior and installment plans typically charge very high interest rates. What is particularly interesting is that this effect is no longer statistically significant in follow-up 2, and in fact the coefficient on purchases using credit cards is strongly positive and significant. Note that the confidence intervals of using installments and credit cards for purchases do overlap in follow-up 2, so we cannot identify the difference as being statistically significant, but the difference in coefficient size remains. Nevertheless, either borrowing on credit cards or installments is a fairly costly proposition as both represent very expensive forms of credit.

To investigate these new purchasing habits further, Table 8 studies treatment effects on credit repayment behavior. First, columns 1 and 2 show a 2.9–3.7 percentage point higher propensity to borrow money among the treated students over a control average of 30 percent (borrowing here is from any source, including family and friends). This finding raises a concern as to whether treated students were managing this increased borrowing responsibly. Columns 3 and 4 of Table 8 thus

TABLE 7—STUDENT PURCHASING BEHAVIOR

	Has bought consumer items with cash		Has bought consumer items with a credit card		Has bought consumer items with installments	
	Follow-up 1 (1)	Follow-up 2 (2)	Follow-up 1 (3)	Follow-up 2 (4)	Follow-up 1 (5)	Follow-up 2 (6)
Panel A. No controls						
Treatment school	0.018	0.004	−0.006	0.016	0.021	0.010
	(0.007)	(0.007)	(0.009)	(0.010)	(0.011)	(0.011)
R^2	0.001	0.000	0.000	0.000	0.001	0.000
Panel B. With school pair dummies						
Treatment school	0.012	0.001	−0.008	0.012	0.022	0.013
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)
R^2	0.042	0.039	0.056	0.057	0.075	0.078
Panel C. With school pair dummies, baseline dependent variable, and student gender						
Treatment school	0.014	0.002	0.000	0.016	0.015	0.007
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
R^2	0.167	0.091	0.232	0.126	0.226	0.142
Sample size (number of students)	16,667	18,033	16,338	17,760	16,312	17,690
Number of schools	845	845	845	845	845	845
Dependent variable mean in control group	0.825	0.834	0.252	0.280	0.243	0.231
F-test *p*-value (treatment on background characteristics)	0.283	0.232	0.301	0.236	0.238	0.227

Notes: This table presents OLS regression results for the impact of the financial education program on student purchasing behavior. The number of students and schools included in the sample fluctuate within a wave because not all students answered every question; and they fluctuate across waves because of student turnover. The outcome variables in this table are: an indicator variable equal to 1 if the student has bought a consumer item (electronics, shoes, or clothing) with cash (columns 1 and 2); an indicator variable equal to 1 if the student has bought the same items with a credit card (columns 3 and 4); and an indicator variable equal to 1 if the student has bought the same items with installments (columns 5 and 6). Electronics include cell phones, computers, or tablets. 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. The last row in the table shows the *p*-value from an *F*-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

examine the likelihood that students reported being behind on loan payments. We find some evidence for being behind on repayments in follow-up 1, however, the coefficient is not statistically significant with our full set of controls in panel C. Nevertheless, the *p*-value is barely above conventional levels of significance, hence, columns 5–7 break up the analysis by source of borrowing for follow-up 1.[19] Note that these source categories do not match one-to-one with the questions in Table 7, and the available sources include banks or financial institutions which, typically issue credit cards, retail stores, which typically sell items on installments, and family or friends, which typically lend money informally. The treatment effect on retail store repayments is significant in panels A and B but not in panel C. However, we do find that treated students were more likely to be behind on repayments to friends and family even with our full set of controls in panel C.

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[19] This breakdown is only available for follow-up 1.

TABLE 8—STUDENT REPAYMENT BEHAVIOR

	Has borrowed money from any source		Is behind on any repayments		Is behind on repayments to:		
					Banks or financial institutions	Retail stores	Friends or family
	Follow-up 1 (1)	Follow-up 2 (2)	Follow-up 1 (3)	Follow-up 2 (4)	Follow-up 1 (5)	Follow-up 1 (6)	Follow-up 1 (7)
Panel A. No controls							
Treatment school	0.031	0.031	0.012	0.010	0.001	0.008	0.009
	(0.008)	(0.009)	(0.006)	(0.006)	(0.002)	(0.005)	(0.005)
R^2	0.001	0.001	0.000	0.000	0.000	0.000	0.000
Panel B. With school pair dummies							
Treatment school	0.034	0.029	0.011	0.007	−0.002	0.007	0.009
	(0.007)	(0.006)	(0.004)	(0.005)	(0.002)	(0.004)	(0.004)
R^2	0.035	0.038	0.047	0.043	0.054	0.042	0.026
Panel C. With school pair dummies, baseline dependent variable, and student gender							
Treatment school	0.037	0.029	0.006	0.005	−0.002	0.004	0.007
	(0.006)	(0.006)	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)
R^2	0.210	0.099	0.164	0.068	0.169	0.136	0.097
Sample size (number of students)	16,503	17,135	16,336	17,959	16,428	16,428	16,428
Number of schools	844	845	844	845	844	844	844
Dependent variable mean in control group	0.304	0.288	0.094	0.121	0.015	0.051	0.071
F-test *p*-value (treatment on background characteristics)	0.401	0.360	0.446	0.284	0.406	0.406	0.406

Notes: This table presents OLS regression results for the impact of the financial education program on student repayment behavior. The number of students and schools included in the sample fluctuate within a wave because not all students answered every question; and they fluctuate across waves because of student turnover. The outcome variables in this table are: an indicator variable equal to 1 if the student has borrowed money from any source (columns 1 and 2); and an indicator variable equal to 1 if the student is behind on any repayments (columns 3 and 4); an indicator variable equal to 1 if the student is behind on repayments to a bank or financial institution (column 5), retail store (column 6), and friends or family (column 7). 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. The last row in the table shows the *p*-value from an *F*-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

Overall, the results in Tables 7 and 8 go against the financial education's goal of encouraging responsible financial behavior. On the one hand we acknowledge that these are short-term findings and longer term surveys would be needed to understand the full welfare implications. But on the other hand, even these short-term findings are concerning, and we attempt to investigate further as to why students were spending more on purchases using expensive credit and falling behind on some repayments.

First, we consider the hypothesis that making students focus on savings and budgeting made them more aware of money, which in turn led them to spend more. In Table 9, we study student entrepreneurship and work outcomes and find a 2 percentage point greater likelihood of treated students working as employees in both follow-up surveys (columns 3 and 4). Additionally, columns 5 and 6 show a 4–5 percentage point higher likelihood of earning an income among treated students. The financial education curriculum did focus on entrepreneurship, with discussion of

TABLE 9—STUDENT ENTREPRENEURSHIP AND WORK OUTCOMES

	Works in own or family business		Works as an employee		Earns an income	
	Follow-up 1 (1)	Follow-up 2 (2)	Follow-up 1 (3)	Follow-up 2 (4)	Follow-up 1 (5)	Follow-up 2 (6)
Panel A. No controls						
Treatment school	−0.004	0.005	0.027	0.022	0.053	0.047
	(0.006)	(0.005)	(0.012)	(0.013)	(0.010)	(0.010)
R^2	0.000	0.000	0.001	0.001	0.003	0.002
Panel B. With school pair dummies						
Treatment school	−0.003	0.002	0.023	0.015	0.054	0.041
	(0.005)	(0.004)	(0.009)	(0.009)	(0.007)	(0.008)
R^2	0.032	0.028	0.084	0.095	0.048	0.049
Panel C. With school pair dummies, baseline dependent variable, and student gender						
Treatment school	−0.001	0.002	0.021	0.016	0.050	0.040
	(0.004)	(0.004)	(0.006)	(0.008)	(0.006)	(0.007)
R^2	0.218	0.094	0.261	0.134	0.230	0.111
Sample size (number of students)	16,782	18,039	16,782	18,039	16,750	17,972
Number of schools	845	845	845	845	845	845
Dependent variable mean in control group	0.130	0.107	0.235	0.297	0.609	0.625
F-test p-value (treatment on background characteristics)	0.382	0.258	0.382	0.258	0.318	0.238

Notes: This table presents OLS regression results for the impact of the financial education program on student entrepreneurship and work outcomes. The number of students and schools included in the sample fluctuate within a wave because not all students answered every question; and they fluctuate across waves because of student turnover. The outcome variables in this table are: an indicator variable equal to 1 if the student works in own or family business (columns 1 and 2); an indicator variable equal to 1 if the student works elsewhere as an employee (columns 3 and 4); and an indicator variable equal to 1 if the student earns an income (columns 5 and 6). 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. The last row in the table shows the p-value from an F-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

successful graduates who were presented in the textbooks as role models as well as market visits to meet real entrepreneurs. The material also covered issues related to finding a job, including how to prepare a CV, respond to job ads and improve interview skills.

Hence, combined with the evidence on improved savings and budgeting, the findings on work outcomes are consistent with the hypothesis that the financial education program made students more aware of money and ways to earn more.

But these results do not explain why students opted to purchase consumer items on expensive credit and fell behind on some repayments. To understand this choice, we consider whether students faced a multi-tasking problem in that the financial education curriculum urged them to save, budget, and spend wisely all at the same time, and the simultaneous emphasis on all fronts was too much. In fact, closer examination of the course books suggest that while the curriculum offered very clear direction on actions, such as saving and budgeting as well as entrepreneurship, there was no such clear indication when it came to purchasing items on expensive credit cards or installment plans. Instead, the emphasis was on developing an understanding and appreciation of the pros and cons of available

credit options. Hence, it is possible that students followed direction to save, budget, and work because these actions were fully endorsed and directly recommended by the curriculum, but they did less well in credit decisions where a clear choice was not recommended.

Insights from the literature in psychology and economics can help reconcile these findings further. Existing evidence from psychology supports a mechanism by which individuals can be taught to self-regulate better. The literature identifies self-control as being important in overriding temptation (Baumeister et al. 1994; Barkley 1997), and how abiding and following learned rules can help individuals exhibit self-control and delay gratification (Hayes 1989; Hayes et al. 1996). At the same time, however, such constant exertion of willpower can lead to perverse effects on subsequent self-control situations. The literature describes willpower as a muscle that can be depleted as it gets overworked or fatigued (Baumeister et al. 1998; Vohs, Baumeister, and Ciarocco 2005; Vohs et al. 2011), even leading to subsequent impulsive spending and consumption choices (Vohs, Baumeister, and Tice 2008; Vohs and Faber 2007). Related studies have also shown biological effects of willpower depletion with lower glucose levels (Gailliot et al. 2007) and neural stress (Inzlicht and Gutsell 2007). Hence, the literature does point to a potential multi-tasking problem where students focused a lot on certain financial decisions but had problems with others where it was easier to make mistakes or cheat since clear direction was not provided.

Note that the psychology literature also talks about mechanisms that can replenish limited willpower resources. For instance, it identifies how willpower may be made less vulnerable to being depleted in the first place. While the muscle analogy suggests limited capacity due to exhaustion, the muscle can also be strengthened over time with greater exercise. Moreover, repeated practice of willpower in a particular decision space can improve self-control mechanisms by making these decisions akin to second nature (Baumeister and Heatherton 1996; Baumeister et al. 1998; Muraven and Baumeister 2000; Baumeister, Vohs, and Tice 2007). Eventually, such habits become routine and do not require making decisions at all, which can free up capacity for other choices requiring willpower. Oaten and Cheng (2006) demonstrate this effect in a physical exercise study and find that regularly exercising willpower by conducting physical activities over a two month period led to better willpower in other aspects of life such as improving study habits, drinking less alcohol, and monitoring spending.

In sum, while our short-term results do show cautionary and perverse effects on students' purchasing and repayment behavior, there is a potential mechanism by which students could improve these outcomes over time. Longer-term follow-ups would enable us to test this important hypothesis.

VI. Intergenerational Effects on Parents

Apart from students' own financial behavior, we investigate whether students become more proactive in their households' financial decisions. Indeed, the curriculum included several take home exercises that were meant to be completed with parents, such as making a household budget and exploring savings account options.

TABLE 10—INTERGENERATIONAL EFFECTS ON PARENT FINANCIAL OUTCOMES

	Financial knowledge					Financial behavior				
	Interest rates		Inflation		Budgeting	Lists monthly expenses in a budget?		Has formal savings?		Percent of income saved
	Follow-up 1	Follow-up 2	Follow-up 1	Follow-up 2	Follow-up 2	Follow-up 1	Follow-up 2	Follow-up 1	Follow-up 2	Follow-up 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. No controls										
Treatment school	0.004	0.018	0.016	0.011	0.062	−0.014	0.016	0.016	0.023	0.761
	(0.011)	(0.012)	(0.011)	(0.012)	(0.012)	(0.010)	(0.009)	(0.013)	(0.012)	(0.347)
R^2	0.000	0.000	0.000	0.000	0.005	0.000	0.000	0.000	0.001	0.001
Panel B. With school pair dummies										
Treatment school	0.004	0.019	0.012	0.014	0.065	−0.015	0.010	0.005	0.014	0.807
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)	(0.007)	(0.008)	(0.007)	(0.269)
R^2	0.043	0.048	0.051	0.056	0.070	0.034	0.036	0.104	0.107	0.043
Panel C. With school pair dummies, baseline dependent variable, and student gender										
Treatment school	0.002	0.019	0.014	0.015	0.065	−0.008	0.018	0.002	0.014	0.807
	(0.008)	(0.009)	(0.008)	(0.008)	(0.009)	(0.007)	(0.007)	(0.006)	(0.007)	(0.269)
R^2	0.086	0.066	0.126	0.098	0.070	0.185	0.112	0.328	0.201	0.043
Sample size (number of parents)	13,675	13,043	13,635	12,963	13,065	13,493	13,951	13,381	13,908	13,317
Number of schools	829	838	829	838	838	830	838	830	838	838
Dependent variable mean in control group	0.444	0.444	0.328	0.321	0.679	0.367	0.368	0.736	0.764	12.106
Dependent variable standard deviation in control group										16.461
F-test *p*-value (treatment on background characteristics)	0.745	0.457	0.696	0.498	0.514	0.773	0.268	0.710	0.253	0.510

Notes: This table presents OLS regressions results for the impact of the student financial education program on their parents' financial outcomes–intergenerational transmission. The number of parents and schools included in the sample fluctuate within a wave because not all parents answered every question; and they fluctuate across waves because of student turnover. The outcome variables on financial knowledge are three financial literacy questions in the parent surveys: the interest rate question (columns 1 and 2) tests the ability to calculate an interest rate using percentages; the inflation question (column 3 and 4) tests the understanding of how inflation affects future purchasing power; and the budgeting question (column 5) tests the knowledge of what goes into a budget—this question was not asked in follow-up 1. The outcome variables on financial behavior include: an indicator variable equal to 1 if the parent makes a list of monthly expenses in a budget (columns 6 and 7); an indicator variable equal to 1 if the parent has formal savings such as a current account, savings account, debit card or checks (columns 8 and 9); and the percentage of monthly income that is saved (column 10). The percentage of income saved question was not asked in follow-up 1. 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. The last row in the table shows the *p*-value from an *F*-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

First, as proof of concept we find that a significantly larger percentage of students in the treatment group talked to their parents about finances and participated in organizing the household budget. Panel C of Appendix Table A4 shows that 70 percent of students in treatment schools participated in household financial decisions compared to 67 percent in control schools in follow-up 1, and 74 percent compared to 70 percent in follow-up 2. Due to the program, students were also significantly more likely to help organize the household budgets with significant improvements of 3.2–4.5 percentage points over the control group.

Next, we examine intergenerational impacts of student financial education on parents' financial knowledge and behavior. Financial knowledge is measured

through two standard questions on interest rates and inflation (listed in a footnote in Section IVA). The results in Table 10 show that parents of treatment school students were significantly more likely to correctly answer both financial knowledge questions than parents of students in control schools in follow-up 2. We also detect a positive and statistically significant difference in knowledge of budgeting between the two groups. When examining the impact on parents' financial behavior, we detect no effects in follow-up 1 but several positive impacts in follow-up 2. The percentage of parents who save more than 0 increased from 76 percent in control schools to 78 percent in treatment schools. The average percentage of income saved increased from 12 percent in control schools to close to 13 percent in treatment schools. Parents in student treatment schools were also more likely to list monthly expenses in a budget, with an increase from 37 percent of parents in the control schools to 39 percent of parents in the treatment schools.

We measure the impact of parent financial education workshops in Table 11. This table also provides further insight on the mixed results we found for student behavior. We can directly test whether parents who attended the financial education workshop had influence over the decisions and financial choices of their children, compared to parents who attended the health education workshops.

As shown in Table 11, we find no significant improvements in parents' own financial behavior as a result of watching the financial education video in columns 1–4, but column 6 for student outcomes shows a significant improvement in the percentage of disposable money saved among students by 2.5 percentage points. Importantly, we do not find any significant effects on any other student financial behavior including decisions on buying items on credit or being behind on repayments.

One interpretation of these results that is consistent with the earlier discussion of student results is that the parent workshops helped reinforce the main thrust of the student program (i.e., savings) but was not as effective in making marginal improvements on other aspects, such as credit choices, which were not as heavily pushed in the student program.

VII. Conclusion

Financial education in schools is an important policy focus in both developed and developing countries, yet its impacts are not well understood. This paper contributes to the literature by using a randomized control trial to study the impact of a financial education program for high school students in Brazil. The analysis combines administrative data on test scores and class graduation rates with multiple elicitation methods in surveys to measure financial outcomes. To date, our study is the largest randomized evaluation in the financial education literature.

Our analysis shows that the program was successful on many dimensions but some results also suggest caution in interpreting this success. While we find significant improvements in financial proficiency and graduation outcomes of students, the evidence on financial behaviors is mixed. We observe improvements in savings, budgeting, and even spillover effects on parent financial knowledge and behavior, but we also find a significantly higher use of expensive financial products, such as credit cards and installment plans by students to make consumer

TABLE 11—PARENT FINANCIAL EDUCATION WORKSHOP

	Parent outcomes				Student outcomes						
	Budgeting question (1)	Lists monthly expenses in a budget? (2)	Has formal savings? (3)	Percentage of income saved (4)	Saves for purchases? (5)	Percentage of disposable money saved (6)	Has bought consumer item with cash (7)	Has bought consumer items with a credit card (8)	Has bought consumer items with installments (9)	Has borrowed money from any source (10)	Is behind on any repayments (11)
Panel A. No controls											
Attended financial education workshop	−0.033 (0.025)	0.023 (0.030)	0.008 (0.028)	0.983 (1.013)	0.014 (0.028)	2.121 (1.084)	0.009 (0.021)	0.008 (0.024)	−0.025 (0.025)	−0.005 (0.026)	−0.008 (0.018)
R^2	0.002	0.001	0.000	0.001	0.000	0.003	0.000	0.000	0.001	0.000	0.000
Panel B. With school pair dummies											
Attended financial education workshop	−0.042 (0.026)	0.024 (0.031)	0.021 (0.028)	1.350 (1.057)	0.027 (0.029)	2.445 (1.135)	0.013 (0.021)	0.007 (0.025)	−0.024 (0.025)	−0.011 (0.027)	−0.012 (0.018)
R^2	0.197	0.195	0.241	0.202	0.171	0.170	0.163	0.165	0.195	0.156	0.177
Panel C. With school pair dummies, strata dummies, and student gender											
Attended financial education workshop	−0.042 (0.026)	0.008 (0.030)	0.018 (0.026)	1.350 (1.057)	0.026 (0.027)	2.445 (1.135)	0.022 (0.021)	−0.002 (0.024)	−0.023 (0.024)	−0.013 (0.026)	−0.012 (0.018)
R^2	0.197	0.254	0.375	0.202	0.265	0.170	0.222	0.244	0.271	0.223	0.198
Sample size (number of parents/students)	1,022	1,063	1,059	1,016	1,273	1,239	1,286	1,269	1,267	1,219	12,86
Dependent variable mean in control group	0.824	0.376	0.705	12.550	0.474	13.484	0.829	0.223	0.285	0.296	0.119
Dependent variable SD in control group				14.838		17.567					
F-test p-value (treatment on background characteristics)	0.305	0.558	0.422	0.302	0.302	0.303	0.302	0.302	0.302	0.558	0.302

Notes: This table presents OLS regression results for the impact of the parent financial education workshop. The sample in columns 1–4 includes parents in treatment schools who attended either a financial education or health education workshop. Since workshop assignment was not revealed in advance, the analysis only includes parents who attended. All data in this table is from follow-up 2 since the parent workshops occurred between the two follow-up survey rounds. Four parent outcomes are presented in this table: an indicator variable equal to 1 for correct answers to a question that tests the knowledge of what goes into a budget (column 1); an indicator variable equal to 1 if the parent makes a list of monthly expenses in a budget (column 2); an indicator variable equal to 1 if the parent has formal savings such as a current account, savings account, debit card or checks (column 3); and the percentage of monthly income that is saved (column 4). In addition, seven student outcomes are presented in columns 5–11: an indicator variable equal to 1 if the student saves for purchases (column 5); the percentage of monthly disposable money that is saved (column 6); an indicator variable equal to 1 if the student has bought consumer items with cash (column 7), with a credit card (column 8), with installments (column 9); the student has borrowed money from any source (column 10); and the student is behind on any repayments (column 11). 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 student gender as well as strata dummies, where strata include (i) no baseline information on parents; (ii) parent had low baseline financial literacy; and (iii) parent had high baseline financial literacy. Robust standard errors are in parentheses. The last row in the table shows the p-value from an F-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

purchases, and some evidence on late repayments. We acknowledge that these findings only represent short-term effects. Ultimately, longer term follow-up surveys would be needed to understand the full effect of the program on financial behavior, but the initial evidence does temper the full endorsement of financial education in schools.

A potential explanation for why the financial education program led to an improvement in savings behavior, but to a deterioration in purchasing and borrowing behavior, is that the material contained simpler messages regarding savings. That is, it gave several examples illustrating the benefits of savings. With regards

to purchasing consumer products, the material laid out the advantage and disadvantages of different payment methods. It then emphasized that students needed to make their own informed decision as to whether using installment plans, credit cards or loans was appropriate in their situation or not. Students may have found these messages too complex and ended up making mistakes in their decisions. While we cannot test this hypothesis explicitly, it suggests an avenue for future research on how to best convey financial education messages in schools. Related research on financial education for micro-entrepreneurs, for example, concludes that reducing the complexity of training programs might improve their effectiveness (Drexler, Fischer, and Schoar 2014).

Appendix

TABLE A1—SAMPLE REPRESENTATIVENESS AND EXTERNAL VALIDITY

	Graduation rate (1)	Graduation rate (2)	Graduation rate (3)	Dropout rate (4)	Dropout rate (5)	Dropout rate (6)
In sample school	−1.569 (1.417)	−1.091 (1.424)	−0.044 (0.808)	−0.051 (0.702)	0.303 (0.995)	−0.549 (0.650)
In sample school × Sao Paulo State		−1.837 (2.063)			−0.123 (1.070)	
In sample school × Rio de Janeiro State		2.217 (4.360)			−0.667 (2.444)	
In sample school × Minas Gerais State		−2.917 (2.191)			−1.806 (1.218)	
In sample school × Tocantins State		−2.641 (2.654)			−0.512 (1.744)	
R^2	0.189	0.191	0.506	0.237	0.238	0.445
Sample size (number of schools)	7,936	7,936	7,936	7,936	7,936	7,936
Fixed effects	State	State	Municipality	State	State	Municipality
Dependent variable mean in control group	79.034	79.034	79.034	8.171	8.171	8.171

Notes: This table presents regression results comparing graduation and dropout rates for schools in our study sample relative to schools outside our study sample. Robust standard errors, clustered at the municipality level, are in parentheses.

TABLE A2—BACKGROUND CHARACTERISTICS OF BASELINE SAMPLE WHO ATTRIT IN FOLLOW-UP SURVEYS

	Number of schools (1)	Number of students (2)	Control		Treatment		Difference in means test (p-value) (7)
			Mean (3)	Standard deviation (4)	Mean (5)	Standard deviation (6)	
Panel A. Attrited in follow-up 1							
Student is female	790	4,418	0.55		0.53		0.251
Mother attended secondary school	788	4,329	0.46		0.45		0.684
Father attended secondary school	785	4,313	0.44		0.42		0.343
Student has failed at least one school year	787	4,349	0.39		0.42		0.243
Student's family receives *Bolsa Familia* cash transfer	789	4,384	0.29		0.32		0.189
Student has computer with Internet at home	789	4,364	0.49		0.51		0.403
Financial proficiency score	793	4,530	48.89	15.3	48.25	15	0.416
Student is unemployed	807	4,817	0.39		0.38		0.556
Student has some form of income	808	4,815	0.69		0.7		0.274
Makes a list of expenses every month	806	4,744	0.11		0.11		0.782
Negotiates prices or payment methods	804	4,711	0.75		0.76		0.929
Financial autonomy index	801	4,606	48.43	20.13	48.57	19.84	0.831
Intention to save index	802	4578	46.93	18.76	46.02	19.12	0.144
Panel B. Attrited in follow-up 2							
Student is female	831	7,106	0.52		0.54		0.116
Mother attended secondary school	831	7,006	0.44		0.44		0.984
Father attended secondary school	830	6,947	0.41		0.41		0.818
Student has failed at least one school year	831	6,983	0.34		0.38		0.018
Student's family receives *Bolsa Familia* cash transfer	831	7,059	0.32		0.32		0.957
Student has computer with Internet at home	831	6,995	0.52		0.52		0.995
Financial proficiency score	833	7,238	48.06	15.44	48.52	14.84	0.46
Student is unemployed	837	7,540	0.37		0.37		0.891
Student has some form of income	837	7,549	0.67		0.68		0.459
Makes a list of expenses every month	836	7,461	0.11		0.11		0.401
Negotiates prices or payment methods	837	7,375	0.75		0.75		0.717
Financial autonomy index	835	7,002	49.07	19.76	48.22	19.88	0.122
Intention to save index	835	7,015	47.65	19.14	47.42	18.77	0.654

Notes: This tables presents baseline summary statistics as well as *p*-values for difference in means tests between students in treatment and control schools, using data from the baseline survey. Panel A includes students who participated only in the baseline survey, but not in follow-up 1 or follow-up 2. Panel B includes students who participated both in the baseline survey and follow-up 1, but not in follow-up 2.

TABLE A3—QUANTILE REGRESSIONS FOR STUDENT FINANCIAL PROFICIENCY

	Financial proficiency score	
	Follow-up 1 (1)	Follow-up 2 (2)
First decile	4.177 (0.289)	3.179 (0.312)
Second decile	5.926 (0.328)	3.930 (0.342)
Third decile	6.078 (0.389)	4.012 (0.305)
Fourth decile	5.248 (0.345)	3.504 (0.284)
Fifth decile	4.642 (0.308)	3.575 (0.339)
Sixth decile	4.128 (0.308)	3.404 (0.306)
Seventh decile	3.565 (0.289)	3.168 (0.318)
Eighth decile	3.397 (0.293)	3.069 (0.321)
Ninth decile	3.236 (0.319)	2.872 (0.319)
Tenth decile	1.518 (0.309)	1.735 (0.353)
Sample size (number of students)	18,276	18,953
Number of schools	852	847
Dependent variable mean in control group	56.050	59.045
Dependent variable standard deviation in control group	14.808	14.866

Notes: This table presents quantile regression results for the impact of the financial education program on student financial proficiency. The dependent variable is a student financial proficiency score, which aggregates financial knowledge questions included in the survey on a 0–100 scale. The output presents treatment effects for each decile. Standard errors in parentheses are bootstrapped with 1,000 replications.

TABLE A4—STUDENT PARTICIPATION IN HOUSEHOLD FINANCE

	Student discusses financial matters with parents?		Student helps organize household budget?	
	Follow-up 1 (1)	Follow-up 2 (2)	Follow-up 1 (3)	Follow-up 2 (4)
Panel A. No controls				
Treatment school	0.027	0.042	0.036	0.034
	(0.009)	(0.009)	(0.011)	(0.011)
R^2	0.001	0.002	0.001	0.001
Panel B. With school pair dummies				
Treatment school	0.029	0.038	0.041	0.033
	(0.007)	(0.007)	(0.008)	(0.008)
R^2	0.038	0.041	0.049	0.046
Panel C. With school pair dummies, baseline dependent variable, and student gender				
Treatment school	0.034	0.041	0.045	0.032
	(0.007)	(0.007)	(0.007)	(0.008)
R^2	0.168	0.098	0.192	0.111
Sample size (number of parents)	13,663	14,235	13,650	14,257
Number of schools	830	838	829	838
Dependent variable mean in control group	0.671	0.700	0.485	0.520
F-test p-value (treatment on background characteristics)	0.725	0.289	0.783	0.290

Notes: This table presents OLS regression results for the impact of the financial education program on student participation in household finance. The number of parents and schools included in the sample fluctuate within a wave because not all parents answered every question; and they fluctuate across waves because of student turnover. The outcome variables in this table are: an indicator variable equal to 1 if a student discusses financial matters at home (columns 1 and 2); and an indicator variable equal to 1 if a student helps organize the household budget (columns 3 and 4). Both questions are based on responses in the parent questionnaires. 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. The last row in the table shows the p-value from an F-test of joint significance from regressing the treatment indicator on background characteristics for the analysis sample in each column.

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