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 23, 2023

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 18% 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 and patience 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.

*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, Sofia Anyfantaki, Manuel Arellano, 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 experimental evidence regarding the effects of those programs suggest positive average impacts on the financial knowledge of students (see, for example, Kaiser and Menkhoff, 2020 or Kaiser et al., 2022). Those meta-analyses point at lower and more heterogeneous impacts of financial education on outcomes like reductions on borrowing, adoption of budgeting or increases in saving.[2] One possible reason for the heterogeneity in those downstream behaviors lies on whether different financial literacy programs alter the intertemporal preferences of high school students and shape their future choices. A second reason is that different students may react differently to financial literacy programs.

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 to be delivered during 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] Regarding earlier non-experimental work, Bernheim et al. (2001) found 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. A second source of heterogeneity in observational studies is the lack of information about the implementation (see Urban et al. 2020).

This paper assesses a randomized controlled trial aimed at gauging the impact of that financial literacy course. 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 within strata defined by place of residence and type of school (see Table 1 for the timing of the intervention). 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 measured by standardized tests as well as on labor supply, saving choices and measures of patience as elicited via short surveys. Furthermore, three months after the course was delivered, we conducted an incentivized saving task aimed at eliciting patience - namely, a Convex Time Budget Task - see Andreoni and Sprenger (2012). 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. Due to budgetary considerations, only the subsample of 10th graders in Madrid participated in that experiment, so that analysis is conducted using only strata in Madrid.

Our results can be summarized as follows. First, as regards *financial knowledge*, we find that students receiving the material between January and March 2015 increased their scores in a financial literacy exam delivered in March 2015 by 18% of one standard deviation. Conversely, 10th graders in those same schools - who had not taken the course - scored similarly than 10th graders in control schools, suggesting absence of spillovers across grades and within schools. Secondly, we document a significant increase in forms of *informal labor supply* among treated students, like working for money in family business or getting money in Exchange of household tasks. Third, in the *Convex Time Budget Task* performed three months after the program was delivered, treated students allocated an amount to sooner payments that was lower than that of controls by 18% of one standard

deviation (standard error: 10% of one standard deviation). When we compare all the treated to all controls, the estimate is lower (12% of one standard deviation increase in patience with a standard error of 8%).[3]

The fall in the preference for sooner payments among the treated while, at the same time, having youths engaging more in money earning activities at home, raises the issue of whether more patient choices are due to changes in preferences for the future or, alternatively, through increases in resources that alleviated liquidity constraints. To disentangle the channels at work, we take advantage of the stratification of the intervention and turn to a heterogeneity analysis between public and non-public schools, the latter being attended by students with a better parental background. By comparing strata, we observe a marked improvement in the lower part of the distribution of financial knowledge scores in public schools only, while in non-public schools the improvement happened mainly at the top of the distribution of financial knowledge. Furthermore, patience elicited in the incentivized saving task increased in public schools only, while student's income sources only changed in non-public ones. Those results suggest that, at least in public schools, the program operated through a fall in the discount rate, not through expanded resources that alleviated liquidity constraints.

We contribute to the literature on financial literacy by examining jointly the impact of financial education on the distribution of financial knowledge, labor supply and measures of patience in a new setting. As mentioned before, the impact of financial literacy interventions on downstream financial behaviors varies across studies. In the particular context of *high school interventions*, Bruhn et al. (2016) and Frisancho (2023a) evaluate two large-scale interventions in Brazil and Peru, respectively, that increase the financial knowledge of students. Although the latter study documents that the intervention diminished the propensity to borrow and the probability of having credit records, the former documents that, at least in the short run, treated students increased their use of expensive credit to make consumer purchases.[4] Thus, to better understand the channels through

[3] We discuss the treated between January and March 2015 as those are the ones we can estimate impacts on knowledge, labor supply and patience.

[4] Bruhn et al (2022) examine the impacts nine years after the intervention, and document a lower use of expensive credit among treated students, which could be attributed to the negative experiences with

which financial literacy operates it is important to know whether or not financial education affects the time preferences of students. We show that, on average, financial literacy in high school increases financial knowledge and measures of patience of the students, at least among those treated between January and March, like Sutter et al. (2023) or Alan and Ertac (2018) but unlike Lührmann et al. (2018). We also detect increases in labor supply, an outcome detected in programs aimed to the youths in developing countries and that may cause increases in savings through channels other than patience (see Berry et al. 2018 or Horn et al., 2023).

Second, the stratification of the randomization allows us to estimate the response of financial knowledge, labor supply and patience by type of school. We detect that students in public schools (who come from a relatively more disadvantaged background) experience increases in financial knowledge and in patience, but not in labor supply, a finding consistent with the notion that increases in financial knowledge affect intertemporal decisions (like Sutter et al. 2023 for high school or Alan and Ertac, 2018 for elementary school students). On the other hand, as we cannot reject the hypothesis that financial education does not affect intertemporal choices in non-public schools, our results reproduce the heterogeneity in responses of patience to financial education across studies (for example between Sutter et al. 2023 and Lührmann et al. 2018, both using interventions in Germany). As supplementary material, we discuss possible correlates of heterogeneity in responses across types of schools.

Finally, and while not the main contribution of our study, we use the staggered nature of the implementation to discuss externalities of financial education across grades within the school. Haliassos et al. (2020) exploit quasi-random variation of migrant settlement patterns in Sweden to estimate a social multiplier of financial knowledge among adults. In the context of a very intense financial education intervention in Brazil, Bruhn et al. (2016) and Frisancho (2023b) document increases in financial knowledge among the families of treated students, through the latter's engagement in familial financial affairs. Similarly, Frisancho (2023a) finds that teachers giving financial literacy courses improved

such products earlier in the life cycle.

their own financial decisions. While we confirm that treated students started talking about economic matters with their family, once we exploit the staggered nature of the intervention and the presence of a set of non-treated students within treated and control schools, we fail to detect spillovers in financial knowledge, labor supply or preferences for time across grades.

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

2 Intervention and study design

Since 2012, every year about 400 high schools in Spain have voluntarily delivered a 10-hour financial education under the BdE-CNMV program. Participant students are typically 9th graders (i.e., are between 14 and 15 years of age). Compulsory education finishes at age 16 in Spain in 10th grade, and 9th grade is the last year with very few electives.[5]

The course covers several areas. A first module on *Savings and Financial planning* includes notions on how to elaborate a budget to be able to save and meet future needs. Saving is presented as a means to achieve future consumption possibilities. Also, students learn about the allocation of regular and irregular expenses in a monthly budget. A second set introduces *Means of payment* covering the different types of bank accounts, 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. In the third set, students are introduced to the notion of interest rate and interest rate com-

[5] 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.

pounding (*Banking Relationships*). In addition, the module introduces the notion of risk associated to different investment choices. A fourth set of modules deals with *Sustainable Consumption*, aimed at characterizing environmentally responsible consumption. Finally, there are two more modules on specific investment vehicles, such as pension funds and insurance vehicles.

2.1 Expected outcomes and their measurement

The stated objective of the overall intervention was "to contribute to improving citizen's financial culture, providing them with tools, skills and knowledge to adopt informed financial choices". We discuss next our measures of financial knowledge as well as the channels through which students could adopt informed financial choices.

Financial knowledge To measure financial knowledge, educational experts designed a set of around 200 items for a previous evaluation in 2012. The items were multiple choice (single-answer) questions designed to determine if students had acquired competences in *Savings and Financial planning*, *Means of Payment*, *Banking relationships* and *Sustainable Consumption*. Based on these questions and on the tests designed for the previous evaluation, we elaborated three different tests of 30 items each. There were two alternative set of questions posed in each assessment so no student faced the same question twice.

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.

In *Means of Payments*, students were asked about basic security rules of banking accounts and in the use of money. They were also asked about under which situations the use of a bank account is preferable to cash.

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.

Time preferences and saving choices Financial literacy programs emphasize the students' awareness about the future consequences of their actions, a set of contents that can modify intertemporal trade-offs. The view that financial education can shape preferences relates to a literature that considers time preferences not as deep parameters governing choices, but as shaped by rational consumers' decisions to invest in goods that expand their horizon of decision (see Becker and Mulligan, 1997). Alan and Ertac (2018) conduct an intervention in Turkey directed at young children that included vivid images of how their future selves are shaped by their current actions. They find that such intervention increased the degree of patience, elicited by intertemporal consumption choices in the context of a Convex Time Budget Task (see Andreoni and Sprenger, 2012). However, among teenagers, studies present different results. Lührmann et al. (2018) find that after a short financial literacy course, disadvantaged German youths increased the *quality* of their decision making, but not the overall degree of patience. On the other hand, Sutter et al. (2023) find instead increases in patience and in *elicited risk aversion*.

A first measurement of saving choices was obtained via hypothetical questions in a short survey after each test. Firstly, we asked each student four hypothetical choices between receiving 100€ today and another amount (ranging from 120 to 180€) in three weeks or in six weeks. However, previous research has documented that a higher preference for early payments varies over the business cycle and may capture the market cost of bringing resources to the present (see Krupka and Stephens, 2013).

In a separate assessment aimed at recovering preferences for time, we implemented a Convex Time Budget Task. Namely, students were presented with nine sequential choices asking them to allocate fractions of 6€ between payments at various dates and

with varying interest rates.[6] 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. 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 A.1), (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.[7]

The choice of that sort of payoff was driven by the consideration that USBs are homogeneous goods whose attractiveness varies along one dimension (storage capacity), and because institutionally it was not possible to use money as payment. Providing a durable good in an intertemporal task may have implications on the elicited preferences. On one hand, giving a good with unique characteristics (rather than money) may diminish the pooling of the experimental payoffs with other resources, diminishing the degree of linearity in the utility function -see Cohen et al. 2020 or Lührmann et al. 2018. On the other hand, in the case of durable goods there is a difference between consumption (use) and receipt.

Preferences for leisure Financial knowledge may shape preference parameters other than patience, such as preferences for leisure. In this respect, Berry et al. (2018) or Horn et al. (2023) find that financial education interventions increased child labor in an intervention in Ghana and Uganda, respectively. The possibility of responses along the leisure margin raises the issue that if youths receive additional income as a result of the exposure to financial literacy courses, their urge for immediate consumption can be alleviated and may increase their saving (see Lührmann et al. 2018 or Krupka and

[6] 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.

[7] 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. The USBs had the logo of the *Finance for All* program and their storage capacity ranged between 2 GB and 32 GB.

Stephens, 2013). In those settings, financial literacy programs may not increase patience, or even the quality of financial decision-making, but rather expand the student's budget constraint, an issue we discuss below. To elicit such behavior, the survey to students follows the 2012 PISA Financial Assessment questionnaire, and contains information about students' sources of income (allowances - distinguishing between conditional on conducting tasks at home or not, work in the labor market, occasional sales, etc.).

Spillovers A literature examines possible spillovers of financial knowledge. Students may 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., 2020, Frisancho 2023b). A second possible channel for spillovers happens at the school: students may also communicate with other students about the material received and spread changes in attitudes. We can test for spillovers both through surveys (we elicit if students talk to their parents about economics) and in attitudes and knowledge through the staggered design of the intervention. Namely, students from 9th grade in treated schools were delivered the material between January and March, while those in 10th grade were not, so comparing the financial knowledge and attitudes of 10th graders in treated and non-treated schools by March 2015 permits detecting spillovers.

Heterogeneity Financial literacy can be viewed as a form of accumulating human capital (Lusardi et al. 2017, or Jappelli and Padula, 2013). Under that view, individuals sacrifice resources in the present to acquire that knowledge and the pay-off of that investment is a higher return on their saving. Specially patient individuals are more likely to acquire financial literacy because they discount streams of future benefits at a lower rate than the rest - see Meier and Sprenger (2013). If the alternative to school-based financial education is receiving that education at home, and disadvantaged students come from less financially literate families, the impact of financial literacy programs on knowledge should be higher the more disadvantaged the student's background. Another interpretation is that financial literacy courses were most effective at increasing knowledge among

the students with a highest incentive to acquire those skills -see Frisancho, 2023a, or Cole et al. 2016. To disentangle between those possibilities, our research design stratifies by proxies of parental background (type of school within region), and compares outcomes across strata.

2.2 Evaluation features

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. As neither the teaching body nor students in the school had had any previous experience on the contents of the specific program, the results are 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.[8] There are 16 strata in total (see Table A.2 for details).[9]

The randomization was conducted before schools were presented the conditions to participate.[10] Namely, the material was to be delivered in regular school hours to 9th graders (and only to 9th graders). Second, all 9th graders receiving the course would

[8] 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.

[9] 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.

[10] We sent letters 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. 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.

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.[11] Out of 169 schools contacted, 77 schools agreed to participate under those conditions (see Table A.2).

2.3 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 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 Convex Time Budget Task. Due to budgetary considerations, only 10th graders in the schools in strata 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 short-run impacts of the financial literacy course on financial knowledge and attitudes of young adults.

Formally, we consider linear regression models of the form:

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

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)

[11] 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.

and it is included to improve precision. Finally, X_k are dummies indicating the strata the school belongs to (see Table A.2). $\varepsilon_{i,s}$ 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 and attitudes).

We estimate longer-run impacts using 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.[12] 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 Convex Time Budget 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, given the scope of the evaluation, we test multiple hypotheses in this paper. To account for that we present p-values adjusted following Romano and Wolf (2016).[13]

Heterogeneity of impacts 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 for non-public schools, the design guarantees that students in treated (non-) public schools have similar characteristics to those in control (non-) public schools. Finally, we also experiment obtaining up to 14 strata-specific experimental estimates of θ_1^s, as randomization

[12] 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.

[13] We start with a family of hypotheses to test. The null is that the coefficient with the highest t-statistic in the family is 0. We then resample the data and obtain estimates of each coefficient in the family of hypotheses and compute a Studentized "null statistic". The empirical quantile of the distribution of the maximum Studentized t-statistic across coefficients and resamples provides the critical value of the null hypothesis. If the original t-statistic is below that critical value, we stop the algorithm and accept the null that all coefficients in the family are zero. Otherwise, we exclude from the family of hypotheses the one just rejected and re-start with the remaining ones.

of treatment was conducted within each of the strata.[14] Those specifications are useful to the extent that they allow us to relate differences in θ_1^s to strata-level characteristics (public vs non-public school), but also to details about how the program was implemented in the different schools (averaged within strata).

2.4 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 to teachers 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 about a third of the schools, the material was delivered by more than two teachers (Table A.3).[15] The median number of hours devoted to the course was 10.[16]

The average number of lessons covered was seven out of the ten lessons available.[17] 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 Mathematics.[18]

[14] In two strata, public and non-public schools were mixed. As we use the strata-specific estimates mostly to understanding the differences in outcomes between both sets of schools, we use the remaining 14 strata.

[15] 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.

[16] 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.

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

[18] We detected two main forms of noncompliance through surveys and personal contact with the teachers. 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. 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 main analysis so that estimates can be interpreted as intent-to-treat estimates where both non-compliant schools are still considered as treated. For robustness, we also present results without those two schools.

2.5 The sample and balancing at baseline

The geographical coverage of the final sample is quite broad but not necessarily representative of the universe of Spanish high schools, as seventy percent of centers are located in three regions: Madrid, Aragon and Valencia.[19] The final sample of 9th graders we use contains 3,050 students in the baseline measurement. However, most of the analysis uses a balanced sample of 2,696 9th graders.[20] Table 2 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.

One third of both treated and control schools are located in Madrid. The share of students in public schools is somewhat higher in treated (64.3%) relative to control schools (59.7%). The fraction of females is 50.6% in control schools, slightly higher than the 47.5% observed in treated schools. 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). However, none of these differences are statistically significant at usual confidence levels.

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 answered correctly almost 60% of the questions.

[19] 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.

[20] 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 and they should not be classified under *special educational needs* (medical conditions, autism, etc). Table W1 in the Online Appendix lists the selection criteria.

3 Results

3.1 Impacts on financial knowledge and behavior

Financial knowledge Panel A in Table 3 presents the impact of the financial literacy course on short-run financial knowledge.[21] 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 other interventions summarized in the meta-analysis of Kaiser et al. (2022) that includes a previous version of our paper. There, the mean effect size on financial knowledge of 18% of one standard deviation in the age group between 14-25. Turning to particular studies, our results are in line with those of Bruhn et al. (2016), Frisancho (2023a), Hospido et al. (2015) or Walstad et al. (2010), who report a positive impact of financial literacy courses in high schools.[22]

Spillovers in knowledge We analyze if 10th graders in treated schools could have been affected by the material received by 9th graders -for example, because teachers use the material in other grades or because students receiving the material share some of the knowledge with students in other grades - see Haliassos et al. 2020. Were that the case, we would expect that (non-treated) 10th graders in treated schools obtained higher grades than those in non-treated ones. The estimate in Panel B in Table 3 is -.057% of one standard deviation, rejecting sizable spillovers across grades.

Panel C in Table 3 examines if the difference in financial knowledge between treated

[21] Table W2 in the Online Appendix reports estimates in a subsample that excludes the two non-complaint schools. Results barely change.

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

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.[23]

Hypothetical saving choices and labor supply Regarding hypothetical saving choices, we document a decrease in the preference for current income among students who went through financial education (Table 4, panel A). 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. The fraction of earlier choices diminishes when treated students are confronted with low interest rates and, when we pool all 4 hypothetical choices (column 5), 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 could reflect a fall in the true rate of *time preference*. However, more patient choices could also reflect an alleviation of *liquidity constraints* associated to higher income (Krupka and Stephens, 2013; Carvalho et al. 2016; Cohen et al, 2020). To further explore this possibility, columns 1 to 4 in Panel B of Table 4 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 increased 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.

[23] 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.

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 in Berry et al. (2018) or in Horn et al. (2023), who also document similar results among Ghanaian and Ugandan children following a financial literacy course, but not with those in Lührmann et al. (2018), who focus on disadvantaged German youths.[24]

Panel C of Table 4 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. 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.

In summary, the lower preference for current income among treated students documented in Table 4 could be either due 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 B of Table 4 suggests that at least part of the decrease in the preference for current income could be associated with an increase in income.

Spillovers in attitudes As it was the case with knowledge, we analyze if 10th graders in treated schools could have been affected by the material received by 9th graders. Table A.4 examines spillovers among 10th graders in attitudes (hypothetical saving choices, sources of income, or talking to parents about economics). None of these changed as a result of the program.

[24] Tables W3 and W4 in the Online Appendix use Differences-in-Differences (DID) models to re-estimate selected Models in Tables 3, and 4. We do this for outcomes for which we have a comparable measure before (December 2014) and after the treatment (March 2015). Unlike Model (1), DID models estimate the impact of the program by netting out from the 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 do not include controls for the lagged outcome $Y^0_{i,s}$. 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.

3.2 Impacts on time preferences

A second measure of time preferences was elicited through a Convex Time Budget Task performed in June 2015, three months after students treated between January and March received the course with 10th graders as the control group. 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. For example, Panel A in Figure 1 shows that 23% of 9th graders treated between January and March 2015 preferred 100€ on the survey date to 120 euros three weeks later, while the corresponding number among 10th graders was 27% (the difference is not statistically significant). On the other hand, when the hypothetical payoff for waiting three weeks was increased to 150€, the fraction of treated 9th graders who chose the sooner hypothetical payment is 12%, *higher* than that observed among controls (9%).

Figure 2, panel A, plots the amount that treated between January and March 2015 in the strata in Madrid allocated to the earlier date in the Convex Time Budget Task. The controls are the full set of 10 graders. We see that 9th graders treated between January and March 2015 allocated a lower amount to earlier payments than the group of controls did at any interest rate or maturity. For example, 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 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 2), but the magnitude of the responses is *lower* than those of early treatment students (see Panel A).

Panel D in Table 4 summarizes the results of the Convex Time Budget Task in a regression format. The dependent variable in column (1) is the amount 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. 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.[25]

Across all choices, students receiving the material in January-March 2015 (column 1) chose in June 2015 allocations that involved 27 cents lower early consumption than controls (standard error: 15 cents). The amount allocated to the sooner payment amounts to 18% of one standard deviation of the amount allocated to the sooner date (1.49€).

Column 2 in Panel D of Table 4 compares the amount allocated to the sooner payment by the full group of 9th graders to those chosen by the full group of 10th graders as controls. In this case, treated students also reduced the amount allocated to the earlier date, but the magnitude of the reduction is 18 cents (standard error: 11 cents).[26]

We detect little evidence that financial education diminishes optimization errors or inconsistent choices, defined as choices in which students allocate more resources to the sooner payment when the interest rate increases - see Table A.5. In any case, columns 3-4 in panel D of Table 4 re-examine the impact of financial education on the amount allocated to the sooner payment in a sample *without* inconsistent choices. The results are qualitatively similar to those shown in columns 1-2, but more precise.

Channels Table A.6 shows possible channels that underlie the lower amount allocated to the earlier date among treated youths.[27] There, we recover the curvature of the utility function, the degree of present bias and the weekly discount factor for treated and control students - see Andreoni and Sprenger, 2012.[28] The implied weekly discount factor among

[25] The base category reflects the amount chosen in the earlier date by students in public schools requesting the material before September 2014 (stratum 1 in Table A.2) who prefer 120€ in two weeks to 100€ today. 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.

[26] Table W5 in the Online Appendix reports the balancing tests between 9th grade (treated) and 10th grade (control) students for the Madrid subsample. Treated students have similar characteristics to those in the control group.

[27] In what follows, as Cohen et al. (2020), we use the term discount *factor* to denote the ratio between future utility and current utility, and discount *rate* as 1 minus that term. A higher discount *factor* denotes more patience, while a higher discount *rate* means the opposite.

[28] Namely, we compute for each choice in Table A.1 the log difference between the amount allocated to the earlier choice and the amount allocated to the later one. We replace the zeroes with .01. Then we regress the log difference on euros on the implied logarithm of the gross rate (100, 200 or 300), the time delay (1 or 2 weeks) and an indicator of the earlier choice being the day of the test. We use both OLS and Tobit models, as the latter account for the concentration of choices of 0€ allocated to the earlier choice (49% of the cases).

treated students is .92, whereas it is .85 among controls (see Table A.6, Panel B, row 2). The difference is both statistically and economically significant: over a quarter, treated youths would have a discount factor of .305, while controls would have .099. The increase in the discount factor is qualitatively in line with that detected in Sutter et al. (2023), but departs from Lührmann et al. (2018). We discuss these heterogeneous results below.

Summing up, students treated in January-March 2015 displayed more patient choices than controls at various interest rates and maturities. Results are, however, less precise for students treated in April-June 2015. Our results for the group receiving the treatment between January and March 2015 suggest that the impact of financial literacy programs on preferences persists three months after the program took place.

4 Heterogeneity by type of school

4.1 Heterogeneity across strata

The research design randomized treatment by type of school, an indicator of parental characteristics. As shown in Table A.7, 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] Table A.8 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 5 presents the effect of the financial literacy program on normalized

[29] Namely, public schools are those in strata 1, 4, 7, 9, 11 and 16 in Table A.2. 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.

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 3 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 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 5, 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.

Further, we decompose the impacts in financial knowledge in four separate areas of personal finance in each type of school: *saving and financial planning*, *means of payment*, *banking relationships* and *intelligent consumption*. Figure A.1 shows that financial knowledge gains in public schools are distributed equally in the last three areas, each with a gain of 17% of one standard deviation. Conversely, the impacts of the program in non-public schools are confined to the area of *banking relationships*, where the impact is 34% of one standard deviation. We discuss the issue below.

When we turn to attitudes, we observe that treated students in non-public schools reported a higher probability of receiving any source of labor income, but students in public schools did not (Table 5, panel B).[31]

Figure A.2 further illustrates the heterogeneity of responses by strata in the Convex

[30] Note that the sample does not coincide with that in Table 3, as strata 14 and 15 are not used in Table 5.

[31] Table A.9 shows that the effect is due to sources of income from the family. We also find a positive impact on talking to their parents (possibly linked to an exchange of services for money). 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-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.

Time Budget Task. It compares the amount 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. Panel C in Table 5 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 (with a standard error of 20 cents), while the corresponding difference among students in non-public schools is 8 cents (four times smaller in absolute value).

Figure A.3 shows the cumulative distribution function of the impact of the program on the fraction of euros allocated to the earlier choice in the incentivized saving task.[32] The leftward Panel of Figure A.3 shows that exposure to financial literacy material increased each fraction of euros allocated to the earlier date (notably at zero), while diminishing the fraction of students allocating 2€ or 4€ to the earlier date. Conversely, in non-public schools, there is a modest increase in the fraction of students devoting 0€ to the earlier choice, mostly at the expense of choices allocating 2€ to the present. For the rest of the distribution of euros allocated to the earlier date, fractions remain unchanged.

4.2 What can we learn from heterogeneity in outcomes?

Patience vs liquidity constraints Table 4 documents that treated students obtained additional income sources in exchange from family services. In turn, the expansion of resources may allow treated students to allocate less euros to the present in a Convex Time Budget Task. Hence, two alternative channels can account for the overall increase in saving in Convex Time Budget Task: an increase in the preference for future consumption and an alleviation of liquidity constraints.

An implication of the liquidity constraint channel is that we should observe decreases

[32] Namely, we run the following type-of-school specific regressions

$$1(Y_{i,s} <= \frac{c}{6}) = \theta_0 + \theta_c TREAT_s + \theta_{2,c} Y^0_{i,s} + \sum_{k=1}^{k=15} \pi_{k,c} X + \varepsilon_{i,s,c}$$

Where c takes values between 0€ (the student saves everything) and 6€ (the student allocates the full amount to the present). Figure A.3 plots the estimates of θ_c against each of the values of $\frac{c}{6}$. $Y^0_{i,s}$ is proxied by hypothetical choices in a Multiple Price List elicited in the December 2014 test.

in the preference for sooner payments precisely among the students whose income sources expanded. The results in Table 5 Panel C, and Figure A.3 show that all the increase in saving in the incentivized task was observed mainly among students in public schools, whose income sources did not increase. While other interpretations are possible, those patterns are at odds with the hypothesis that the increase in saving is associated to an alleviation of credit constraints.

Differential impacts across strata and correlates To investigate if the differences in the distribution of financial knowledge, attitudes and preferences between public and non-public students are due to different baseline characteristics we reweight the sample of public students to have characteristics similar on average to non-public school students and vice-versa. Namely, we use the covariates listed in Table A.7. plus additional parental background variables elicited from surveys to the families of the students to construct a propensity score and reweight each observation in the strata.[33] The results in the top Panel of Table 6 suggest a limited impact of student characteristics on the impact of the program. The main estimates imply that the program diminished the fraction of public-school students answering *less* than 25% of the questions correctly by 4.4%. If we reweight the sample of public school students to have characteristics similar to those of non-public school students on average -for example, giving less weights to grade repeaters- the resulting estimate remains basically unchanged at 4.2%.

A possibility is that public and non-public schools implemented the program differently, as non-public schools tended to deliver the course primarily as a part of the Math curriculum. To examine the correlation between program implementation and the distribution of gains of financial knowledge, we obtain 14 strata-specific estimates of the impact of the program on financial knowledge and regress them on type of school (public

[33] We construct a propensity score P of being a student of a non-public school using the expected age at baseline of leaving the educational system, dummies for grade repeater, born outside Spain, three dummies with the labor status of each parent (self-employed, employee and unemployed, with inactive as the omitted category) and two for educational attainment (high school or college). As parental education was only available for the sample of students who returned a paper survey (60%) we interact those variables with a dummy of answering the survey. We then reweight the sample of public students with the inverse propensity score as in Di Nardo et al (1996).

or private), the fraction of students receiving the course in the core courses of Mathematics or Social Sciences and the number of hours devoted to the program. The results in Table 6 suggest that a 10% increase in the fraction of students receiving the course as part of Mathematics *increases* by 8.6% the fraction of students answering correctly less than 25% of the questions and *diminishes* by 12% the fraction with less than 70% of correct answers -see Table 6, Panel B, columns 1 and 3, respectively.[34] In other words, as we consider strata with a higher fraction of treated students receiving the material as a part of the Mathematics curriculum (like it was the case in non-public schools) we observe a larger number of students in both tails of the distribution of financial knowledge gains.

5 Conclusions

This paper describes a randomized controlled trial in which 9th grade students from 77 high schools received a financial education course. Right after the treatment, treated 9th graders increased their test performance by 18% of one standard deviation, showed more patience in hypothetical saving choices, and a higher likelihood to conduct work at home in exchange of money. 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 and measures of patience in public schools, which over-represent disadvantaged students, but not in non-public ones.

A final note is that judging the success or not of a program by whether it changes the preferences of students may seem paternalistic or outside the realm of what financial education should do (Ambuehl et al., 2022). 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] We present scatterplots of the results in Figure W1 of the Online Appendix.

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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: 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€ today to 120 in 3 weeks	0.273	0.273	0.877
Prefers 100€ today to 150 in 3 weeks	0.152	0.127	0.224
Prefers 100€ 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 3: 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 4: The effect of the financial literacy program on attitudes, savings and preferences

Panel A: 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	
Panel B: 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
Panel C: 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	
Panel D: amount allocated to sooner payment in the Convex time budget task					
Dependent variable:		€allocated to sooner payment		€allocated to sooner payment, consistent choices only	
Treatment sample:	Jan.-March	All		Jan.-March	All
Treated	-0.269*	-0.178		-0.222**^	-0.115
	(0.148)	(0.114)		(0.090)	(0.073)
Sample size	3,510	5,976		3,059	5,264
Standard deviation dependent variable	1.49	1.51		1.17	1.21

*Notes: Panels A-C: sample of 2,690 students from 77 schools. 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. Panel D: 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. Control group includes 10th graders. OLS regressions using as the dependent variable the amount allocated to sooner payment. Stratification dummies and controls for interest rate, immediate payment, and delayed payment included. Standard errors (in parentheses) are clustered at the school-grade level. Correction for multiple testing implemented separately for columns 1 and 3 together; and columns 2 and 4 together.*

Table 5: 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: Sources of income (March 2015)			
5. Any source of income	0.012	0.078***^	0.074
	(0.030)	(0.022)	[0.43]
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.*

Table 6: Financial knowledge by school type

Panel A: Accounting for composition effects in the gains of financial knowledge between public and private schools		
Answers correctly...	(1) <= 25% questions	(2) <= 70% questions
1. Public school	-.044	-.007
	(.016)**	(.005)
2. Public school, reweighted like private school	-.042	-.007
	(.016)**	(.005)
3. Non-public school	-.000	-.038
	(.008)	(.012)**
4. Non-public school, reweighted like public school	.010	-.026
	(.015)	(.008)**

Panel B: Effects of school characteristics on the distribution of financial knowledge			
Answers correctly...	(1) <= 25% questions	(2) <= 60% questions	(3) <= 70% questions
Public school	.002	-.031	-.014
	(.026)	(.022)	(.025)
Fraction receiving FL course in math	.086	-.129	-.124
	(.041)**	(.053)**	(.028)**
Fraction receiving FL course in Social Sciences	.019	.040	.035
	(.058)	(.098)	(.032)
Number of hours	-.004	-.011	-.005
	(.004)	(.005)**	(.003)
Constant	-.006	.097	.041
	(.048)	(.055)	(.047)

Notes: Panel A - The dependent variable in Column 1(2) is an indicator of whether the student achieved answered correctly to less than 25% (70%) of the questions. Each cell is the coefficient of TREAT in an OLS regression of the dependent variable in the column. Other covariates are the score at baseline and strata dummies. In row 1 the sample of public school students is reweighted so average characteristics coincide with those of students in non-public schools. The reweighting is conducted by inverse propensity score reweighting, using as covariates the age at which the student planned to drop-out at baseline, whether or not s/he has repeated a grade, whether s/he is a migrant, three dummies with the labor status of mother and father (self-employed, employee or unemployed), an indicator of whether the parent answered a survey sent to the families and two dummies with the educational attainment of each of the parents.

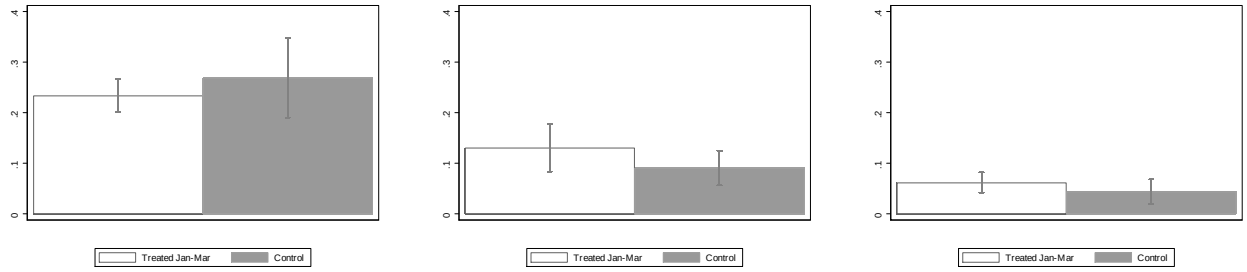
Panel B - The dependent variable are strata-specific estimates of the impact of treatment on the fraction of students answering correctly the fraction of questions shown in each column. The independent variables are averages of school level variables within the strata. For example, the fraction of students receiving the course in Math represents the mean of students in the strata receiving the FL course in a math course. Each observation is weighted by the number of students in the strata, as a measure of the precision of the corresponding estimate.

Figures

Figure 1: Fraction of treated and control students who choose the earlier payment in hypothetical choices between current and future income at baseline (December 2014)

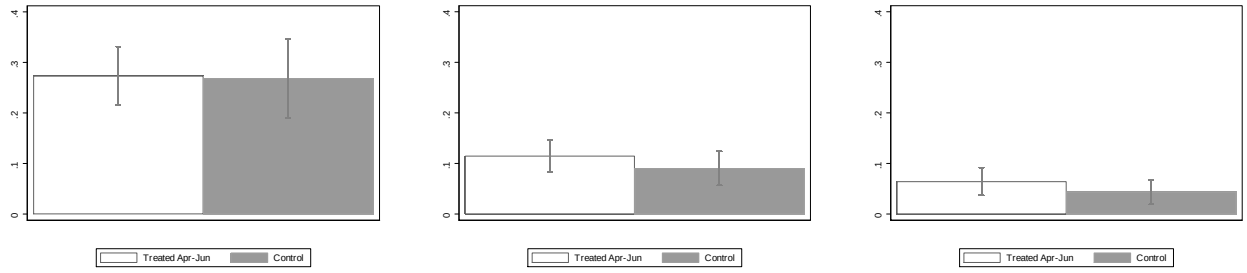
Panel A. Treated in January-March (9th graders) and controls (10th graders)

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

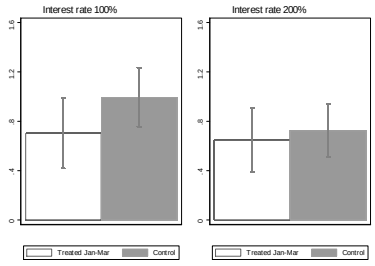


Notes: In panel A (B), treated students are 9th graders in Madrid receiving the course between January and March (April and June). Controls are all 10th graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.2). The black (gray) bars represents the fraction of 9th (10th) graders choosing 100€ today in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

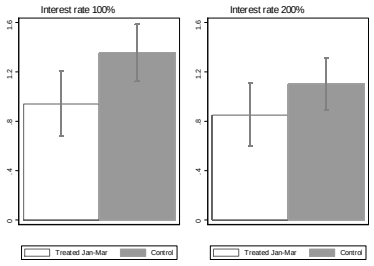
Figure 2: Euros allocated to sooner payment in the incentivized saving task (June 2015)

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

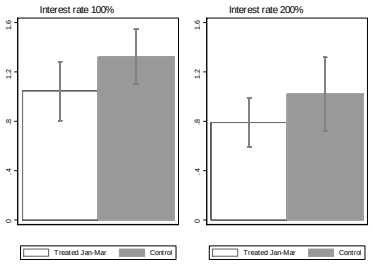
Today *vs.* One week



Today *vs.* Two weeks

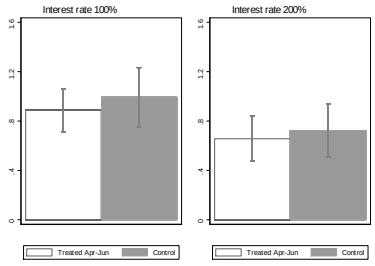


One week *vs.* Two weeks

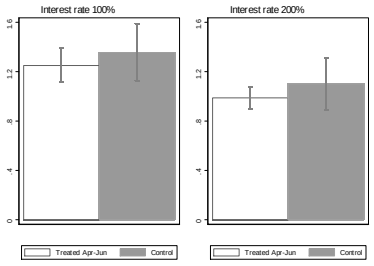


Panel B. Treated in April-June (9th graders, late treatment students) and controls (10th graders)

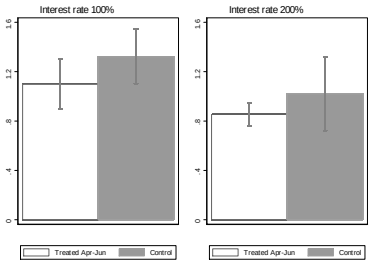
Today *vs.* One week



Today *vs.* Two weeks



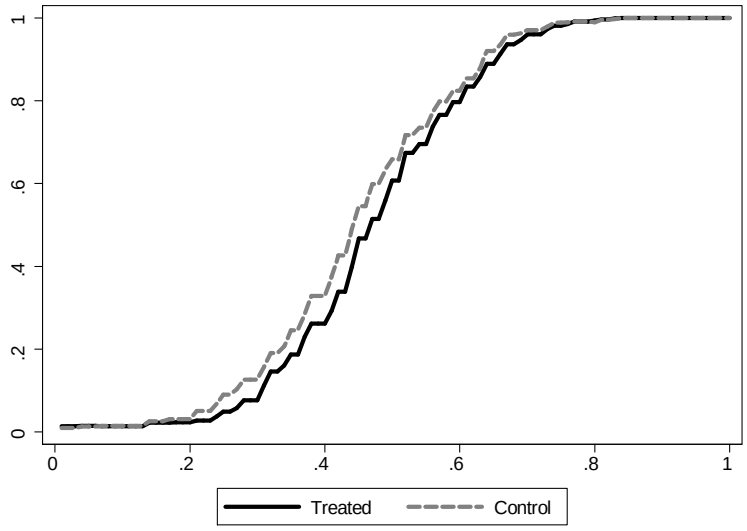
One week *vs.* Two weeks



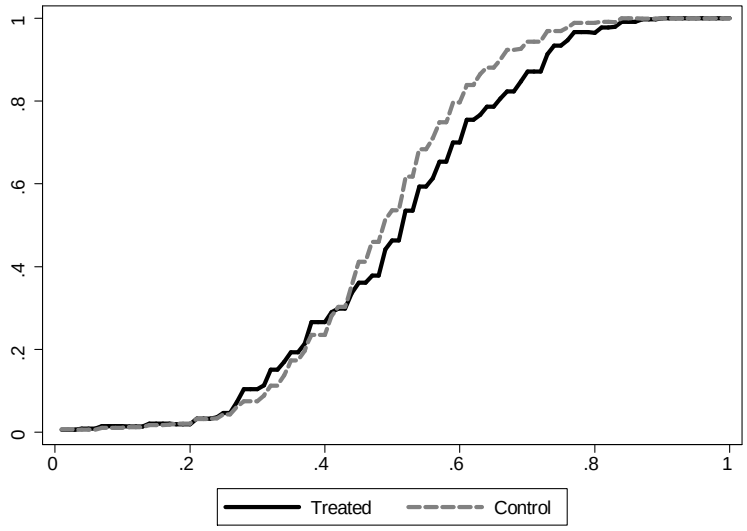
Notes: In panel A (B), treated students are 9th graders in Madrid receiving the course between January and March (April and June). Controls are all 10th graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.2). Estimates are means, unadjusted by covariates or strata dummies. Table 4 shows adjusted estimates both for students in panel A, and for all students together.

Figure 3: CDF of the raw scores by strata (March 2015)

Public schools



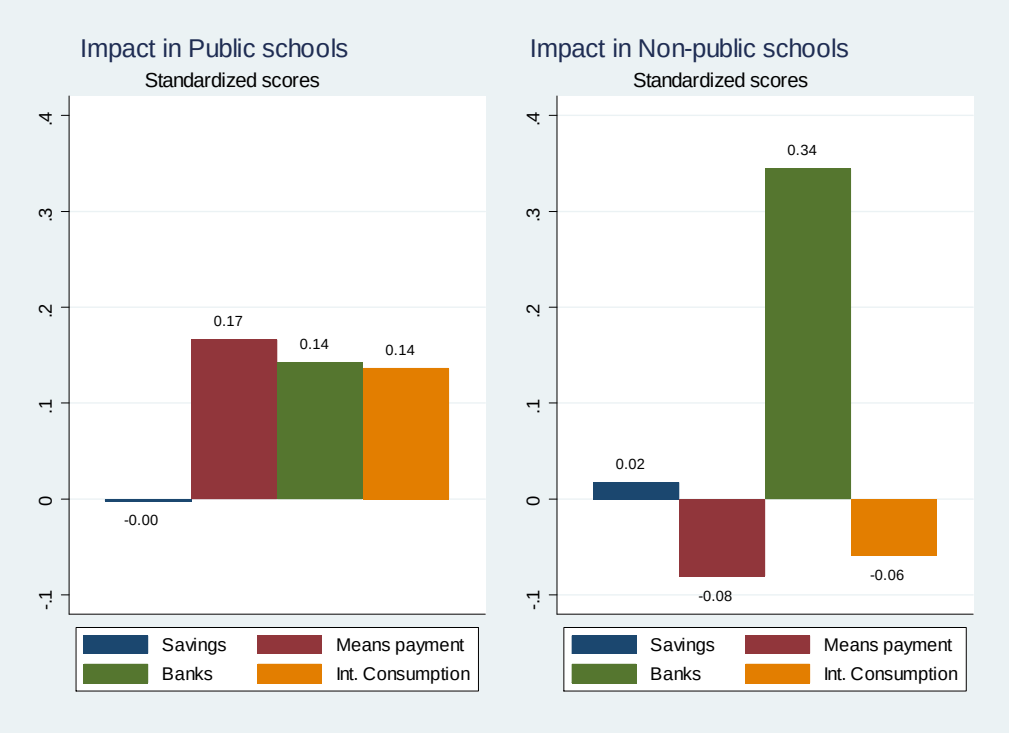
Non-public schools



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).

A Additional figures and tables

Figure A.1: Decomposing the gains in financial knowledge in specific areas or personal finance

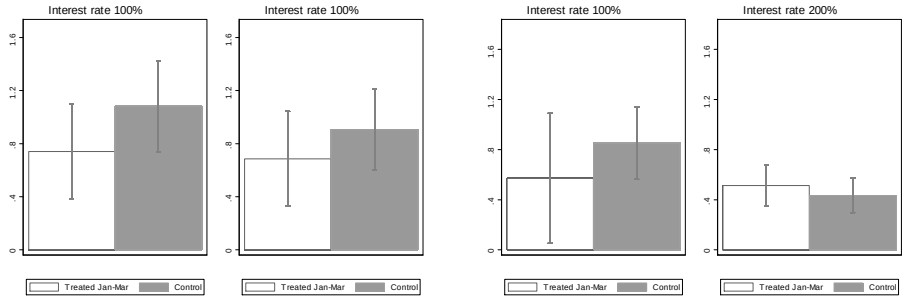


Notes: The left (right) panel shows the impact of the FL course in public (non-public) schools in the scores of four different areas: savings, means of payment, banks and intelligent consumption. Each estimate is obtained by type-of-school specific regressions of the score in each part of the test (normalized) on the treatment dummy, the score at baseline and the strata dummies using separate samples of public and non-public schools.

Figure A.2: Euros allocated to sooner payment in the incentivized saving task by strata (June 2015): early treatment students

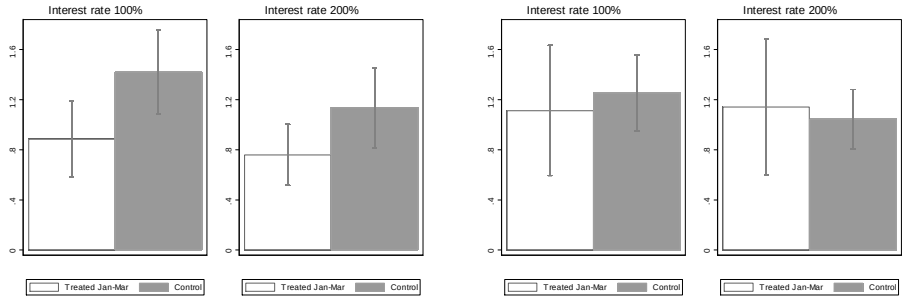
Panel A. Today *vs.* One week

Public schools | Non-public schools



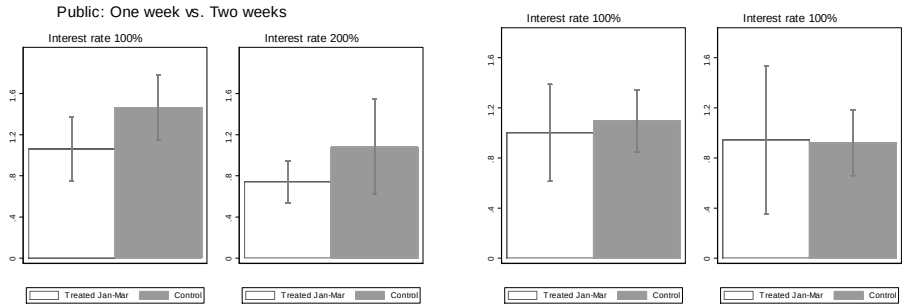
Panel B. Today *vs.* Two weeks

Public schools | Non-public schools



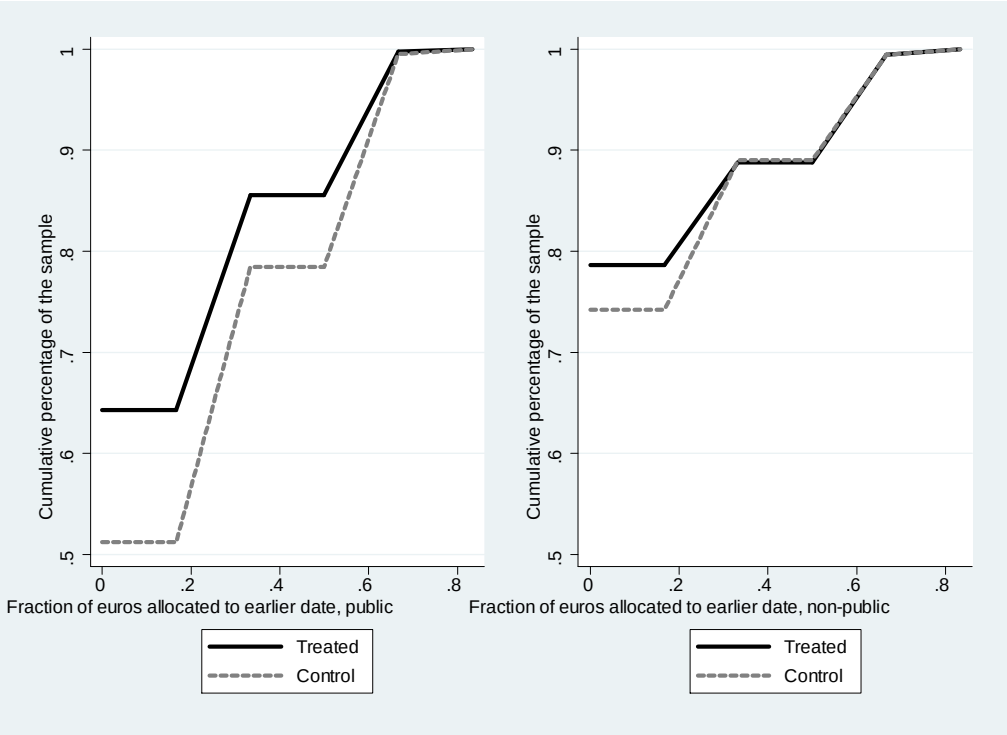
Panel C. One week *vs.* Two weeks

Public schools | Non-public schools



Notes: Treated students are 9th graders in Madrid receiving the course in January-March 2015. Controls are all 10^{th} graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.2). Estimates are means, unadjusted by covariates or strata dummies. Table 5 shows adjusted estimates.

Figure A.3: The distribution of impacts on earlier choices: public and non-public schools



Notes: In each panel, the horizontal axis shows the fraction of euros allocated to the earlier choice, while the vertical axis displays the fraction of students. Each point is the predicted cumulative distribution function of euros allocated to the earlier date obtained from a Logit regression of the fraction of euros allocated below a threshold (shown in the horizontal axis) on treated, strata dummies and choices at baseline. Each model is ran in separate samples of students in public and non-public schools.

Table A.1: Choices in the Convex Time Budget Task (CTBT)

SHEET 1: WHAT DO YOU PREFER?				
		Get _ _ € TODAY...	and _ _ € IN ONE WEEK	Please, pick 1 out of the 4 options in each part
Part 1	Choice a	6	0	a
	Choice b	4	2	b
	Choice c	2	4	c
	Choice d	0	6	d
Part 2	Choice a	6	0	a
	Choice b	4	4	b
	Choice c	2	8	c
	Choice d	0	12	d
Part 3	Choice a	6	0	a
	Choice b	4	6	b
	Choice c	2	12	c
	Choice d	0	18	d
SHEET 2: WHAT DO YOU PREFER?				
		Get _ _ € TODAY...	and _ _ € IN TWO WEEKS	Please, pick 1 out of the 4 options in each part
Part 1	Choice a	6	0	a
	Choice b	4	2	b
	Choice c	2	4	c
	Choice d	0	6	d
Part 2	Choice a	6	0	a
	Choice b	4	4	b
	Choice c	2	8	c
	Choice d	0	12	d
Part 3	Choice a	6	0	a
	Choice b	4	6	b
	Choice c	2	12	c
	Choice d	0	18	d
SHEET 3: WHAT DO YOU PREFER?				
		Get _ _ € IN ONE WEEK...	and _ _ € IN TWO WEEKS	Please, pick 1 out of the 4 options in each part
Part 1	Choice a	6	0	a
	Choice b	4	2	b
	Choice c	2	4	c
	Choice d	0	6	d
Part 2	Choice a	6	0	a
	Choice b	4	4	b
	Choice c	2	8	c
	Choice d	0	12	d
Part 3	Choice a	6	0	a
	Choice b	4	6	b
	Choice c	2	12	c
	Choice d	0	18	d

Table A.2: 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.3: Program Implementation

	Total	Public	Concerted	Private	Concerted or private
	N=1,228	N=762	N=425	N=41	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 A.4: The effect of the financial literacy program on financial knowledge, attitudes and savings of treated and non-treated students

	Non-treated students 10th graders March	Treated students 9th graders March
Panel A: Hypothetical saving choices		
Prefers early choice	-0.016	-0.026
	(0.013)	(.012)**
Panel B: Sources of income		
Any source of labor income	-0.032	0.038
	(0.027)	(.021)*
Panel C: Talks to parents about economics		
Talks to parents about economics	0.006	0.121
	(0.069)	(0.054)**

*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%.*

Table A.5: The effect of the financial literacy program on the probability of inconsistent choices in the incentivized saving task

Dependent variable:	Probability of inconsistent choice	
Treatment sample:	Jan.-March	April-Jun.
Treated	-0.018	-0.024
	(0.029)	(0.020)
Interest rate	0.062***	0.043***
	(0.011)	(0.006)
Immediate payment	-0.008	0.008
	(0.015)	(0.013)
Delayed payment	0.021	-0.010
	(0.014)	(0.012)
Sample size	3,510	4,272
Standard deviation dependent variable	0.33	0.32
R^2	0.01	0.01

*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 an indicator of choice inconsistent with revealed preference, if euros allocated to earlier date increase when interest rate increases. Stratification dummies included. Standard errors (in parentheses) are clustered at the school-grade level. * significant at 10%, ** significant at 5%, *** significant at 1%.*

Table A.6: Decomposing choices into present bias, patience and curvature of the utility function

Panel A: Determinants of log(euros allocated to earlier date)-log(euros allocated to later date)						
Estimation method:	(1) OLS			(2) Tobit		
Treated*Delay	-.348			-.616		
	(.179)*			(.532)		
Treated*Ln (Gross Interest Rate)	-.625			-1.201		
	(.461)			(.532)**		
Treated*Immediate	-.098			-.361		
	(.195)			(.530)		
Delay (1 vs 2 weeks)	.852			1.549		
	(.113)**			(.294)**		
Ln (Gross Interest Rate)	-5.249			-7.895		
	(.336)**			(.367)**		
Immediate payment	-1.121			-1.991		
	(.164)**			(.361)**		
Treated	.619			.806		
	(.362)			(.633)		
Panel B: Discounting and Curvature Parameter Estimates						
	Treated	Control	Difference	Treated	Control	Difference
Weekly discount factor	.918	.850	.068	.902	.821	.081
	(.022)	(.017)	(.029)	(.032)	(.033)	(.046)
Present bias	1.231	1.238	-.007	1.295	1.287	.008
	(.022)	(.042)	(.047)	(.0654)	(.069)	(.010)
CRRA curvature	.830	.809	.020	.890	.873	.017
	(.009)	(.012)	(.015)	(.006)	(.005)	(.008)

Number of observations: 5265 choices (585 students).

Notes: The upper panel shows the coefficients of a regression of the logarithm of the euros allocated to the earlier date minus the logarithm of the euros allocated to the later one. The covariates are those shown in rows, plus strata fixed effects and dummies with choices at baseline. Choices of 0 are given a 1 cent. The Tobit specification accounts for accumulation at 0€in the earlier date (49% of observations). The lower panel shows the discounting and curvature parameter estimates as in Andreoni and Sprenger (2012). The weekly discount factor is calculated as the exponentiated ratio between the coefficients of delay and ln (Gross Interest Rate) in Panel A. Standard errors calculated by the delta method.

Table A.7: Sample composition by strata

	Public schools	Non-public schools
Financial knowledge at baseline		
Fraction of correct answers	0.58	0.62
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

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.8: Balancing tests at baseline by strata

	Public schools			Non-public schools		
	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
Madrid	0.324	0.303	0.438	0.270	0.358	0.357
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
Prefers 100€ today to 120 in 3 weeks	0.259	0.265	0.628	0.288	0.280	0.754
Prefers 100€ today to 150 in 3 weeks	0.160	0.132	0.311	0.137	0.120	0.493
Prefers 100€ today to 180 in 3 weeks	0.080	0.080	0.791	0.059	0.058	0.764
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 and notes: see Table A.7.

Table A.9: 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)	
Panel C: Hypothetical saving choices (March 2015)			
4. Earlier choice (pooled)	-0.017	-0.038**	0.373
	(0.017)	(0.016)	[0.791]

*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%.*