The Impact of High School Financial Education on Financial Knowledge and Saving Choices: Evidence from a Randomized Trial in Spain*

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This draft: June 6, 2020

Abstract

We conducted a randomized controlled trial where 3,000 9th grade students coming from 77 high schools received a financial education course at different points of the year. Right after the treatment, test performance among treated 9th graders increased by 16% of one standard deviation and they showed more patience in hypothetical saving choices. In an incentivized saving task conducted three months after, treated students made more patient choices than a control group of 10th graders. Within randomization strata, we uncover distinct distributional impacts, as financial education shifted upward the distribution of low scores in public schools, which over-represent disadvantaged students, but not in non-public ones.

Keywords: Financial Education, Financial Knowledge, Saving choices, Impact Evaluation.

JEL Codes: D14, D91, G53, I22, J24.

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*This study is an evaluation of *Educación Financiera en Tercero de la ESO*, financed by the Plan de Educación Financiera. The work of the Department of Markets and Claims of Banco de España (especially Fernando Tejada and Julio Gil) and the team at the Comisión Nacional del Mercado de Valores (Gloria Caballero and Isabel Oliver) has been crucial in setting up this project. We would like to thank Ángel Sanchez-Bayuela, Virginia Morales, and María Torrado for their excellent research assistance. We thank the comments from seminar participants at the European Central Bank, Simposio de la Asociación Española de Economía, Universidad Carlos III de Madrid, Cherry Blossom Financial Education Institute, IZA Workshop of Education, EEA and EALE meetings. We also thank the comments from Sule Alan, Marco Celentani, Steve Lehrer, Annamaria Lusardi, Luigi Minale, Manuel Bagues, Pedro Rey-Biel and Marta Serra-Garcia. The implementation benefited greatly from the suggestions and help of Ismael Cruz, Isabel Peleteiro and Sara Varela. Finally, but not less importantly, we thank all students, teachers and the school management of all participant institutions. The opinions and analyses are the responsibility of the authors and, therefore, do not necessarily coincide with those of the Banco de España or the Eurosystem.

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1 Introduction

In order to equip the general population with the necessary tools for making wise financial decisions, many educational systems have incorporated Financial Education (FE) as part of their curriculum in secondary education. For example, since 1957, various US states have been adopting mandates to include FE in the curriculum of high school students.[1] The consequences of those interventions in the educational system on adults' income, wealth and indebtedness are subject to debate, with some researchers showing increases in net wealth (Bernheim et al., 2001) or credit scores (Brown et al., 2016) while others document much more nuanced impacts (Cole et al., 2016).[2] One reason for the discrepancy can be an insufficient knowledge of how financial education was actually implemented (Urban et al., 2018). A second reason is a lack of understanding of the channels through which financial education works. For example, unless financial education alters the preferences of high school students, their contents could be easily forgotten without affecting students' future financial choices.[3]

In this context, the BdE (the Spanish Central Bank) and the CNMV (the Spanish equivalent to the Security Exchange Commission) launched in 2012 the program *Finance for All* aimed at improving financial knowledge among the population. One of the interventions provides basic financial literacy training in the third year of Mandatory Secondary Education in Spain (the equivalent of ninth grade in the US). The general objective of that program is that students become sufficiently financially literate to make sound financial decisions. In particular, the intervention provides teaching guidelines, quizzes, and games aimed to help interested teachers in delivering this new material. The contents were designed for a ten hour-course, possibly given over one quarter.

[1] Cole et al. (2016) document that 44 states in the US have such mandates.

[2] Bernheim et al. (2001) find that investment income and higher equity in real estate were higher among adults who had been exposed to financial mandates than those who had not. Brown et al. (2016) use detailed credit data to document that youths exposed to financial education programs in the 1990s had a higher creditworthiness. Cole et al. (2016) reexamine the evidence in Bernheim et al. (2001) to explore how sensitive are the results to the use of state fixed effects. Urban et al. (2018) highlight the importance of examining compliance with the financial education mandates.

[3] Lusardi et al. (2017) use a life-cycle model of investment in financial human capital that accounts for the low effectiveness of financial education courses. Their model assumes that preferences are stable parameters that do not change as a result of financial education. We discuss below evidence on the impact of financial education on preferences.

This paper assesses a randomized trial aimed at gauging the impact of the intervention. As part of the intervention design, 77 schools that applied to deliver the material for the first time were randomly assigned to treatment and control. 9th grade students in treated schools (i.e., students turning 15 years of age by December 2015 under normal progression) received the materials between January and March 2015; whereas 9th graders in control schools went through the course between April and June 2015. In each school, a group of 10th graders who did not receive the course was also surveyed and tested (i.e., students turning 16 years of age by December 2015 under normal progression). We analyze the impact of the materials taught on financial knowledge and saving choices. We measured financial knowledge via standardized tests delivered in the class and attitudes toward saving through short surveys to students. Furthermore, three months after the course was delivered, we conducted an incentivized saving task in the spirit of Andreoni and Sprenger (2012), with actual saving choices.[4] In that task, students could split their resources between current and future payments at different interest rates and maturities, and a randomly-selected student in each class would obtain one of her stated choices.

Previous studies have examined the impact of similar courses on the financial literacy of young adolescents and, in some instances, on hypothetical choices (Lührmann et al., 2015, Bruhn et al., 2016, Berry et al., 2018).[5] Other studies focus on the impact of financial training on experimental measures of the degree of patience of students (Alan and Ertac, 2018; Lührmann et al., 2018). While Alan and Ertac (2018) find that a program aimed at increasing the awareness of the future consequences of current choices did increase the degree of patience of 9-10 year-old kids, Lührmann et al. (2018) document instead a fall in the degree of present-biasedness, but not an increase in the degree of patience. Disparate results across studies on the effect of financial education on students' choices may reflect differences in the contents of the course or to what extent student's understood the material. For example, Brown et al. (2016) illustrate that personal finance and economics courses have very different impacts on the behavior of young adults.

[4]Due to budgetary considerations, the incentivized saving task was performed with a subsample of students in Madrid.

[5]Lührmann et al. (2015) consider a very short course delivered by professionals (4.5 hours), Bruhn et al. (2016) evaluate a 80 hours course, and Berry et al. (2018) a curriculum of around 24 hours in total.

We aim to shed some light on this debate by making three main contributions. First, our study decomposes the general notion of financial knowledge into the specific areas of personal finance in which students improved their scores. Secondly, saving choices elicited in incentivized tasks or in hypothetical choices may reflect preference for time or, more generally, changes in the economic situation of an individual (Krupka and Stephens, 2013; Carvalho et al., 2016). Responses in our survey about changes in saving choices complemented with information on income generating activities give us indications of whether changes in behavior reflect changes in preferences or in the student's budget constraint. A final contribution of our study is the variety of schools that participate in the program. Many financial education interventions have focused on a particular set of schools in a few urban areas. The fact that the financial literacy program we examine was taught in public and private schools all over Spain, coupled with the stratified design of the randomization, allows us to examine if results are heterogeneous in a dimension (type of school) strongly related to students' parental background. Indeed, compared to their peers in the non-public system, students in public schools are more likely to be grade repeaters, expect to leave the educational system before professional specialization or have parents not working.

Our results can be summarized as follows. As regards *financial knowledge*, we find that the course increased the scores of treated students by one sixth of one standard deviation in a financial literacy test delivered right after the course. The effect was concentrated on topics related to banking relationships and means of payment, while knowledge about budgeting or sustainable consumption did not change significantly. We also document a significant increase in patience, measured by *hypothetical saving choices* between consumption today and in three or six weeks' time, together with increases in forms of informal labor supply among treated students, like working for money in family business or getting money in exchange of household tasks. Finally, in the *incentivized saving task* performed three months after the program was delivered, we find that treated students allocated an amount to sooner payments that was lower than that of controls by 18% of one standard deviation. Overall, the fall in the preference for early choices both in hypothetical and incentivized tasks while, at the same time, youths engaging more in money earning activities at home, is consistent with the hypothesis that financial education changes students' awareness about the importance of future resources not only through changes in preferences for the future, but also through changes in their available resources. On the contrary, we do not find evidence that this program changed students' behavior by lowering possible present biasedness.

Regarding the subgroup analysis, we highlight two main findings. The first one is that, while mean responses of financial knowledge, are very similar across both types of schools, changes in the distribution of test scores are not. In public schools, we observe a marked improvement in the lower part of the distribution of scores, possibly due to grade repeaters and students expecting to drop-out early. Secondly, the results from the incentivized saving task also suggest that exposure to financial education changed financial decision-making of students in strata with a relatively poorer background. In particular, we observe that early treated students in public schools allocated 36 cents less to the sooner payment than controls, while the corresponding difference among students in non-public schools is 8 cents, a response more than four times smaller. Both findings indicate that financial education could motivate students with relatively weaker performance.

The rest of the paper is organized as follows. In Section 2 we briefly describe the most important features of the program. Section 3 presents the sampling and research methodology. Section 4 summarizes descriptive statistics at baseline. Sections 5 and 6 present the main results for the full sample immediately after the course and in the June incentivized saving task, respectively. Section 7 presents the heterogeneity analysis by type of school. Finally, Section 8 discusses the interpretation of the results and concludes.

2 Description of the program

Since 2012, every year about 400 high schools in Spain have voluntarily delivered a 10-hour financial education under the BdE-CNMV program.

Implementation Although the implementation varies across centers, participant students are typically 9th graders (that is, third grade in compulsory high school, or *Tercero de la ESO* in Spanish). Assuming normal progression through the educational system (i.e., in the absence of grade retention), students complete 9th grade between ages of 14 and 15. That particular grade was chosen to maximize the potential number of students who receive the material, as 9th grade is the last grade of compulsory schooling with few, if any, electives. Compulsory education finishes at age 16 in Spain in 10th grade.[6]

Contents of the course The course covers several areas. The first one introduces the notion of saving, which is presented as a means to achieve future consumption possibilities. Students are introduced to the notion of interest rate and interest rate compounding. In addition, the module introduces the notion of risk associated to different investment choices. A second set of modules includes notions on how to elaborate a budget to be able to save and meet future needs. For example, students learn about the allocation of regular and irregular expenses in a monthly budget. A third set of modules deals with sustainable consumption, aimed at characterizing environmentally responsible consumption. A fourth set introduces the different types of bank accounts. Students are presented with the concept of commissions and fees, as well as on the trade-off between liquidity and return. That part also covers basic security rules in checking and saving accounts. Finally, there are two more modules on specific investment vehicles, such as pension funds and insurance vehicles.

Teachers had discretion over the emphasis and the order of the topics to be covered. In many instances, as 10 hours of new material are difficult to introduce in a single subject, several teachers taught different modules in each of their subjects -for example, by teaching the interest rate compounding module in Maths while the rest of the material in Social Sciences. We provide below survey evidence on how schools delivered the material.

[6] Tenth grade contains many electives (such as *Economics*). There were concerns that schools would deliver the material as part of one of these elective courses, and the outreach of the program would be restricted. Students in Spain must complete 6 grades of compulsory primary schooling, starting at the age of 6 and finishing at the age of 12. After that age, students attend secondary education for four extra years. At the time of the program, all those degrees were common and compulsory for every student in Spain.

3 Evaluation features

3.1 The evaluation sample

The population of interest are 9th grade students in high schools applying to participate in the program for the first time during the 2014-2015 academic year. Neither the teaching body nor students in the school had had any previous experience on the contents of the specific program. The impact of the program in this particular set of schools is informative about how the *introduction* of financial literacy education affects financial behavior, less so about the effects of a settled program with experienced teachers.

We used a phased-in randomized design within the 2014-2015 academic year, as institutional reasons prevented us from excluding any applying school from accessing the material (Table 1 shows the timing of the design). Namely, between July and October 2014 we received three rounds of applications submitted by first-time applicants. The quarter when the material would be delivered was randomized at the school level (the options being either January-March 2015 or April-June 2015). Given the heterogeneity in applicants, in the first three rounds of applications randomization was done within strata defined by the type of school (public, private or concerted) and on whether the school was in Madrid or not.[7] The fourth round of applications was received shortly before the beginning of the program, we stratified only on the grade in which schools intended to teach the material, to maximize the acceptance rate. There are 16 strata in total (see Table A.1 for details).[8]

The randomization was conducted before schools were presented the conditions to participate.[9] Emails and ordinary mail letters were sent to each teacher or school principal who applied for the program communicating that, due to the evaluation, participation in 2014-2015 was conditional on accepting a set of conditions. First, the material was to

[7]The type of school proxies for parental and children characteristics unobserved at baseline. As we discuss below, students in public schools are more likely to have repeated a grade, expect to leave school earlier or have parents not working.

[8]We reordered the schools in each stratum using a random draw from an uniform distribution and split the sample in two halves. Within each stratum there could be an odd number of schools. In those cases, we decided the share of treated was N/2 or (N+1)/2 randomly.

[9]By sending the letter with a pre-specified date of delivery of the course we also wanted to avoid self-selection of teachers into quarters.

be delivered in regular school hours to 9th graders (and only to 9th graders). Second, all 9th graders receiving the course would take three financial literacy tests: in December 2014, March 2015 and June 2015. Third, schools should deliver the material either between January and March 2015 or between April and June 2015, as specified in the communication. Finally, one class of 10th graders in the school (chosen at random) should also conduct the tests, but could not be taught the material.[10] After sending the letters, we phoned each applicant school to explain the requirements in person and answer any question. Out of 169 schools contacted, 77 schools agreed to participate under those conditions (see Table A.1).

The geographical coverage of the final sample is quite broad but not representative of the universe of Spanish schools. Seventy percent of centers are located in three regions: Madrid, Aragon and Valencia.[11] Fifty-six percent of the schools are public and six percent are private schools. The remaining thirty-eight percent of schools were *concerted* ones - i.e., publicly funded but privately owned and managed.

The final sample of 9th graders we use contains 3,050 students in the baseline measurement. Most of the analysis uses a balanced sample of 2,696 9th graders.[12]

3.2 Design of the evaluation and methodology

In December 2014 students took a baseline financial literacy test as well as a short survey on demographics during a fifty-minute class (Table 1). In March 2015 students took a second financial literacy test and an additional survey of similar fifty-minute duration. At the time of the March 2015 measurement, neither 9th graders in the control group nor

[10] We also informed schools that the household of each student would be asked to fill a survey about their demographic characteristics. Finally, teachers delivering the course would also fill a survey regarding details about the implementation of the course.

[11] 22 out of 77 schools come from Madrid; 18 schools were located in Aragon; 14 in Valencia; 5 are from Murcia and another 5 from Canary Islands, 3 from Extremadura and another 3 from La Rioja, 2 from Andalusia and another 2 from Balearic Islands; and one single school from Cantabria, Castile La Mancha and Galicia. There are no schools from Asturias, Basque Country, Catalonia, Castile and León or Navarre.

[12] The raw sample size is 3,335 students in 9th grade. As mentioned above, an extra class of 10th grade students was requested to take the tests in each school. Adding both groups, the total sample size is 5,099 students. We use the following selection criteria: students must have taken either the December or March tests partly and not classified under *special educational needs* (medical conditions, attention deficit, etc). Table W1 in the Online Appendix lists the selection criteria.

10th graders had received any material on financial literacy. Finally, in June 2015, 9th graders made a third financial literacy test as well as an incentivized task where students could choose between current and future consumption at different interest rates and time horizons. Due to budgetary considerations, only 10th graders in the schools in Madrid did the incentivized saving task.

The financial literacy test and the survey conducted in March 2015 allow us to compare 9th graders in treated schools (those teaching in January-March 2015) to 9th graders in control schools (those teaching in April-June 2015). That comparison delivers plausibly unbiased estimates of the effect of the financial literacy course on short-run financial knowledge and attitudes of young adults.

Formally, we consider linear regression models of the form:

$$Y_{i,s} = \theta_0 + \theta_1 TREAT_s + \theta_2 Y^0_{i,s} + \sum_{k=1}^{k=15} \pi_k X_k + \varepsilon_{i,s} \tag{1}$$

where $Y_{i,s}$ denotes the outcome of interest of student i in school s. $TREAT_s$ takes value 1 if the school was assigned to receive the course between January and March 2015, and zero otherwise. $Y^0_{i,s}$ is the value of the variable $Y_{i,s}$ measured at baseline (December 2014) and it is included to improve precision. Finally, X_k are dummies indicating the strata the school belongs to (see Table A.1). $\varepsilon_{i,s}$ is a random error term with unrestricted correlation at the school level, but uncorrelated across schools. When estimating model (1) among 9th graders in March 2015 (right after the first set of treated students were assigned to receive the course), θ_1 measures the impact of the assignment to be taught the course on the outcomes analyzed (knowledge or saving choices).

Furthermore, we can also test for the presence of other confounding interventions or possible spillovers at the school level (if, for instance, teachers use the material of the course in other courses) by comparing those same outcomes between 10th graders in treatment and control schools in March 2015.

In addition, we estimate variants of Model (1) in June 2015. Firstly, we can test whether any financial knowledge is forgotten over a three-month period by comparing the financial literacy score of 9th graders in treated and control schools. By June 2015, 9th graders in control schools had just been presented the material, while treated 9th graders had received it three months before.[13] In such a case, $TREAT_s$ measures any differential impact on outcome $Y_{i,s}$ of how long ago the material was delivered. Similarly, the results in the incentivized saving task in June 2015 allow us to assess if students who had gone through the course in different moments in time (immediately or three months later) opt for different consumption choices when confronted with the possibility to save at different interest rates and maturities. Finally, in the same task, we can compare choices of students in 9th and 10th grade to estimate the impact of financial education on consumption choices (as all 9th graders had received the material by then, we use 10th graders as controls).

Heterogeneity We examine heterogeneous effects by splitting the sample between public- and non-public schools (i.e., we estimate type of school-specific estimates of θ_1). As random allocation to treatment was done separately for public- and non-public schools, the design guarantees that students in treated (non-) public schools have similar characteristics to those in control (non-) public schools.

Robustness For those outcomes for which we have a comparable measure before and after the treatment, we also run Differences-in-Differences (DID) models. Unlike Model (1), DID models estimate the impact of the program by netting out from change in each outcome $Y_{i,s}$ between the pre- and post-treatment period for treated schools the corresponding change among control schools. These models pool the observations in December 2014 and March 2015 and do not include controls for the lagged outcome $Y^0_{i,s}$.[14] As an additional robustness test, we also include student-specific fixed-effects.

[13] For example, there would be some evidence of forgetting the material if students treated in January-March 2015 performed worse in the June test that students treated between April and June.

[14] In particular, the model considered is

$$Y_{i,s,t} = \gamma_0 + \gamma_1 AFTER_t * TREAT_s + \gamma_2 AFTER_t + \gamma_3 TREAT_s + \sum_{k=1}^{k=15} \delta_k X_k + u_{i,s,t} \qquad (2)$$

For all observations in March 2015, $AFTER_t = 1$, and 0 for observations in December 2014. Similarly, for students in the treated schools, we set $TREAT_s = 1$ in both periods, and 0 for students in control schools. The impact of the program is identified as the interaction between $AFTER_t$ and $TREAT_s$. The model also includes dummies indicating the strata of the school. The addition of strata specific dummies

3.3 Outcomes of interest

We describe each outcome of interest in detail.

Performance in standardized tests of financial knowledge A group of educational experts designed a set of around 200 items for a previous evaluation in 2012. The items were multiple choice (single-answer) questions and were designed to determine if students had acquired competences in *Savings and Financial planning*, *Banking relationships* and *Sustainable Consumption*. Questions on *Savings and Financial planning* presented students with a fictional budget (including expected incomes and expenses) and asked about the soundness of the financial situation of that family or the feasibility of reaching certain saving targets in a given period. Questions on *Banking relationships* asked about the characteristics of saving and checking accounts and the meaning of key components of a bank statement. Students were also asked to compute the remaining balance in a checking account at a future date given an expected flow of revenues and expenses and an initial balance or, in other assessments, to compare the return of different savings accounts, taking fees into account. Finally, questions on *Sustainable Consumption* posed fictional situations where a given need could be satisfied in alternative ways. The students were to identify which form was healthier or environmentally friendlier. Based on these questions and on the tests designed for the previous evaluation, we elaborated three different tests of 30 items each (and with two different models). No student faced the same question twice.

Saving choices in hypothetical questions After each test, students were asked about their time preferences and their expectations. Firstly, we asked each student four hypothetical choices between receiving 100€ today and another amount of € (ranging from 120 to 180€) in three weeks or in six weeks. While those questions are designed to measure preferences over time, Krupka and Stephens (2013) document that preference for current income increases in worse times. Hence, those hypothetical choices between

implies that the coefficient γ_1 is the difference between the average change in the school-level outcome $Y_{i,s,t}$ between December 2014 and March 2015 among treated schools (net of the pooled strata-specific average) and the same difference among control schools (again, net of the pooled strata-specific average).

current and future income also capture the market cost of bringing resources to the present. Regarding expectations, students were asked which educational grade did they expect to complete.

The survey, following the 2012 PISA Financial Assessment questionnaire, also contains a few other questions about students' sources of income (work, allowances, occasional sales, etc.) and whether they talk to their parents about economic issues - an indication of saving support at home or social interactions that cause parents to benefit from their children's financial literacy training (Berry et al., 2018, Bruhn et al., 2016, Haliassos et al., 2019).

Saving choices in an incentivized task Finally, we measured the degree of students' patience in June 2015 by conducting an incentivized convex time budget task (CTBT), as in Andreoni and Sprenger (2012). The purpose of that task is to elicit incentive-compatible measures of patience -i.e., recover students' preferences in a situation where their stated choices determine actual payments.

3.4 Compliance

The degree of compliance was measured immediately before the beginning of the course, via surveys addressed to the principal about the plans to teach the course within the school. In addition, we obtained information about implementation details via on-line surveys in March 2015 (for treated schools) and June 2015 (for control schools). 50 teachers in 33 treated schools (out of the 34) answered the March 2015 survey: in 20 of those 33 schools, one single teacher was in charge of the materials, in 9 schools 2 teachers were responsible for the course, and in the remaining 4 schools, 3 teachers. 36 of those teachers implemented the materials in one single group, 10 teachers in two groups, and 4 teachers in three different groups within the same grade and school.

The specialization of teachers was diverse (Table 2). Thirty-two percent of teachers reported Economics as their main specialization, while another thirty-seven percent specialized in other Social Sciences. The median number of hours devoted to the course was

10, and only 25% of students in public schools received less than the recommended 10 hours. About one quarter of students received 16 hours or more. An almost universal comment in teachers assessments was that there was "too much material" to be covered in 10 hours.[15]

The average number of lessons covered was seven out of the ten lessons available.[16] Twenty-one percent of students received the material as part of the social sciences curriculum, twenty percent during the weekly tutorial (a one-hour class where teachers discuss matters related to the educational process and to students' professional prospects), and seventeen percent in maths.

We detected two main forms of noncompliance through surveys and personal contact with the teachers.[17] Firstly, one school assigned to teach the material in January-March 2015 reported having taught the course not in this quarter, but between April-June 2015. Secondly, another treated school delivered some material prior to the pre-test. In what follows, we include these two cases in the analysis so that estimates can be interpreted as intent-to-treat estimates where both non-compliant schools are still considered as treated.

4 Balancing at baseline

Table 3 reports the baseline characteristics of the sample. We present in the first two columns the mean characteristics of treated and control students. The third column shows the p-value of the coefficient of the variable $TREAT$ in separate regressions with the characteristic on the left hand side and stratification dummies as additional covariates.

The fraction of correct answers in the financial knowledge test at baseline, measured by the December pre-test, is remarkably similar across groups: both treated and control

[15] Teachers received no special reward for teaching the course, other than a diploma that they could add to their vita (all teachers but one requested it). While special training for the course was not provided, we organized a 4-hour meeting in November 2014 where implementation details were presented and one of the modules was described and discussed. Traveling and accommodation costs were covered by Banco de España.

[16] Compliance was lowest with the modules devoted to advanced saving vehicles, like pension funds, and insurance products.

[17] The survey mentioned that we understood that many unexpected developments may occur during the academic year, and that - to properly analyze the data - it was crucial reporting any deviation from the protocol.

answered correctly almost 60% of the questions.

With regards to the variables used in the stratification, one third of both treated and control schools are located in Madrid. The share of students in public schools is higher in treated (64.3%) relative to control schools (59.7%), but the difference is not statistically significant.

The fraction of females is slightly higher in control schools than in treated schools (50.6% among controls and 47.5% in the treatment group). The fraction of migrants and grade repeaters (namely, students whose exact age was above what normal grade progression would imply) is higher in treated schools (13.9% and 30.0%, respectively, versus 11.0% and 22.3% in control schools). None of these differences are statistically significant at usual confidence levels.

Regarding attitudes towards saving at baseline, 22% of students never talk to their parents about economics. Similarly, 27% of students report some form of impatience in hypothetical saving choices, as they prefer 100€ today to receiving 120€ in three weeks. Unsurprisingly, the fraction of students who prefer 100€ today falls to 13-15% as the hypothetical payoff increases to 150€ and to 7% when the future payoff is 180€. Treated and control students were very similar in all those dimensions.

Turning to the financial situation of youths, their engagement in income-earning activities is relatively low: 31.7% of treated students and 30.4% of controls report collaborating in the family business or doing home chores for pay. 79% of treated students and 77.1% of controls report receiving (unconditional) allowances. Finally, 20.5% of treated students and 18.4% of controls do some occasional job. In neither of those variables we find significant differences between students in treated and control schools.

Finally, students in treated and control schools are also similar in terms of their parental background. We do not find any significant difference, for instance, in the labor market status neither of the father nor of the mother: 58% of fathers work as employees versus 51-53% of mothers; 26-27% of fathers work as self-employees versus 16% of mothers; 10% of fathers are unemployed versus 9% of mothers, and 5% of fathers do not participate in the labor market relative to 22-24% in the case of mothers.

5 Results immediately after the course (March 2015)

Financial knowledge Panel A in Table 4 presents the impact of the financial literacy course on short-run financial knowledge. Students in treated schools improved their performance in the financial literacy test by 14% of one standard deviation (standard error of .07). The result becomes more precise when we control for dummies indicating the strata the school belongs to in the second column. The last two columns of the table focus on a balanced sample of students (column 3) and join two strata where there were no treated school accepted teaching the course (column 4). Those changes improve precision, but have no noticeable impact on the mean impact on financial knowledge. The magnitude of the improvement is in line with the findings in Bruhn et al. (2016), Hospido et al. (2015) or Walstad et al. (2010), who report a positive impact of financial literacy courses in high schools.[18] Figure 1 presents the cumulative distribution function (CDF) of the fraction of correct answers in the March test of treated and control students. The CDF of scores of treated students is almost everywhere shifted to the right relative to that of controls, indicating an overall improvement in financial knowledge as measured by the test. However, the gains are less obvious at the bottom of the distribution of test scores. We come back to this finding below.

To get further insights about which particular components drive the increase in financial test scores, Model (1) is re-estimated separately for four different subscores: *Savings and Financial planning*, *Banking relationships* (further subdivided into *means of payment* and *relationships with banks*) and *Sustainable Consumption.* The results are shown in Table 5 and indicate that treated students improved mostly on both aspects of *Banking relationships* (scoring 2-3% extra correct answers in those subscores). On the other hand, the impacts on *Savings and Financial planning* and on *Sustainable Consumption* were either small (in the first case) or too imprecise (in the latter).

Panel B in Table 4 analyzes if 10th graders in treated schools could have been affected by the material received by 9th graders -for example, because teachers use the material

[18]The first two studies document increases in test scores of about 20% of one standard deviation. Becchetti et al. (2013) and other studies discussed in Bruhn et al. (2016) find much more limited impacts.

of the course in their courses. The results reject the possibility of spillovers of that sort, as 10th graders in treated schools did not perform any better in the financial literacy test than 10th graders in the control schools.

Panel C in Table 4 examines if the difference in financial knowledge between treated 9th graders and control 9th graders is still present in June 2015, once all 9th graders had taken the course. The average scores in the financial knowledge tests are remarkably similar in June, a finding that is consistent with the hypothesis that 9th graders who received the course between January and March had forgotten little of the material taught three months before.[19]

Hypothetical saving choices Regarding hypothetical saving choices, we document a decrease in the preference for current income among students who went through financial education (Table 6). The dependent variable in each column is a dummy variable of preferring 100€ today (that is, the day of the test) to some other amount in three or six weeks. On the right hand side of the regression we include the *Treated* variable as well as indicators of each choice at baseline and the stratification dummies. Columns (1) and (2) show that among treated students the fraction who choose 100€ today over 120 in either three weeks or in six weeks fell between 4% and 5%, respectively. However, results in columns 3 and 4 imply no changes in the fraction of treated students who prefer 100€ today to either 150€ or 180€ in three weeks. While exposure to financial literacy did not change the fraction of students with a strongest preference for current income, the fraction of earlier choices diminishes when treated students are confronted with relatively low interest rates. Column (5) shows a specification where the responses to all 4 hypothetical choices are pooled (i.e., each student contributes four observations to the regression). Students in treated schools are 2.6 percentage points less likely to prefer income on the day of the test (standard error: 1.2 percent).

The fall in preferences for current income among treated students is consistent with

[19] It could also imply that students going through the course between April and June 2015 learned nothing and that students treated in March had forgotten what was learned. Unreported results compare the results of treated 9th graders in June 2015 to 10th graders in December 2014, finding that 9th graders treated between January and March 2015 performed relatively better than 10th graders in December 2014. However, these results are imprecise.

at least two explanations. The first is that exposure to financial literacy diminishes the true rate of *time preference*. The second is that, for some reason, students feel now less *credit constrained* (Krupka and Stephens, 2013; Carvalho et al., 2016). To disentangle between both explanations, we use information on the students' reported sources of income. Columns (1)-(4) in Panel A of Table 7 detail the impact of the program on each income source. The fraction of treated students reporting income in exchange of tasks at home increases by 4 percentage increase, relative to a baseline of 28%. The fraction of students who report working in the family business increases by 2.5 percentage points - from a baseline of 8%. In addition, column (5) shows the results of a regression where the outcome takes value 1 if the student engages in any income generating activity (i.e., occasional jobs, selling things, obtaining income in exchange of housing tasks, or working for money in the family business). The variable takes value zero if the student only reports sources of income that do not involve an exchange of services, such an unconditional allowance. Students in treated schools are 3.8 percentage points more likely to report sources of income related to the exchange of services, although the estimate is significant at the 7% confidence level only (standard error: 2 percentage points). The increase in labor supply of treated students is consistent with previous findings by Berry et al. (2018), who also document similar results among Ghanaian children following a financial literacy course, but not with those in Lührmann et al. (2018), who focus on disadvantaged German youths.[20]

Panel B of Table 7 reports the impact on the probability of talking to parents about economics. That probability is modeled by an ordered probit where each threshold indicates the frequency showed in each column. Results show that the share of students who talk to parents about economics increased among treated students, relative to controls. The overall impact is driven by the 4 percentage points reduction in the proportion of treated students who never talk to parents about economics. As a result of financial

[20] As a robustness check, Tables W2 and W3 in the Online Appendix re-estimate selected Models in Tables 4, 6 and 7 using DID estimators. The results are similar to those reported earlier in the paper. If anything, when we control for student-specific fixed-effects, the impact of the financial education program on students' report of sources of income is no longer statistically significant at the 10 percent confidence level.

courses, we observe a fall in the fraction of students reporting little interest in financial matters at home.

In summary, the fall in the degree of preference for current income among treated students documented in Table 6 could be due either to an increase in the degree of patience or, alternatively, to a higher availability of resources that make present needs less pressing. The increase in domestic labor supply documented in Panel A of Table 7 suggests that at least part of the decrease in the preference for current income could be associated with an increase in income. We also find that exposure to financial education increases the domestic labor supply of youths (as they become more likely to obtain money in exchange from chores at home or working in the family business) but does not increase *market* labor supply. That is a plausible short-run response because over a three-month horizon, doing household chores or working in the family business are less costly forms of labor supply than searching from jobs outside home or finding goods to sell in the market. Finally, the increase in *domestic* labor supply is also consistent with the change in attitudes reported in Panel B of Table 7, where we documented an increase in the fraction of youths who discuss financial matters at home. A possible explanation is that part of financial matters discussed regards the exchange of services at home.

6 Outcomes in an incentivized saving task (June 2015)

A second measure of time preferences was elicited through an incentivized saving task performed in June 2015, three months after students treated between January and March received the course. Measuring outcomes at various points in time is important to establish how likely it is that attitudes change after exposure to financial education. In addition, thus far we have analyzed hypothetical choices that may not reflect accurately actual preferences if there is not a payoff for responding. Hence, we implemented in June 2015 an incentivized convex time budget task (CTBT), a widely used task where subjects are given the choice of splitting resources between present and future consumption with varying interest rates and maturities.[21]

Students were presented with nine sequential choices asking them to allocate 6€ between payments at various dates and with varying interest rates. It was announced that the payment would take the form of USB memory sticks with different capacities in different moments in time, according to their choices. The choice of that sort of payoff was driven by the consideration that USBs are homogeneous goods whose attractiveness mainly varies along one dimension (storage capacity), and because institutionally it was not possible to use money as payment.[22] We considered it is easier to frame choices of USBs at different moments in time as a saving decision than, for example, presenting students with baskets of goods in different moments in time.[23] The USBs had the logo of the *Finance for All* program and their storage capacity ranged between 2 GB and 32 GB. Choices were framed in their equivalent monetary values.[24] The choices were then between obtaining one (or more) USBs with a given storage capacity in the day of the task or a set of USBs with a larger capacity one or two weeks later.

Given the limited period of time imposed by the end of the academic year, we chose very large interest rates: 100%, and 200%. The students had to allocate payoffs between: (i) the day of the task (today) and one week from that date (Sheet 1 in Table W4 in the Online Appendix), (ii) the day of the task (today) and two weeks from that date (Sheet 2), and (iii) between one and two weeks from the day of the task (Sheet 3). After the application, one of the nine choices were chosen at random and one randomly chosen student in the group would be awarded her choice. When the student's choice involved obtaining some USB in one or two weeks' time, the payoff was given to the teacher in an envelope with the delivery date written on it.

[21]By allowing subjects to allocate resources partly to present and future consumption, convex time budgets circumvent the problems that arise when subjects must choose between the dichotomous choice of consuming today or in the future, as it was the case in the hypothetical questions in the March survey.

[22]Lührmann et al. (2018) discuss that monetary payoffs only recover preferences for time if subjects in this kind of experiments incur in *narrow bracketing* (i.e., if they consider payoffs in the experiment separately from their own resources at home). We think that those concerns are less likely to apply when using USBs instead of money.

[23]While the choice of USBs as a payoff implies that our results must be interpreted as preferences over time for this particular durable good, Table 8 documents a positive correlation between the actual choices in the incentivized task and the baseline preferences for time in hypothetical choices.

[24]We assigned to each USB a value in €similar to market prices at the time of the task. In that manner, one 8GB USB would be equivalent to 6€, and one 32GB USB was presented as 12€.

As by June 2015 all students had already gone through the course, in this experiment we use 10th graders as the control group.[25] While the median age of the control group is one year older than that treated students, other comparisons suggest that 9th and 10th graders were similar. According to Panel A in Figure 2, when asked in December 2014 about their hypothetical preferences for receiving 100€ on the day of the survey or 120€ three weeks later, 23% of 9th graders treated between January and March 2015 preferred 100€ on the survey date, while the corresponding number among 10th graders was 27% (the difference is not statistically significant). When the payoff for waiting three weeks was increased to 150€, the fraction of treated 9th graders who chose the sooner hypothetical payment fell to 12%, and to 6% when the future payoff increased to 180€. The corresponding behavior among 10th graders was remarkably similar, which reassured us in the use of this group as a control for 9th graders. The patterns are similar if we compare 9th graders treated between April and June 2015 to 10th graders (Panel B in Figure 2).

Figure 3 plots the amount that treated and control students in Madrid allocated to the earlier date. Panel A compares 9th graders treated between January and March 2015 (early treatment students) to the full group of 10th graders, while Panel B compares 9th graders treated between April and June 2015 (late treatment students) and 10th graders. The estimates are population means, unadjusted by strata composition.[26] According to Panel A in Figure 3, 9th graders treated between January and March 2015 allocated a lower amount to earlier payments than the group of controls did. When the rate of return between the day of the task and one week was 100%, treated students allocated to the sooner payment 29 cents less than controls did (.29 =.99-.70). The difference between treated and controls is smaller (.07=.72 - .65) when the return to saving increases to 200% (that is, each euro saved today results in three extra euro in one week).[27] When

[25] We only report the results for schools in Madrid because budgetary reasons prevented us from implementing the CTBT among 10th graders elsewhere.

[26] Table 3 examines these patterns in a regression format controlling for strata dummies and the preference for current income in the December 2014 test.

[27] This pattern is qualitatively similar to that detected in the hypothetical choices in Table 6, where early consumption choices are less common among treated students for intermediate values of the interest rate, but the case is less clear when interest rates are very large.

the time comparison was between today and two weeks and the rate of return was 100%, control students allocated 1.36€ to the earlier payment, while treated students allocated 42 cents less (.42=1.36-.94). Even when the interest rate increases to 200% over two weeks, treated students allocated to the sooner payment 25 cents less than controls (.25=1.10-.85). Finally, the differences between treated and control students in the one vs two weeks experiment are qualitatively similar to those between today and one week. For each choice, students receiving the course in April-June allocated less cents to the earlier date than controls (Panel B in Figure 3), but the magnitude is much *lower* than relative to the early treatment students (Panel A).

Figure 3 also shows that control students behave according to the revealed preference theory, that is, euros allocated to earlier date decrease when the interest rate increases. When the return on each euro saved is 100% (i.e., one euro saved increases consumption possibilities in one week by two euros) students in the control group allocated .99€ out of the 6€ to the sooner payment, while they allocated .72€ if the interest rate is 200%. In addition, the amount allocated to sooner payment is not significantly higher when the decision is made on the day of the experiment than when it is one week from now, contrary to the hypothesis of present bias.

Table 8 summarizes the results of the incentivized saving task in a regression format. The dependent variable in column (1) is the euros allocated to the earlier payment, while the main independent variable is an indicator of being a 9th grader in the set of schools that received the financial literacy course between January and March 2015 (i.e., 9th graders receiving the course between April and June 2015 are not included in this regression). We also include as regressors the interest rate in each choice, the lag between payments, three indicators with the strata the school belongs to and indicators expressing preference for sooner hypothetical payments in December 2014.[28] Across all choices, students receiving the material in January-March 2015 chose in June 2015 allocations

[28]The base category reflects the euros chosen in the earlier date by students in public schools requesting the material before September 2014 (stratum 1 in Table A.1) who prefer 120 euro in two weeks to 100 euro today. Inclusion of choices at baseline improve precision, but do not alter the magnitude of the coefficients. In this case, we cluster standard errors at the school-grade level, because 10th graders are conceptually a separate control group for 9th graders. We experimented clustering at the school level, and the standard errors were very similar.

that involved 27 cents lower early consumption than controls. The 27 cents reduction of the amount allocated to the sooner payment amounts to 18% of one standard deviation of the euros allocated to the sooner date (1.49€, bottom of Table 8).

Column (2) in Table 8 compares the euros allocated to the sooner payment by students who went through the material between April and June 2015 to those chosen by the full group of 10th graders as controls. In this case, treated students also reduced the number of euros allocated to the earlier date, but the magnitude of the reduction is 12 cents, half that of the January-March group and not statistically different from zero.

Several studies establish that a violation of the law of demand occurs if, for a given time horizon and initial choice, a student chooses to allocate *more* resources to the sooner payment when interest rate *increases*. Namely, if students choose an allocation of current and future consumption for a given interest rate R, an increase in that interest rate R cannot make saving less attractive, so students should not increase the amount allocated to the sooner payment. Using that definition, 11% of the choices of students in the control group can be considered an optimization error or inconsistent choice. Columns (4) and (5) in Table 8 examine whether financial education improved the quality of students' financial decision-making by regressing an indicator for those errors on the dummy $TREAT$, strata dummies, the interest rate in each choice, and the lag between payments. We find little evidence that financial education reduces the probability of making such errors.

Finally, columns (6)-(9) in Table 8 re-examine the impact of financial education on the euros allocated to the sooner payment in a sample without inconsistent choices. The results are qualitatively similar to those shown in columns (1)-(3), but more precise.

Summing up, students treated in January-March 2015 displayed more patient choices than controls at various interest rates and maturities, the results being more imprecise for students treated in April-June 2015. While we do not have a good explanation for the lower response of the group that received the material later, our results do not support the notion that the impact of financial literacy programs on preferences vanishes three months after the program took place.

7 Differential responses by type of school

One of the features of our study is the wide variation in the characteristics of students participating in the program. The research design took advantage of this variation and randomized treatment by type of school, a feature that correlates strongly with parental characteristics. As shown in Panel A of Table A.2, students in public schools are more likely to be born outside Spain (14% vs. 8% in non-public schools), to have repeated a grade (28% vs. 17% in non-public schools) and to expect leaving education earlier (72% expects to finish college in public schools vs. 82% in non-public schools). Furthermore, students in public schools are more likely to face worse economic conditions, with a higher proportion of fathers who don't work (17% vs 11% in non-public schools). In this section, we partition the sample between strata with public schools and non-public ones.[29] Panel B of Table A.2 reports the corresponding balancing tests between treated and control students within each subsample. As expected, students in treated (non-) public schools have similar characteristics to those in control (non-) public schools.

Panel A in Table 9 presents the effect of the financial literacy program on normalized tests scores in March 2015. Relative to controls of the same type of school, treated 9th graders in either public or non-public schools experience similar mean increases in the financial test score: about 18% of one standard deviation.[30] However, the distribution of the responses differs across schools. Figure 4 shows the predicted CDF of the fraction of correct answers of treated and control students in each type of school. In public schools, the fraction of treated students achieving low scores -between 25% and 50% of correct answers- fell by around 5 percentage points relative to the control group. Conversely, for non-public schools, the distribution of low scores is very similar among treated and control students while the main increase in test scores is due to changes in the upper part of the distribution. For example, the fraction of treated students in public schools

[29]Namely, public schools are those in strata 1, 4, 7, 9, 11 and 16 in Table A.1. Non-public schools include strata 2, 3, 5, 6, 8, 10, 12 and 13. Strata 14 and 15 were not used in that partition, as they mixed public and non-public strata. We have also experimented with finer partitions of the strata, interacting region (Madrid vs rest) and type of school, but the number of schools in some of the strata would be too small to conduct appropriate inference. We end up using 42 public schools and 32 non-public schools.

[30]Note that the sample does not coincide with that in Table 4, as strata 14 and 15 are not used in Table 9.

answering correctly less than 25% or 35% of the questions fell by between 4.4% or 6.1%, respectively, while the same fraction remain unaltered in private schools (Table 9, Panel A, rows 2 and 3). In other words, financial education shifted upward the distribution of low scores in financial tests in public schools, but not in non-public ones.

Table A.3 examines the issue more closely by further splitting the sample by the level of accumulated human capital (having repeated a grade or not) and by expected schooling (whether or not expect to leave after high school). Both groups (grade repeaters and students expecting to drop out early) performed relatively worse in the pre-test. The results in Table A.3 confirm that, after the course, treated students with a lower level of human capital improved more in the financial test than the rest of students. For example, treated grade repeaters performed 28% of one standard deviation better than grade repeaters in the control group. In contrast, treated non-repeaters outperformed non-repeater controls in the financial test by 10% of one standard deviation (i.e., the magnitude of the response is about one third than that of repeaters). Treated students who expect to drop out right after upper secondary school (if not earlier) outperform controls who expect to drop out by that age by 20% of one standard deviation. The improvement in test scores among treated students who expect to finish college is 16% of one standard deviation. Those differences may provide an explanation for the improvement in low scores detected in public schools, as those schools concentrate a much higher share of grade repeaters or students expecting to drop out early.

Panel B in Table 9 presents the effect of the financial literacy program on hypothetical saving choices in March 2015. Treated students in both strata reported to be less likely to choose income today relative to controls. These responses are largest in absolute value and most precisely estimated in the sample of non-public schools[31]. According to Table A.4, treated students in non-public schools reported a higher probability of receiving any source of labor income income (due to sources of income from the family) and of talking to their parents (possibly linked to an exchange of services for money).[32]

[31] However, a test of equality of coefficients cannot reject the null that the coefficient of treated is the same in public and non-public schools.

[32] A possible explanation for why domestic labor supply and communication with parents increase the most in the strata with highest parental income is presented in Weinberg (2001). He builds a principal-

Figures 5 and Figure 6 further illustrate the heterogeneity of responses by strata in the incentivized saving task performed in June 2015. Figure 5 compares the amount (in €) allocated to the sooner payment in public and non-public schools separately by early treated students and controls. For each interest rate and delay, the gap between the amount allocated to the earlier payment by treated and controls in public schools is larger than the corresponding gap in non-public schools. For example, when there was a two-week delay between payments and an interest rate of 100%, treated students allocated 53 cents less to the sooner date than controls in public schools. Among students in non-public schools the corresponding difference was 14 cents, a response four times smaller. Qualitatively similar results hold when we compare students treated between April and June 2015 and controls (Figure 6). Panel C in Table 9 shows the results in regression format. Students in public schools treated between January and March allocated 36 cents less to the sooner payment than controls, while the corresponding difference among students in non-public schools is 8 cents. While we cannot reject the null of equal coefficients, the results suggest that when we measure time preferences using an incentivized saving task, exposure to financial education changed financial decision-making of students in strata with a relatively poorer background.

Overall, the responses to financial education largely vary by type of school. Regarding financial knowledge, similar mean responses to financial education mask rather different shifts in the distribution of test scores. In particular, financial education diminished the fraction of low-achievers in financial knowledge in public schools but not in non-public ones. That is a novel result, as grade repeaters are typically very hard to motivate.[33] Secondly, treated students in the strata with the poorest parental background opt for more patient choices in incentivized saving tasks than controls, although the evidence here is more imprecise.

agent model of the interaction between parents and young children predicting that, unlike the poor, financially better-off families are able to offer monetary incentives to their young offspring in exchange of services.

[33] For example, in the context of the math curriculum mandate, Cole et al. (2016) document that exposure to those mandates increases income in adulthood among whites, but not among non-whites.

8 Interpretation of the results

The results in our study are consistent with the notion that financial education changes the awareness of students about the value of resources and the future consequences of current choices. Supporting evidence in this respect comes from the analysis of financial knowledge tests, where students mainly improved in their understanding of the workings of simple banking products, such as the functioning of savings and borrowing products that make more salient the opportunity cost of transferring resources over time. In addition, the results from surveys suggest that financial education changes student's attitudes through the budget constraint (as treated subjects report receiving more family income in exchange of chores after the course) and an increased preference for time. The latter result is observed in hypothetical choices between current and future income measured right after the course and three months afterwards, using an incentivized saving task. Those results are in line with those in Alan and Ertac (2018), who analyze an intervention aimed at emphasizing the future consequences of current actions that increases the degree of patience of much younger children. There are however two main differences between their study and ours. Firstly, we study 15 year-olds, who have extra alternatives to raise money to achieve higher income -money in exchange of chores, working in the family business and so on. A second difference is that youths at 15 years of age take crucial decisions related to their academic career, like continuing in the educational system or dropping out. Interventions at this critical age may have impacts that go beyond the time frame we study, specially if they mainly affect students with a lower motivation.

How does financial education change individuals' financial knowledge or behavior? The results from the program we analyze do not suggest that financial education improve their intertemporal decision-making by making less present-oriented choices. Were that the case, students would have reduced the euros allocated to sooner payments mainly when these are received the very same day of the task, because present-bias does not affect choices between future and distant future consumption (Carvalho et al., 2016) or Lühmann et al. (2016). Instead, the fall in the euros allocated to the sooner payment is similar when the decision is made on the date of the first payment or in one week from now. These results depart from Lührmann et al. (2018), who document that students receiving a short financial education did not change their degree of patience in a task using monetary payoffs.[34]

In terms of who is affected by financial education, several models predict that the incentive to invest in learning about finance increases with wealth (Lusardi et al., 2017, or Jappelli and Padula, 2013). According to those studies, if the alternative to receiving high school education was receiving information at home, we should expect larger impacts of financial courses among students with low-wealth parents, as they are unlikely to have invested in financial education themselves. The finding that the distribution of financial knowledge becomes more equal in public schools after the course, coupled with a larger increase in knowledge among students who have either repeated a grade or who expect to drop out earlier is indeed consistent with higher impacts among students with less favorable background. In addition, the increase in the preference for time in incentivized tasks among students in public schools is also consistent with the notion that financial education increases awareness of the value of waiting among students with a weakest background -at least in the period we consider.[35]

A final note is that judging the success or not of a program by whether it changes the preferences of students may seem paternalistic and arguably outside the realm of what financial education should do (Ambuehl et al., 2016). We note that a substantial fraction of students in our sample are performing poorly (28% have repeated a grade in public school) or expect leaving school early (17% of students in public schools plan to leave school without any degree of professional or academic specialization). Arguably, some of those choices could be considered short-sighted and could benefit from a reassessment of the future consequences of current choices.

[34] A number of reasons may explain the difference. One possibility is that they focus on a sample of students with a disadvantaged background, while our sample is more heterogeneous. However, we find responses in patience even in the strata of students with a poorer background. An additional reason for the discrepancy can be the fact that the payment structure in their experiment (money) differs from ours (USBs). Money is arguably likely to be more prone to produce a present bias than USBs are.

[35] The responses to financial education in non-public schools is different. Financial education makes these students become more likely to increase their labor supply at home. That heterogeneity in responses is consistent with models of the family predicting that, unlike parents in worse financial situation, wealthier parents provide monetary incentives to their young children (Weinberg, 2001).

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Tables

Table 1: Evaluation calendar

	December 2014		March 2015		June 2015
9th graders (15 years of age)					
1. Treated schools	Pre-test and baseline survey	FL course	Post-test and survey to students	No course	Third test and incentivized saving task
2. Control schools		No course		FL course	
10th graders (16 years of age)					
1. Treated schools	Pre-test and baseline survey	No course	Post-test and survey to students	No course	Incentivized saving task*
2. Control schools		No course		No course	

*Notes: * Saving task conducted only in Madrid schools. In November 2014 all teachers were invited to Banco de España for a session on the purpose of the evaluation, timetable of the course and going over one of the lessons.*

Table 2: Program Implementation

	Total N=1,228	Public N=762	Concerted N=425	Private N=41	Concerted ir private N=466
Number of hours:					
Minimum	4	4	9	15	9
25th percentile	10	8	10	15	10
Median	10	10	11	17	15
75th percentile	18	16	20	17	20
90th percentile	20	20	22	17	22
Number of lessons taught (out of 10)	7.4	7.0	8.1	7.0	8.0
Fraction that made independent evaluation	0.37	0.35	0.39	0.51	0.40
Fraction that assigned homework	0.31	0.29	0.39	0.00	0.35
Subject where material was delivered:					
Maths	0.17	0.08	0.24	1.00	0.31
Social Sciences	0.21	0.17	0.31	0.00	0.28
Weekly hour with tutor	0.20	0.28	0.07	0.00	0.06
Citizenship	0.11	0.15	0.05	0.00	0.05
Alternative to religion	0.10	0.12	0.08	0.00	0.07
Other	0.22	0.20	0.26	0.00	0.24
Teacher's specialization:					
Social Sciences	0.37	0.43	0.31	0.00	0.28
Economics	0.32	0.37	0.20	0.49	0.22
Maths	0.12	0.08	0.16	0.51	0.19
Computing science	0.09	0.00	0.26	0.00	0.24
Other	0.10	0.12	0.08	0.00	0.07

Source: on-line surveys to 50 teachers in 33 schools that taught the course between January-March 2015.

Notes: the unit of analysis are the 9th graders that were taught by those 50 teachers (in total 1,228 students).

Table 3: Balancing tests at baseline

	Treated (34 schools)	Control (43 schools)	p-value of the difference
Fraction of correct answers in pre-test	0.591	0.596	0.784
Variables used in the stratification:			
Madrid	0.324	0.303	0.233
Public school	0.643	0.597	0.324
Concerted school	0.325	0.302	0.113
Private school	0.032	0.101	0.262
Concerted/private	0.357	0.403	0.324
Demographic characteristics:			
Female	0.475	0.506	0.131
Foreign born	0.139	0.110	0.377
Older than normal progression	0.300	0.223	0.191
Expected age to finish school	21.088	21.413	0.093
Expects to finish at 18 or earlier	0.178	0.140	0.229
Hypothetical preferences:			
Prefers 100 euro today to 120 in 3 weeks	0.273	0.273	0.877
Prefers 100 euro today to 150 in 3 weeks	0.152	0.127	0.224
Prefers 100 euro today to 180 in 3 weeks	0.072	0.073	0.781
Sources of income:			
Family business/allowance home duties	0.317	0.304	0.681
Unconditional allowances	0.790	0.771	0.135
Occasional jobs	0.205	0.184	0.328
Talk to parents about economics:			
More than once a week	0.220	0.221	0.280
Once a week	0.217	0.221	0.948
Less than once a week	0.305	0.313	0.957
Never	0.258	0.246	0.380
Labor status of father:			
Self-employed	0.264	0.274	0.881
Employee	0.578	0.576	0.644
Unemployed	0.104	0.096	0.611
Other	0.054	0.054	0.806
Labor status of mother:			
Self-employed	0.161	0.158	0.600
Employee	0.513	0.530	0.863
Unemployed	0.090	0.090	0.490
Other	0.236	0.221	0.927

Source: information about demographics comes from the December survey to students. Information about grade repetition (date of birth) comes from school records.
Notes: Sample of 3,050 9th graders in 77 schools. Students with special educational needs or who did not take the December test are excluded.

Table 4: The effect of the financial literacy program on normalized tests scores

	Unbalanced panel		Balanced panel	
	No strata	Strata dummies	Strata dummies	Strata dummies†
	(1)	(2)	(3)	(4)
Panel A: Treated students vs controls (9th graders). March				
Treated	0.136**	0.157**	0.169**	0.183***
	(0.067)	(0.070)	(0.066)	(0.062)
Fraction correct in pre-test	0.532	0.532	0.596	0.596
R^2	0.158	0.129	0.330	0.332
Number of students (schools)	3,025 (77)	3,025 (77)	2,696 (77)	2,696 (77)
Panel B: Non-treated students in treated schools vs those in control schools (10th graders). March				
"Treated"	-0.085	-0.038	-0.083	-0.095
	(0.090)	(0.096)	(0.092)	(0.086)
R^2	0.29	0.31	0.35	0.35
Number of students (schools)	1,545 (77)	1,545 (77)	1,346 (77)	1,346 (77)
Panel C: Treated students vs controls (9th graders). June				
Treated	-0.080	-0.062	-0.055	-0.047
	(0.084)	(0.074)	(0.073)	(0.067)
R^2	0.27	0.30	0.34	0.34
Number of students (schools)	2,682 (77)	2,682 (77)	2,398 (77)	2,398 (77)

*Notes: the dependent variable is the normalized score in the March 2015 (or June 2015) test. All models include as covariate the score in the December pre-test. Models (2) and (3) include strata dummies. †Model (4) merges two strata where no school assigned to treatment accepted to participate. Estimation method: OLS. The standard errors (in parentheses) are corrected for heteroscedasticity and arbitrary correlation at the school level. * significant at 10%, ** significant at 5%, *** significant at 1%.*

Table 5: The effect of the financial literacy program on the March tests scores by topic

	Savings and Financial planning	Means of payment	Banking relationships	Sustainable consumption
	(1)	(2)	(3)	(4)
Treated	0.000	0.023*	0.032***^^	0.029
	(0.014)	(0.012)	(0.010)	(0.018)
Score in the pre-test	0.737***	0.553***	0.422***	0.809***
	(0.033)	(0.031)	(0.019)	(0.048)
Constant	0.096***	0.322***	0.054***	0.462***
	(0.023)	(0.028)	(0.015)	(0.044)
R^2	0.186	0.155	0.245	0.141
Number of students (schools)	2,696 (77)			

*Notes: The dependent variable is the fraction of correct answers in the March test, and it is not normalized by its standard deviation. The estimation method is OLS, and all models control for stratification dummies (stratum 1 excluded). The standard errors (in parentheses) are corrected for heteroscedasticity and arbitrary correlation at the school level. * significant at 10%, ** significant at 5%, *** significant at 1%. ^significant at 10%, ^^significant at 5%, ^^^significant at 1%, after correcting p-values for multiple testing using the Romano and Wolf (2016) correction. Correction for multiple testing implemented for all topics together.*

Table 6: The effect of the financial literacy program on hypothetical saving choices

Prefers:	100€ today to 120 in 3 weeks	100€ today to 120 in 6 weeks	100€ today to 150 in 3 weeks	100€ today to 180 in 3 weeks	Earlier choice† (pooled)
Treated	-0.041**^^	-0.049**^^	-0.009	-0.006	-0.026**^^
	(0.018)	(0.020)	(0.014)	(0.008)	(0.012)
Mean dependent variable	0.28	0.64	0.12	0.06	
Number of students (schools)	2,690 (77)	2,690 (77)	2,689 (77)	2,690 (77)	10,759 (77)

*Notes: All models estimated by OLS, including stratification dummies and lagged values of a similar hypothetical choice in December 2014. †Earlier choice pools the four hypothetical choices and controls for three dummies that indicate the particular temporal choice. The variable treated measures to what extent those who received the course between January and March tend to choose to receive the hypothetical payment earlier, regardless of the time horizon and the interest rate. Standard errors (in parentheses) are clustered at the school level. * significant at 10%, ** significant at 5%, *** significant at 1%. ^significant at 10%, ^^significant at 5%, ^^^significant at 1%, after correcting p-values for multiple testing using the Romano and Wolf (2016) correction. Correction for multiple testing implemented for all hypothetical saving choices together.*

Table 7: The effect of the financial literacy program on attitudes

Panel A: Sources of income					
	Occasional jobs in the market	Selling things (online, street markets)	Money for tasks at home	Work in family business	Any source of labor income
Treated	0.002	-0.011	0.037*	0.025**^	0.038*
	(0.013)	(0.009)	(0.019)	(0.010)	(0.021)
Mean dependent variable	0.16	0.12	0.28	0.08	0.46
Number of students (schools)		2,690 (77)			2,690 (77)
Panel B: Talks to parents about economics					
	More than once a week	Once a week	Less than once a week	Never	Overall†
Treated	0.017	0.027	-0.005	-0.039**^	0.121**^
	(0.017)	(0.018)	(0.015)	(0.018)	(0.054)
Mean dependent variable	0.26	0.25	0.29	0.20	
Number of students (schools)		2,690 (77)			2,690 (77)

*Notes: All models estimated by OLS, including stratification dummies and lagged values in December 2014, except † Overall that is the latent index coefficient of an ordered Probit, with outcomes from never to more than once a week. Standard errors (in parentheses) are clustered at the school level. * significant at 10%, ** significant at 5%, *** significant at 1%. ^significant at 10%, ^^significant at 5%, ^^^significant at 1%, after correcting p-values for multiple testing using the Romano and Wolf (2016) correction. Correction for multiple testing implemented for all outcomes in panel A together (sources of income), and separately for all outcomes in panel B together (talks to parents about economics).*

Table 8: The effect of the financial literacy program on euros allocated to sooner payment in the incentivized saving task

Dependent variable:	€ allocated to sooner payment			Probability of inconsistent choice		€ allocated to sooner payment, consistent choices only		
Treatment sample:	Jan.-March	April-June	All	Jan.-March	April-June	Jan.-March	April-June	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	-0.269*	-0.117	-0.178	-0.018	-0.024	-0.222**^	-0.050	-0.115
	(0.148)	(0.108)	(0.114)	(0.029)	(0.020)	(0.090)	(0.068)	(0.073)
Interest rate	-0.204***	-0.263***	-0.225***	0.062***	0.043***	-0.367***	-0.354***	-0.347***
	(0.041)	(0.028)	(0.029)	(0.011)	(0.006)	(0.039)	(0.029)	(0.027)
Immediate payment	-0.306***	-0.247***	-0.254***	-0.008	0.008	-0.281***	-0.261***	-0.266***
	(0.043)	(0.049)	(0.039)	(0.015)	(0.013)	(0.047)	(0.048)	(0.040)
Delayed payment	0.313***	0.358***	0.320***	0.021	-0.010	0.259***	0.400***	0.320***
	(0.058)	(0.049)	(0.047)	(0.014)	(0.012)	(0.054)	(0.037)	(0.044)
Prefers 100€ today to 120€ in 3 weeks	0.320**	0.555***	0.436***			0.420***	0.529***	0.462***
	(0.155)	(0.094)	(0.098)			(0.127)	(0.094)	(0.087)
Prefers 100€ today to 150€ in 3 weeks	-0.024	-0.162	-0.041			-0.045	-0.176	-0.090
	(0.308)	(0.184)	(0.191)			(0.205)	(0.151)	(0.139)
Prefers 100€ today to 180€ in 3 weeks	0.173	0.534***	0.264			0.116	0.423**	0.202
	(0.387)	(0.185)	(0.225)			(0.287)	(0.174)	(0.173)
Sample size	3,510	4,272	5,976	3,510	4,272	3,059	3,766	5,264
Standard deviation dependent variable	1.49	1.54	1.51	0.33	0.32	1.17	1.26	1.21
R^2	0.03	0.05	0.04	0.01	0.01	0.07	0.08	0.06

*Notes: Sample of 996 students from 20 schools in Madrid doing the incentivized saving task in June 2015 and present in the test of December 2014. Controls are always 10th graders. OLS regressions using as the dependent variable the amount in € allocated to sooner payment (columns 1, 2, 3, 6, 7, and 8) and an indicator of choice inconsistent with revealed preference, if euros allocated to earlier date increase when interest rate increases (columns 4 and 5). Stratification dummies included. Standard errors (in parentheses) are clustered at the school-grade level. * significant at 10%, ** significant at 5%, *** significant at 1%. ^significant at 10%, ^^significant at 5%, ^^^significant at 1%, after correcting p-values for multiple testing using the Romano and Wolf (2016) correction. Correction for multiple testing implemented separately for columns 1, 4, and 6 together; columns 2, 5, and 7 together; columns 3 and 8 together.*

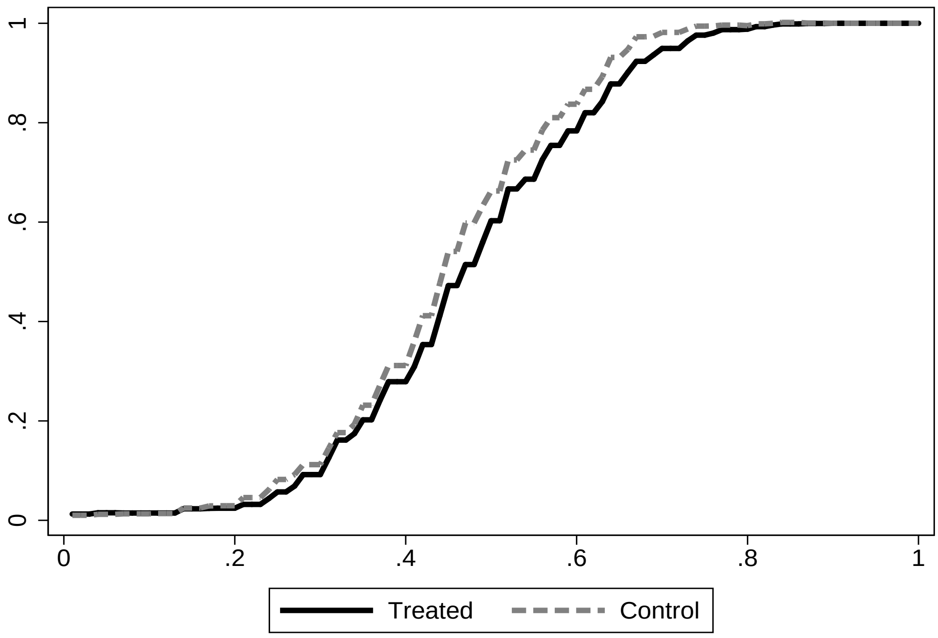
Table 9: The effect of the financial literacy program by strata

	Public	Non-public	p-value of the difference [Adjusted p-value]
Panel A: Financial knowledge (March 2015)			
1. Normalized tests scores	0.187**	0.179**^	0.958
	(0.092)	(0.074)	[0.998]
2. Fraction of students with less than 25% questions correct	-0.044***^^	-0.000	0.018
	(0.016)	(0.008)	[0.194]
3. Fraction of students with less than 35% questions correct	-0.061**	0.019	0.040
	(0.029)	(0.025)	[0.253]
4. Fraction of students with less than 50% questions correct	-0.054	-0.088**^^	0.511
	(0.037)	(0.035)	[0.844]
Panel B: Hypothetical saving choices (March 2015)			
5. Earlier choice (pooled)	-0.017	-0.038**^	0.373
	(0.017)	(0.016)	[0.791]
Panel C: Actual saving choices (June 2015)			
6. Euros allocated to sooner payment (early treatment students)	-0.356*	-0.078	0.295
	(0.197)	(0.135)	[0.486]

*Notes: each cell reports the estimate of the variable Treated in a regression where the dependent variable is shown in the row and covariates include the lagged dependent variable and strata dummies. All specifications estimated by OLS. Standard errors (in parentheses) are clustered at the school level. * significant at 10%, ** significant at 5%, *** significant at 1%. ^significant at 10%, ^^significant at 5%, ^^^significant at 1%, after correcting p-values for multiple testing using the Romano and Wolf (2016) correction. Correction for multiple testing implemented for outcomes 1-5 together, and separately for outcome 6.*

Figures

Figure 1: Cumulative distribution function (CDF) of the raw scores (March 2015)

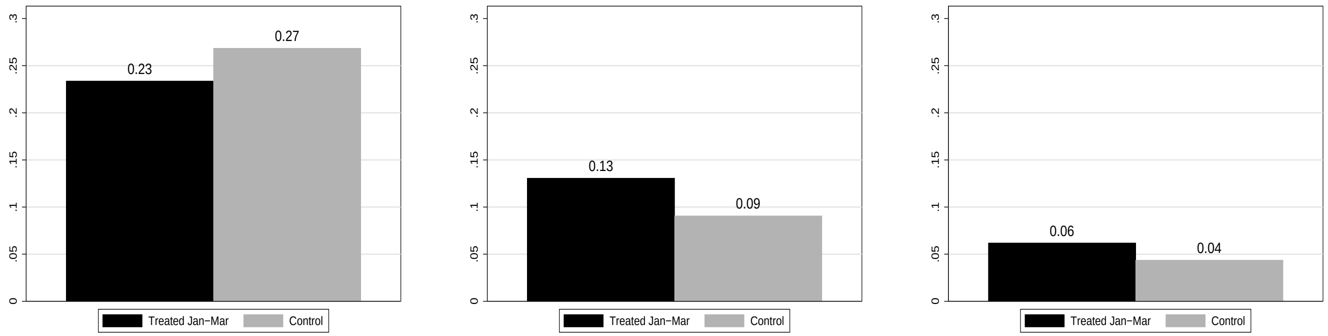


Notes: The horizontal axis shows the fraction of correct answers, while the vertical axis contains the fraction of students. Each point is the predicted proportion of students with correct answers that are equal or below the value in the horizontal axis. Predictions are obtained from OLS regressions of the proportion of students with correct answers equal or below each value in the horizontal axis on treated, the pre-test score and strata dummies (stratum 1 excluded).

Figure 2: Fraction of treated and control students who choose the earlier payment in hypothetical choices between current and future income at baseline (December 2014)

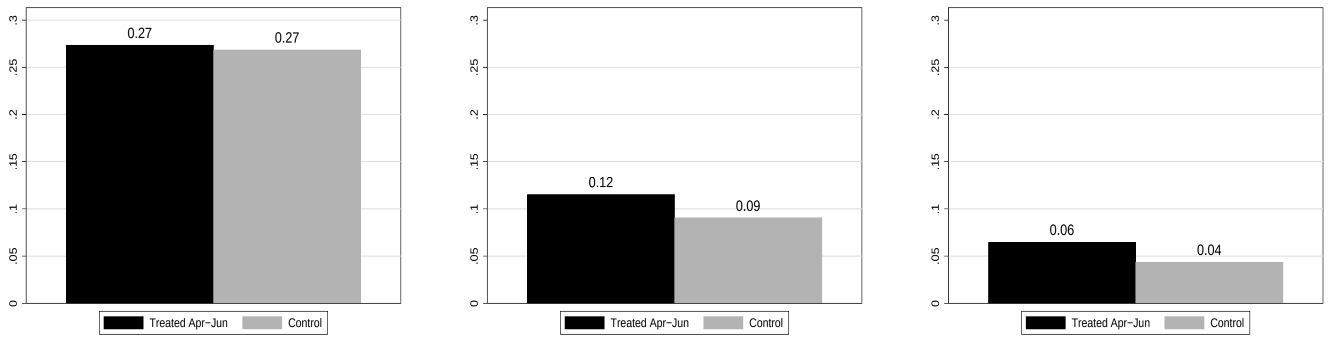
Panel A. Treated in January-March (9th graders) and controls (10th graders)

100€ today *vs.* 120€ in 3 weeks   100€ today *vs.* 150€ in 3 weeks   100€ today *vs.* 180€ in 3 weeks



Panel B. Treated in April-June (9th graders) and controls (10th graders)

100€ today *vs.* 120€ in 3 weeks   100€ today *vs.* 150€ in 3 weeks   100€ today *vs.* 180€ in 3 weeks

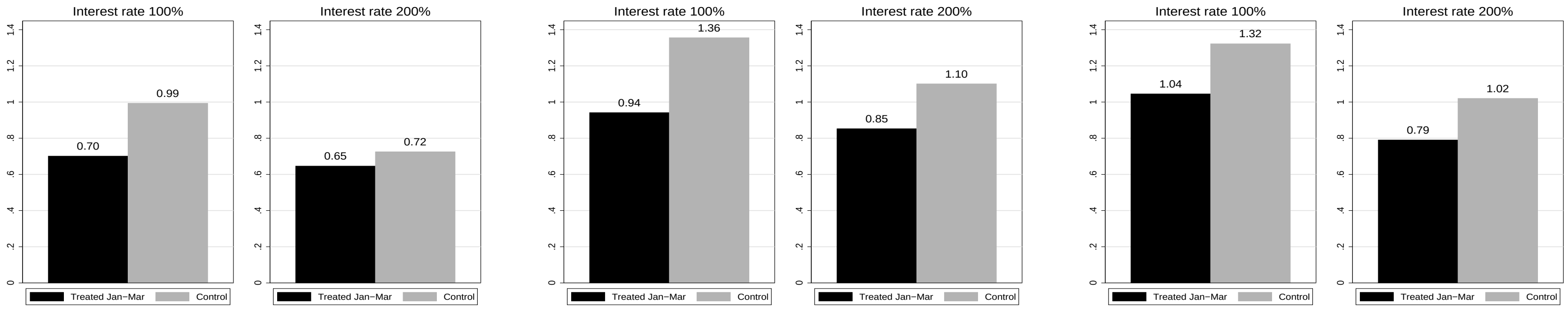


Notes: In panel A (B), treated students are 9th graders in Madrid receiving the course between January and March (April and June). Controls are all 10th graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.1). The black (gray) bars represents the fraction of 9th (10th) graders choosing 100€ today in each choice. Estimates are sample means, unadjusted by covariates or strata dummies.

Figure 3: Euros allocated to sooner payment in the incentivized saving task (June 2015)

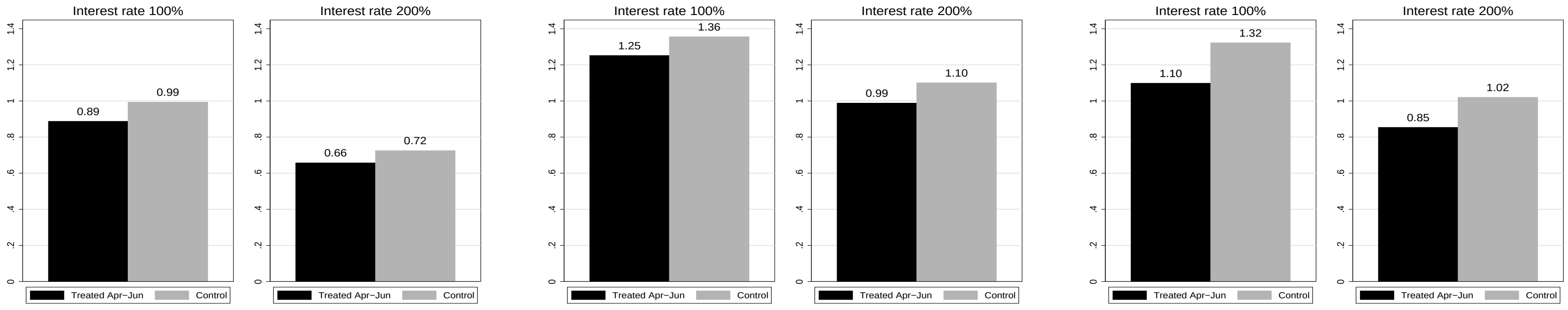
Panel A. Treated in January-March (9th graders, early treatment students) and controls (10th graders)

Today *vs.* One week | Today *vs.* Two weeks | One week *vs.* Two weeks



Panel B. Treated in April-June (9th graders, late treatment students) and controls (10th graders)

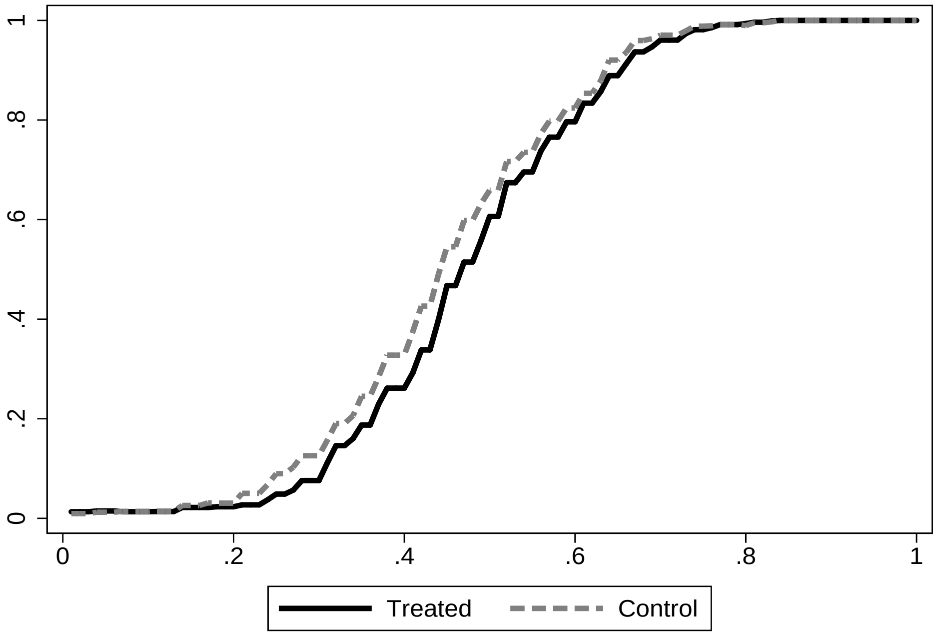
Today *vs.* One week | Today *vs.* Two weeks | One week *vs.* Two weeks



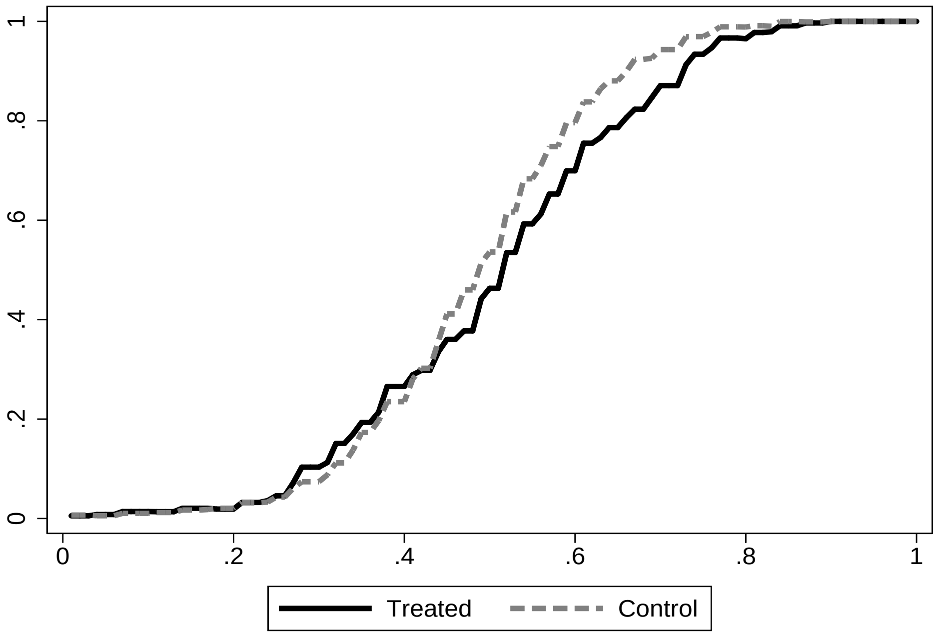
Notes: In panel A (B), treated students are 9th graders in Madrid receiving the course between January and March (April and June). Controls are all 10th graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.1). Estimates are means, unadjusted by covariates or strata dummies. Table 8 shows adjusted estimates.

Figure 4: CDF of the raw scores by strata (March 2015)

Public schools



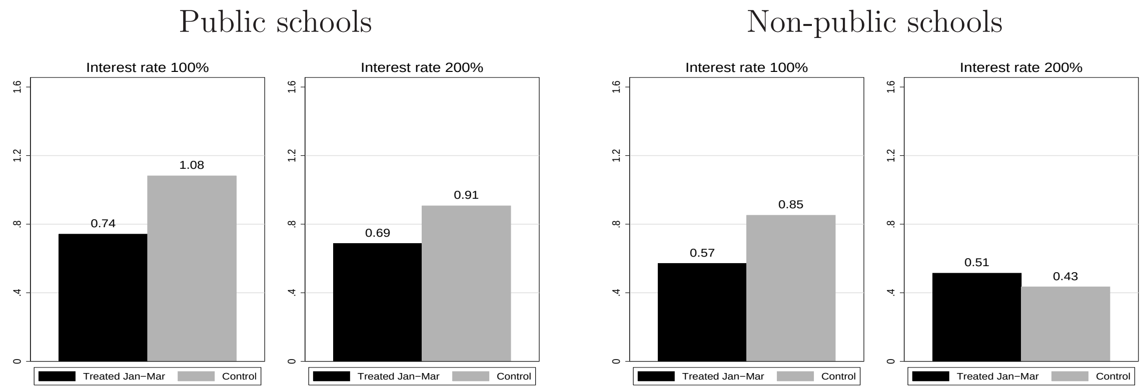
Non-public schools



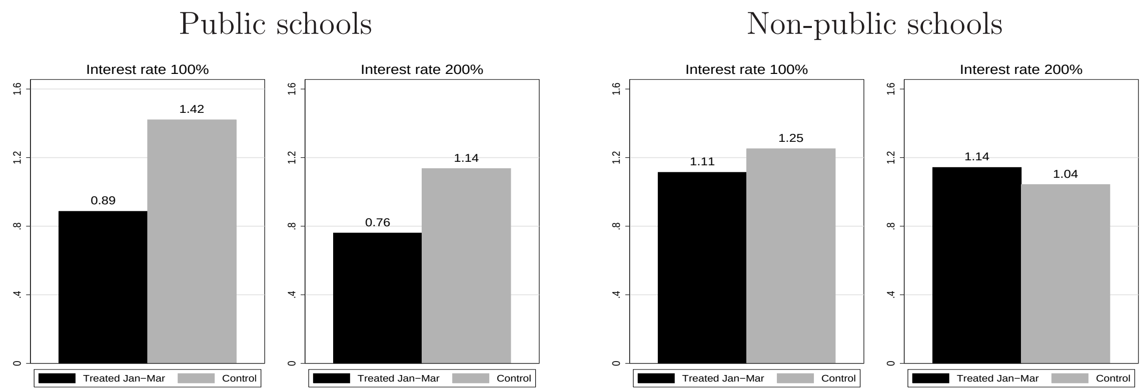
Notes: The horizontal axis shows the fraction of correct answers, while the vertical axis contains the fraction of students. Each point is the predicted proportion of students with correct answers that are equal or below the value in the horizontal axis. Predictions are obtained from OLS regressions of the fraction of students in public and non-public schools with correct answers equal or below each value in the horizontal axis on treated, the pre-test score and strata dummies (stratum 1 excluded for public and stratum 2 for non-public).

Figure 5: Euros allocated to sooner payment in the incentivized saving task by strata (June 2015): early treatment students

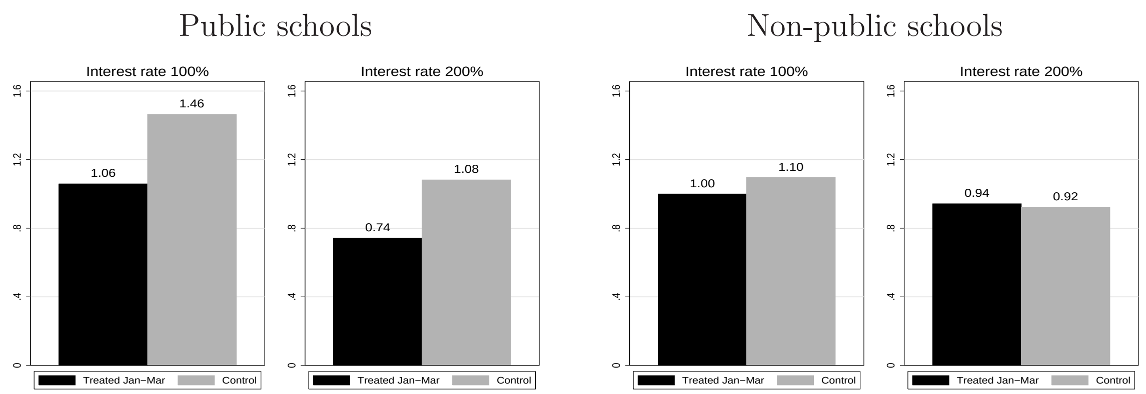
Panel A. Today *vs.* One week



Panel B. Today *vs.* Two weeks



Panel C. One week *vs.* Two weeks

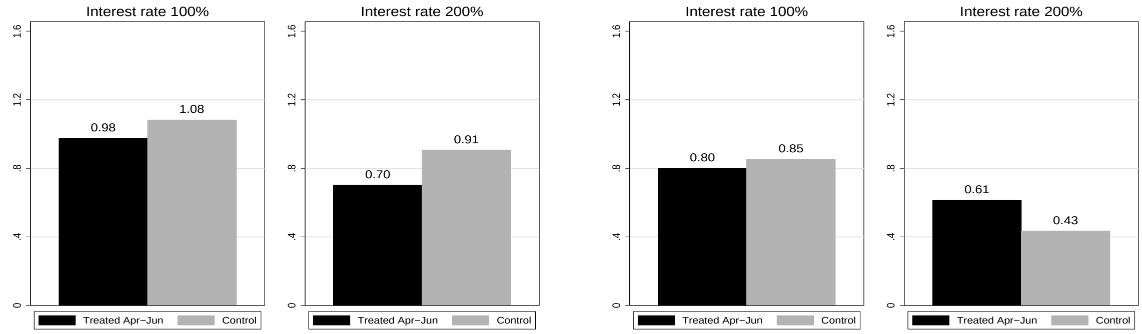


Notes: Treated students are 9th graders in Madrid receiving the course in January-March 2015. Controls are all 10^{th} *graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.1). Estimates are means, unadjusted by covariates or strata dummies. Table 9 shows adjusted estimates.*

Figure 6: Euros allocated to sooner payment in the incentivized saving task by strata (June 2015): late treatment students

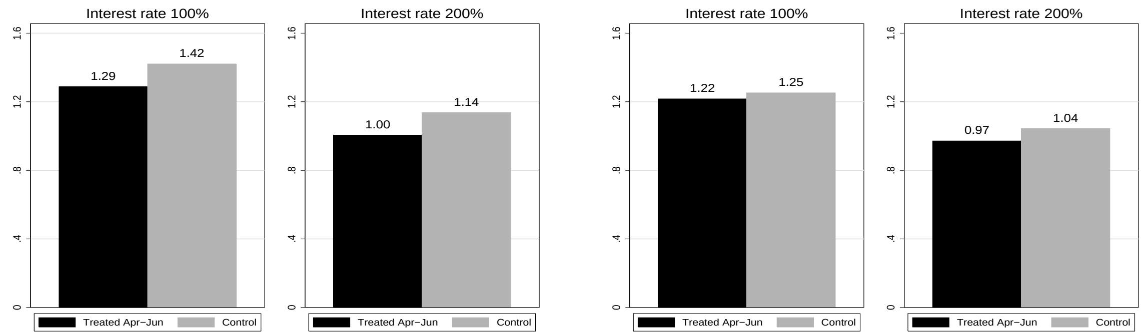
Panel A. Today *vs.* One week

Public schools | Non-public schools



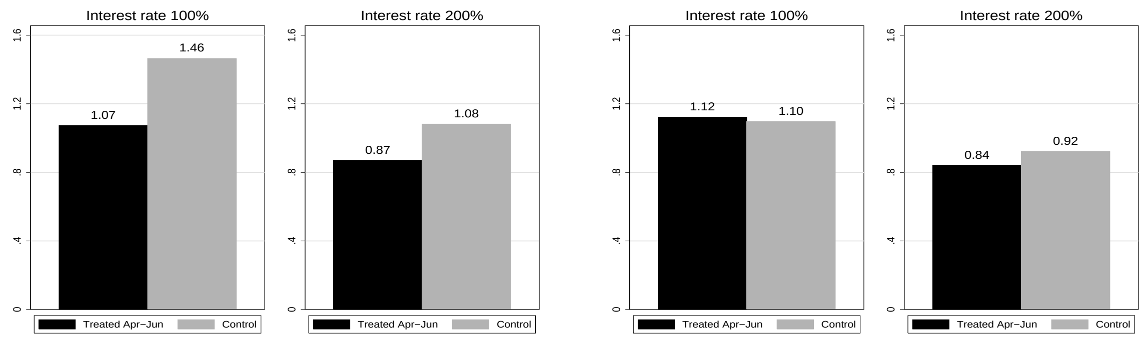
Panel B. Today *vs.* Two weeks

Public schools | Non-public schools



Panel C. One week *vs.* Two weeks

Public schools | Non-public schools



Notes: Treated students are 9th graders in Madrid receiving the course in April-June 2015. Controls are all 10th graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.1). Estimates are means, unadjusted by covariates or strata dummies.

A Appendix

Table A.1: Description of the strata

		Originally contacted		Accept participating	
		Total	Treated	Total	Treated
Applying before August 2014:					
Stratum 1:	Public schools, Madrid	15	8	9	5
Stratum 2:	Concerted schools, Madrid	10	6	4	2
Stratum 3:	Private schools, Madrid	5	2	2	0
Stratum 4:	Public schools, rest	18	9	6	2
				[12]	[8]
Stratum 5:	Concerted schools, rest	9	4	6	2
Stratum 6:	Private schools, rest	4	2	3	1
Applying September 2014:					
Stratum 7:	Public schools, Madrid	9	4	3	1
				[4]	[2]
Stratum 8:	Private schools, Madrid	2	1	2	1
Stratum 9:	Public schools, rest	30	15	13	4
				[15]	[6]
Stratum 10:	Concerted schools, rest	9	4	6	3
Applying October 2014:					
Stratum 11:	Public schools	6	3	4	3
Stratum 12:	Concerted schools	9	4	7	4
Applying November 2014:					
Stratum 11a:	Public schools	8	4	0	0
Stratum 13:	Concerted schools	6	3	2	1
Stratum 14:	Intended to give the material in 7^{th} grade	9	5	1	1
				[2]	
Stratum 15:	Intended to give the material in 8^{th} grade	5	2	2	0
Stratum 16:	Intended to give the material in 1^{st} year upper secondary school	23	11	7	4
Stratum 16a:	Intended to give the material in 2^{nd} year upper secondary school	7	4	0	0
Total number of schools		169	83	77	34
Percentage participants (%)				45.6	41.0

Notes: Each cell is the number of schools in the stratum that applied to teach the course (first column) and the subset assigned to treatment (second column). The third column is the number of schools that accepted the conditions while the fourth is the number of treated schools accepting the conditions. The numbers in brackets are the total number of schools accepting the conditions, including schools whose participation was not comparable to the rest and were subsequently excluded from the evaluation. In some models, we join strata 3 and 8 and 14 and 15 because no school assigned to treatment accepted the conditions. The information about the grade where the school intended to give course was only available for applications submitted after October 2014.

Table A.2: Balancing tests at baseline by strata

	Public schools			Non-public schools		
Panel A: Sample composition by strata						
Demographic characteristics:						
Foreign born	0.14			0.08		
Older than normal progression	0.28			0.17		
Expectations:						
Expects to finish at most HS academic track	0.17			0.10		
Expects to finish at most HS vocational training	0.28			0.18		
Expects to finish college	0.72			0.82		
Labor status of father:						
Self-employed	0.24			0.32		
Employee	0.59			0.57		
Unemployed	0.17			0.11		
Panel B: Balancing tests at baseline by strata						
	Treated (19 schools)	Control (23 schools)	p-value of the difference	Treated (14 schools)	Control (18 schools)	p-value of the difference
Fraction of correct answers in pre-test	0.572	0.585	0.714	0.624	0.619	0.426
Variables used in the stratification:						
Madrid	0.324	0.303	0.438	0.270	0.358	0.357
Demographic characteristics:						
Female	0.475	0.494	0.484	0.464	0.527	0.036
Foreign born	0.164	0.142	0.526	0.105	0.066	0.462
Older than normal progression	0.359	0.256	0.089	0.219	0.171	0.791
Expected age to finish school	20.860	21.256	0.060	21.452	21.683	0.666
Expects to finish at 18 or earlier	0.208	0.171	0.205	0.130	0.098	0.638
Hypothetical preferences:						
Prefers 100 euro today to 120 in 3 weeks	0.259	0.265	0.628	0.288	0.280	0.754
Prefers 100 euro today to 150 in 3 weeks	0.160	0.132	0.311	0.137	0.120	0.493
Prefers 100 euro today to 180 in 3 weeks	0.080	0.080	0.791	0.059	0.058	0.764
Sources of income:						
Family business/allowance home duties	0.310	0.313	0.811	0.338	0.292	0.201
Unconditional allowances	0.777	0.736	0.094	0.806	0.831	0.620
Occasional jobs	0.210	0.171	0.260	0.199	0.210	0.882
Talk to parents about economics:						
More than once a week	0.216	0.235	0.168	0.234	0.196	0.806
Once a week	0.211	0.224	0.413	0.234	0.219	0.287
Less than once a week	0.290	0.305	0.582	0.330	0.330	0.781
Never	0.283	0.236	0.045	0.202	0.255	0.086
Labor status of father:						
Self-employed	0.207	0.264	0.082	0.361	0.294	0.028
Employee	0.606	0.563	0.194	0.523	0.596	0.216
Unemployed	0.128	0.112	0.821	0.064	0.067	0.106
Other	0.059	0.061	0.995	0.052	0.042	0.997
Labor status of mother:						
Self-employed	0.118	0.154	0.163	0.233	0.169	0.084
Employee	0.525	0.501	0.536	0.482	0.572	0.125
Unemployed	0.100	0.100	0.725	0.077	0.076	0.695
Other	0.257	0.244	0.739	0.208	0.183	0.960

Source: information about demographics comes from the December survey to students. Information about grade repetition (date of birth) comes from school records.

Notes: † The samples exclude one stratum that mixes 1 public and 2 non-public centers. That stratum originally grouped high schools who intended to teach the course to 7th or 8th graders. The sample of 42 public schools contains 1,855 9th graders, while the sample of 32 non-public schools comprises 1,087 9th graders. Students with special educational needs or who did not take the December test are excluded.

Table A.3: The effect of the financial literacy program by student progression

	Normal progression	Older than normal progression	Expects to drop-out after 18	Expects to drop-out at 18 or before
Panel A: Financial knowledge (March 2015)				
1. Normalized tests scores	0.102*	0.283**	0.162**	0.201**
	(0.063)	(0.057)	(0.057)	(0.094)
Panel B: Hypothetical saving choices (March 2015)				
2. Earlier choice (pooled)	-0.034***	0.016	-0.033***	0.003
	(0.013)	(0.027)	(0.010)	(0.028)

*Notes: each cell reports the estimate of the variable Treated in a regression where the dependent variable is shown in the row and covariates include the lagged dependent variable and strata dummies. All specifications estimated by OLS. Standard errors (in parentheses) are clustered at the school level. * significant at 10%, ** significant at 5%, *** significant at 1%.*

Table A.4: The effect of the financial literacy program by strata

	Public	Non-public	p-value of the difference
Panel A: Attitudes toward finances (March 2015)			
1. Talks to parents about economics (overall†)	0.085	0.171***	0.428
	(0.075)	(0.065)	
Panel B: Sources of income (March 2015)			
2. Occasional jobs in the market/selling things (online, street markets)	-0.022*	0.037	0.052
	(0.012)	(0.027)	
3. Money for tasks at home/work in family business	0.034	0.089***	0.148
	(0.030)	(0.026)	
4. Any source of income	0.012	0.078***	0.074
	(0.030)	(0.022)	

*Notes: each cell reports the estimate of the variable Treated in a regression where the dependent variable is shown in the row and covariates include the lagged dependent variable and strata dummies. All specifications estimated by OLS, but the one in row 1†, that is the latent index coefficient of an ordered Probit, with outcomes from never to more than once a week. Standard errors (in parentheses) are clustered at the school level. * significant at 10%, ** significant at 5%, *** significant at 1%.*