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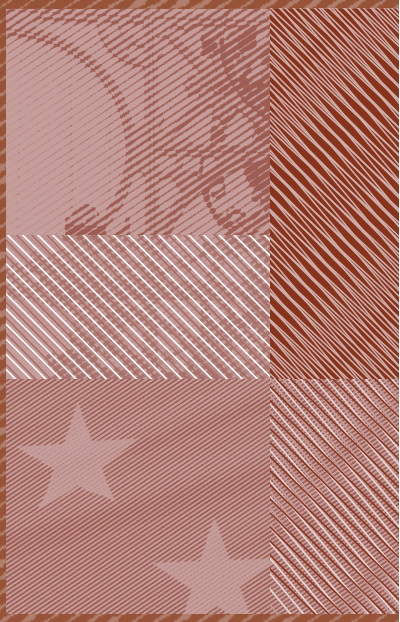

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Abstract

We study how a 10-hour course about personal finance delivered in compulsory secondary education affects a wide range of student's outcomes over a three months horizon. The contents of the course covered budgeting, banking relationship and saving vehicles, but also awareness about future outcomes. To obtain reliable estimates, we conducted a randomized field experiment where 3,000 9th grade students coming from 78 Spanish high schools received financial education at different points of the academic year. Right after the course, performance in standardized tests of financial knowledge increased by 16% of one standard deviation, and treated youths were more likely to become involved in financial matters at home and showed a higher degree of patience in hypothetical saving choices. An incentivized saving task conducted three months after delivering the course suggests that treated youths displayed more patient choices at various interest rates and maturities than a control group of 10th graders. The results of higher performance in financial test scores and the higher degree of patient choices in the incentivized saving task among the treated are statistically significant in strata with students with a relatively more disadvantaged background.

Keywords: financial education, impact evaluation.

JEL classification: D14, D91, I22, J24.

Resumen

Este estudio analiza cómo un curso de 10 horas sobre finanzas personales desarrollado en la Educación Secundaria Obligatoria afecta al conocimiento financiero y a las decisiones de los estudiantes participantes, en un horizonte temporal de tres meses tras su finalización. Los contenidos del curso abarcan el diseño de presupuestos familiares, las relaciones con las entidades bancarias, los vehículos de ahorro, y las consecuencias futuras de decisiones de ahorro y de endeudamiento. Para obtener estimaciones fiables de su efecto, se asignó aleatoriamente el momento del curso académico 2014-2015 en el que alrededor de 3.000 estudiantes de 3.º de la ESO en 78 centros de Secundaria recibieron estas enseñanzas. Justo después del curso, los alumnos tratados obtuvieron calificaciones superiores a los controles en el 16 % de una desviación estándar, además de ser más propensos a involucrarse en asuntos financieros en el hogar y mostrar un mayor grado de paciencia en opciones hipotéticas de ahorro. En una prueba incentivada de ahorro realizada tres meses después de recibir el curso, los jóvenes tratados asignaron al consumo presente una cantidad inferior de recursos (para diferentes tipos de interés y plazos de vencimiento) que un grupo de estudiantes de control de 4.º de la ESO. Los resultados de mayor rendimiento en los exámenes y el mayor grado de elección de opciones pacientes en la prueba incentivada de ahorro entre los tratados son estadísticamente significativos en los estratos con estudiantes provenientes de un entorno socioeconómico menos favorecido.

Palabras clave: educación financiera, evaluación de impacto, decisiones intertemporales de consumo y ahorro.

Códigos JEL: D14, D91, I22, J24.

1 Introduction

In order to equip the general population with the necessary tools for making wise financial decisions, many educational systems have incorporated Financial Education (FE) as part of their curriculum in secondary education. For example, since 1957, various US states have been adopting mandates to include FE in the curriculum of high school students.[1] The consequences of those interventions in the educational system on adults' income, wealth and indebtedness are subject to debate, with some researchers showing increases in net wealth (Bernheim et al, 2001) or credit scores (Brown et al, 2016) while others document much more nuanced impacts (Cole et al, 2015).[2] One reason for the discrepancy can be an insufficient knowledge of how financial education was actually implemented (Brown et al, 2015). A second reason is a lack of understanding of the channels through which financial education works. For example, unless financial education alters the preferences of high school students, their contents could be easily forgotten without affecting students' future financial choices.[3]

In this context, the BdE (the Spanish Central Bank) and the CNMV (the Spanish equivalent to the Security Exchange Commission) launched in the academic year 2012-2013 the program "Finance for All" (*Finanzas para Todos)* aimed at improving financial knowledge among the population. One of the interventions provides basic financial literacy training in the third year of Mandatory Secondary Education in Spain (the equivalent of ninth grade in the US). The general objective of that program is that students become sufficiently financially literate to make sound financial decisions. In particular, the intervention provides teaching guidelines, quizzes, and games aimed to help interested teachers in delivering this new material. The contents were designed for a ten hour-course, possibly given over one quarter.

This paper assesses a randomized trial aimed at gauging the impact of the intervention. As part of the intervention design, 78 schools that applied to deliver the material for

[1] Cole et al (2015) document that 44 states in the US have such mandates.

[2] Bernheim et al (2001) find that investment income and higher equity in real estate were higher among adults who had been exposed to financial mandates than those who had not. Brown et al (2016) use detailed credit data to document that youths exposed to financial education programs in the 1990s had a higher creditworthiness. Cole et al (2015) reexamine the evidence in Bernheim et al (2001) to explore how sensitive are the results to the use of state fixed effects. Brown, Collins et al (2015) highlight the importance of examining compliance with the financial education mandates.

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

the first time were randomly assigned to treatment and control. 9th grade students in treated schools (i.e., students turning 15 years of age by December 2015 under normal progression) received the materials between January and March 2015; whereas 9th graders in control schools went through the course between April and June 2015. In each school, a group of 10th graders who did not receive the course was also surveyed and tested (i.e., students turning 16 years of age by December 2015 under normal progression). We analyze the impact of the materials taught on a wide set of outcomes. We measured financial knowledge via standardized tests delivered in the class, attitudes toward saving and finances elicited through short surveys to students and, finally, actual choices between current and future payments in an incentivized saving task.

Previous studies have examined the impact of similar courses on the financial knowledge of young adolescents and, in some instances, on hypothetical choices (Berry et al, 2015, Lührmann et al, 2013, Bruhn et al, 2013).[4] Other studies focus on the impact of financial training on experimental measures of the degree of patience of students (Lührmann et al., 2016 or Alan and Ertac, 2016). While Alan and Ertac (2016) document that a program aimed at increasing the awareness of the future consequences of current choices did increase the degree of patience of 9-10 year-old kids, Lührmann et al. (2016) document instead a fall in the degree of present-biasedness, but not an increase in the degree of patience. Our first contribution is to examine a broad set of outcomes. Firstly, disparate results across studies on the effect of financial education on students' choices may reflect differences in the contents of the course or to what extent student's understood the material. For example, Brown et al (2016) illustrate that personal finance and economics courses have very different impacts on the behavior of young adults. Our study pays special attention to the specific areas of personal finance in which students improved their scores. Secondly, behavior elicited in incentivized tasks or in hypothetical choices may reflect preference for time or, more generally, changes in the economic situation of an individual (Carvalho et al, 2016, Krupka and Stephens, 2013). Survey responses about changes in attitudes towards consumption or income earning activities give indications of whether changes in behavior reflect changes in preferences or in the student's budget constraint. A final contribution of our study is the variety of schools that participate in the program. Many financial education interventions have focused on a particular set of schools in a few urban areas. The fact that the financial literacy program we examine was

[4] Lührmann et al. (2013) consider a very short course delivered by professionals (4.5 hours), while Brühn et al. (2013) evaluate a 80 hours course.

taught in private and public schools all over Spain, coupled with the stratified design of the allocation to treatment and control allows us to examine if results are heterogeneous in public and non-public schools -a strong predictor of students' parental background.

Our results can be summarized as follows. As regards *financial knowledge*, we document that the course increased the scores in standardized financial literacy tests by one sixth of one standard deviation in a financial literacy test delivered right after the course. The effect was concentrated on topics related to banking relationships and means of payment, while knowledge about budgeting or responsible consumption did not change significantly. We also document that several measures of *attitudes* changed immediately after the program was delivered. The fraction of students who never talk about economics with their parents fell by about 4 percentage points (baseline: 21%) and, secondly, there was a similar increase in patience measured by hypothetical consumption choices between consumption today and in three or six weeks time. In addition, we document increases in forms of informal labor supply among treated students, like working for money in family business or getting money in exchange of household tasks. However, there are no changes in outcomes involving a heavier engagement in the labor market like taking up occasional jobs or selling things in the market. Furthermore, three months after the course was delivered, we conducted an *incentivized saving task* in a subsample of students in Madrid in the spirit of 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. Using 10th graders as controls, we find that treated students allocated in June an amount to sooner payments that was lower than that of controls by 17% of one standard deviation.

Overall, the fall in the preference for early choices both in hypothetical and incentivized tasks while, at the same time, youths engage more in money earning activities at home is consistent with the hypothesis that financial education changes students' awareness about the importance of future resources not only through changes in preferences for the future, but also through changes in their available resources.[5] On the contrary, we find less evidence that this program changed students' behavior by improving their budgeting skills or by lowering possible present biasedness.

A final contribution of our research design is that it allows us to examine separate responses in public and non-public schools. Compared to their peers in the non-public

[5]Those results are also consistent with recent studies showing how knowledge about financial literacy is spread through social interactions (Haliassos et al, 2017).

system, students in public schools are more likely to be grade repeaters, expect to leave the educational system before professional specialization or have parents not working. We highlight two findings from that subgroup analysis. The first one is that while mean responses of financial knowledge are very similar across both types of schools, changes in the distribution of test scores are not. In public schools, we observe a marked improvement in the lower part of the distribution of scores, possibly due to grade repeaters and students expecting to drop-out early. Secondly, the results from the incentivized saving task also suggests a statistically significant propensity to delay payments among students in public schools. Both findings indicate that financial education can motivate students with relatively weaker performance.

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

2 Description of the program

Since 2012, every year about 400 high schools in Spain have delivered a 10-hour financial education under the BdE-CNMV program. Although the implementation varies across centers, participant students are typically 9th graders (that is, third grade in compulsory high school, or "*Tercero de la ESO*", in Spanish). Assuming normal progression through the educational system (i.e., in the absence of grade retention), students complete 9th grade between their ages of 14 and 15. That particular grade was chosen to maximize the potential number of students who receive the material, as 9th grade is the last grade of compulsory schooling with few, if any, electives. Compulsory education finishes at age 16 in Spain in 10th grade.[6]

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

2.1 Contents of the mini-course

The course covers the following areas.

- The first area introduces the notion of saving, which is presented as a means to achieve future consumption possibilities. In that setting, students are introduced to the notion of interest rate and interest rate compounding. In addition, the module introduces the notion of risk associated to different investment choices.

- A second set of modules includes notions on how to elaborate a budget to be able to save and meet future needs. For example, students learn about the allocation of regular and irregular expenses in a monthly budget.

- A third set of modules deals with "responsible consumption", aimed at characterizing conspicuous or environmentally unfriendly expenditure.

- A fourth set of modules introduce the different types of bank accounts. Students are presented with the concept of commissions and fees, as well as on the trade-off between liquidity and return. That part also covers basic security rules in checking and saving accounts.

- There are two final modules on specific investment vehicles, such as pension funds and insurance vehicles.

Teachers had discretion over the emphasis and the order of the topics to be covered.[7] In many instances, as 10 hours of new material are difficult to introduce in a single subject, several teachers taught different modules in each of their subjects -for example, by teaching the interest rate compounding module in Maths while the rest of the material in Social Sciences. We provide below survey evidence on how schools delivered the material.

3 Evaluation features

3.1 The evaluation sample

The population of interest are 9th grade students in high schools applying to participate in the program for the first time during the 2014-2015 academic year. Neither the teaching body nor students in the school had had any previous experience on the contents of the specific program. The impact of the program in this particular set of schools is informative

[7] For more details, see Hospido et al (2015).

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. 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] The fourth round of applications was received shortly before the beginning of the program, we stratified only on the grade in which schools intended to teach the material, to maximize the acceptance rate. There are 16 strata in total -see Table A.1 for details.[9]

The randomization was conducted before schools were presented the conditions to participate.[10] Emails and ordinary mail letters were sent to each teacher or school principal who applied for the program communicating that, due to the evaluation, participation in 2014-2015 was conditional on accepting a set of conditions. Firstly, the material was to be delivered in regular school hours to 9th graders (and only to 9th graders). Secondly, all 9th graders receiving the course would take three financial literacy tests: in December 2014, March 2015 and June 2015. Thirdly, 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] After sending the letters, we phoned each applicant school to explain the requirements in person and answer any question. Out of 169 schools contacted, 78 schools agreed to participate under those conditions -see Table A.1.

The geographical coverage of the final sample is quite broad but not representative of

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

[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] 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.

[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.

the universe of Spanish schools. Seventy percent of centers are located in three regions: Madrid, Aragon and Valencia.[12] Fifty-eight percent of the schools are public and eight percent are private schools. The remaining third of schools were "concerted" ones - i.e., publicly funded but privately owned and managed. The final sample of 9th graders we use contains 3,055 students in the baseline measurement. Most of the analysis uses a balanced sample of 2,734 9th graders (see Table A.2).[13]

3.2 Design of the evaluation and empirical methods

The design of the evaluation is shown in Table 1. In December 2014 students took a baseline financial literacy test as well as a short survey on demographics during a fifty-minutes class In March 2015 students took a second financial literacy test and an additional survey of similar fifty minutes 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 incentivized task where students could choose between current and future consumption at different interest rates and maturities. Due to budgetary considerations, only 10th graders in the schools in Madrid (20) did the incentivized saving task.

The financial literacy test and the survey conducted in March 2015 allow us to compare 9th graders in treated schools (those teaching in January-March 2015) to 9th graders in control schools (those teaching in April-June 2015). That comparison delivers plausibly unbiased estimates of the effect of the financial literacy course on short-run financial knowledge and attitudes of young adults. Furthermore, we can also test for the presence of other confounding interventions or possible spillovers at the school level by comparing those very same outcomes between 10th graders in treatment and control schools.

The financial literacy test and the incentivized saving tasks conducted in June 2015 allow us to conduct three additional tests. 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

[12] 28.2% of schools come from Madrid; 23.1% of schools were located in Aragon; 19.2% in Valencia; 6.4% are from Murcia and from Canary Islands, 3.8% from Extremadura and from La Rioja, 2.6% from Andalusia and 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.

[13] The raw sample size is 3,427 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 5,244 students. We use the following selection criteria. Students must have taken either the December or March tests partly and not classified under "special educational needs" (medical conditions, attention deficit, etc). Table A.2 lists the selection criteria.

schools had just been presented the material, while treated 9th graders had received it three months before. 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. Similarly, the results in the incentivized saving task in June 2015 allow us to assess if students who had gone through the course in different moments in time (immediately or three months later) opt for different consumption choices when confronted with the possibility to save at different interest rates and time horizons.Furthermore, in that same incentivized task, we can compare choices of students in 9th and 10th grade to estimate the impact of financial education on consumption choices.

In other words, we mainly run linear regression models of the following 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 it is zero otherwise. $Y^0_{i,s}$ is the value of the variable $Y_{i,s}$ measured at baseline -December 2014- and it is included mainly to improve precision. Finally, X_k are 15 dummies indicating the strata the school belongs to (see Table A.1). $\varepsilon_{i,s}$ is a random error term with unrestricted correlation at the school level -but uncorrelated across schools. Model (1) is estimated for March 2015 (right after the first set of treated students were assigned to receive the course). θ_1 is the impact of the assignment to be taught the course on the outcomes analyzed.[14]

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

[14] In addition, we estimate variants of Model (1) in June 2015. In that specification, $TREAT_s$ measures any differential impact on outcome $Y_{i,s}$ of how long ago the material was delivered, that we interpret as retention of knowledge or attitudes. Furthermore, we use the data in June to detect changes in attitudes toward the future in an incentivized saving task. As all 9th graders had received the material by June 2015, Model (1) uses 10th graders as controls.

Robustness For those outcomes for which we have a comparable measure before- and after the treatment, we also run Differences-in-Differences models. Unlike Model (1), those models pool the observations in December 2014 and March 2015 and do not include controls for the lagged outcome $Y^0_{i,s}$. Differences-in-differences models estimate the impact of the program by netting out from change in each outcome $Y_{i,s}$ between the pre- and post-treatment period for treated schools the corresponding change among control schools.[15] As an additional test, we also include student-specific fixed-effects in the Differences-in-differences specifications.

3.3 Outcomes of interest

We describe each outcome in detail.

3.3.1 Performance in standardized tests of financial knowledge

A group of educational experts had designed a set of more than 200 items for a previous evaluation in 2012. Those were multiple choice questions and were designed to determine if students had acquired competences in "Savings and Financial planning", "Banking relationships" and "Intelligent Consumption". Questions on "Savings and Financial Planning" presented students with a fictional budget (including expected incomes and expenses) and asked about the soundness of the financial situation of that family or the feasibility of reaching certain saving targets in a given period. Questions on "Banking relationships" asked about the characteristics of saving and checking accounts and the meaning of key components of a bank statement. Students were also asked to compute the remaining balance in a checking account at a future date given an expected flow of revenues and expenses and an initial balance or, in other assessments, to compare the return of different savings accounts, taking fees into account. Finally, questions on "Sustainable Consumption" posed fictional situations where a given need could be satisfied in

[15] That is, the model ran is

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

For all observations in March $AFTER_t = 1$ (0 otherwise). Similarly, for students in the treated schools, we set $TREAT_s = 1$ (0 otherwise) in both periods. The impact of the program is identified as the interaction between $AFTER_t$ and $TREAT_s$. The model also includes dummies indicating the strata of the school. The addition of strata specific dummies implies that the coefficient γ_1 is the difference between the average change in the school-level outcome $Y_{i,s,t}$ between December and March among treated schools (net of the pooled strata-specific average) and the same difference among control schools (again, net of the pooled strata-specific average).

alternative ways. The students were to identify which form was healthier or environmentally friendlier. We drew on these questions and on the tests designed for the previous evaluation to elaborate three different tests of 30 items each. No student faced the same question twice.

3.3.2 Sources of income, saving, attitudes in hypothetical questions

After each test, students filled a short survey containing a few questions about their attitudes toward saving, following the 2012 PISA Financial Assessment questionnaire. Students were asked about their sources of income (work, allowances, occasional sales, etc.) and their attitudes toward saving (whether they save regularly, occasionally or nothing at all), where the responses to the latter question are usually taken as an indication of planning –see Lührmann et al (2013). Finally, the questionnaire asked students if they held a bank account or a money card -both standard measures of financial inclusion- and whether students 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, 2015, Bruhn et al, 2013 or, in another sertting, Haliassos et al, 2017).

In addition, students were asked about their time preferences and their expectations. Firstly, we asked each student four hypothetical choices between receiving 100 euros today and an amount of euros in three weeks' time that ranged from 120 to 180 euros. While those questions are designed to measure preferences over time, Krupka and Stephens (2013) document that preference for current income increases in worse times. Hence, those hypothetical choices between current and future income also capture the market cost of bringing resources to the present. Regarding expectations, students were asked which educational grade did they expect to complete.

3.3.3 Attitudes toward saving in an incentivized task

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

3.4 Compliance

The degree of compliance was measured immediately before the beginning of the course, via surveys addressed to the principal about the plans to teach the course within the school. In addition, we obtained information about implementation details via on-line surveys in March 2015 (for treated schools) and June 2015 (for control schools). 55 teachers (out of 58) in the 35 treated schools answered the March 2015 survey. The median number of hours devoted to the course was 10, and only 25% of students received less than the recommended 10 hours. About one quarter of students received more than 17 hours between January and March 2015 -see Table 2.

Twenty-nine percent of students received the course during the weekly tutorial (a one-hour class where teachers discuss matters related to the educational process and to students' professional prospects). Sixteen percent of students received the material in social sciences and thirteen percent as part of the mathematics curriculum.

The specialization of teachers was diverse. Some thirty percent of teachers reported Economics as their main specialization, while another thirty-four percent specialized in other Social Sciences. According to their own reports, the average teacher delivered seven out of the ten lessons available and compliance was lowest with the modules devoted to advanced saving vehicles, like pension funds, and insurance products. An almost universal comment was that there was "too much material". The teachers' degree of satisfaction with the course was seven in a scale from zero to ten.[16]

We detected three main forms of noncompliance through surveys and personal contact with the teachers.[17] Firstly, one school assigned to teach the material in January-March 2015 reported having taught the course not in this second quarter, but in the third one. Secondly, another treated school delivered some material prior to the pre-test. A third school dropped out in February 2015 because students refused making tests. In what follows, we include the first two cases in the analysis so that estimates can be interpreted as intent-to-treat estimates where both noncompliant schools are still considered as treated.

[16] Teachers received no special reward for teaching the course -other than a diploma that they could add to their vita -that all teachers but one requested. 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] The survey mentioned that we understood that many unexpected developments may occur during the academic year, so to properly analyze the data, it was very important to report any deviation from the protocol.

4 Balancing at baseline

Table 3 reports the baseline characteristics of the sample. We classify student characteristics in four groups in addition to the stratification variables: demographics, sources of income, attitudes toward saving, and parental characteristics. We present in the first two columns the mean characteristics of treated and control students and the third column shows the p-value of the coefficient of the variable $TREAT$ in separate regressions with the characteristic on the left hand side and stratification dummies as additional covariates.

The fraction of females is slightly higher in treated schools than in control schools (52% females in the treatment group, while it is 49% among controls). The fraction of migrants is also higher in treated schools (13.6% vs 11.1%). Grade repeaters (namely, students whose exact age was above what normal grade progression would imply) is 7.8 percentage points larger in treated than in control schools. However, once we condition on the stratification dummies, the difference is not statistically significant at usual confidence levels. The (unadjusted) fraction of correct answers in the financial knowledge test at baseline -measured by the December test- is remarkably similar across groups: both treated and control answered correctly around 59% of the questions.

Turning to the financial situation of youths, their engagement in income-earning activities is relatively low: 35% of treated students report collaborating in the family business or doing home chores for pay. 79% of students report receiving (unconditional) allowances. In neither of those characteristics we find significant differences between students in treated and control schools.

Regarding attitudes towards finance, about one quarter of students never talk to their parents about economics at baseline. Similarly, 27.6% of students report some form of impatience in hypothetical saving choices, as they prefer 100 euros today to receiving 120 euros in three weeks' time. Unsurprisingly, the fraction of students who prefer 100 euros today falls to 14% as the hypothetical payoff increases to 150 euro and to 7% when the future payoff is 180 euro. Treated and control students were very similar in all those dimensions.

5 Results immediately after the course (March 2015)

Financial knowledge

Panel A in Table 4 presents the impact of the financial literacy course on short-run financial knowledge. Students in treated schools improved their performance in the financial literacy test by 14% of one standard deviation (standard error: .07). The result

becomes more precise when we control for dummies indicating the strata the school belongs to in the second column of Table 4. Finally, the last two columns 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) -see Table A.1. Those changes improve precision, but have no noticeable impact on the mean impact on financial knowledge. The magnitude of the improvement is in line with the findings in Bruhn et al. (2013), Hospido et al. (2015) or Walstad et al (2010), who report a positive impact of financial literacy courses in high schools.[18] Figure 1 presents the cumulative distribution function (CDF) of the (unadjusted) fraction of correct answers in the March test of treated and control students. The cumulative distribution function of scores of treated students is almost everywhere shifted to the right relative to that of controls, indicating an overall improvement in financial knowledge -as measured by the test. However, the gains are less obvious at the bottom of the distribution of test scores. We come back to that finding below.

To get further insights about which particular components drive the increase in financial test scores, Model (1) is reestimated separately for four different subscores: "Saving and Budgeting", "Financial Relationships" (further subdivided into "means of payment" and "relationships with banks") and "Intelligent Consumption". The results are shown in Table A.3 and indicate that treated students improved mostly on "banking relationships" (scoring 3% extra correct answers in that subscore). On the other hand, the impacts on "saving behavior" or "intelligent consumption" were either small (in the first case) or too imprecise (in the case of "intelligent consumption").

Panel B in Table 4 shows two different tests. The first one analyzes if 10th graders in treated schools could have been affected by the material received by 9th graders -for example, because teachers use the material of the course in their courses. The results reject the possibility of spillovers of that sort, as 10th graders in treated schools did not perform any better in the financial literacy test than 10th graders in the control schools. Secondly, Panel C in Table 4 examines if the difference in financial knowledge between treated 9th graders and control 9th graders is still present in June 2015, once all 9th graders had taken the course. The average scores in the financial knowledge tests are remarkably similar in June, a finding that is consistent with the hypothesis that 9th

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

graders who received the course between January and March had forgotten little of the material taught three months before.[19]

Saving attitudes

The probability of talking to parents about economics to financial education is modeled by an ordered probit where each threshold indicates the frequency of talking to parents indicated in the columns of the Table 5. The upper panel in Table 5 shows that the share of students who talk to parents about economics increased among treated students, relative to controls. The overall impact is driven by the 4 percentage points reduction in the proportion of treated students who never talk to parents about economics -the proportion at baseline is 20.6% (see Table 5, Panel A, row 1, column 4).

We also document a decrease in the preference for current income in hypothetical choices among students who went through financial education -see the Panel B of Table 5. The specification in that Panel regresses an indicator of preferring income on the day on the test than in three or six weeks time on the variable $TREAT$ as well as indicators of each choice at baseline and the stratification dummies. Columns (1) and (2) of Panel B in Table 5 show that among treated students the fraction who choose 100 euro today over 120 in either three weeks or in six weeks fell between 4% and 5%, respectively. However, among treated students, the coefficients in row 2, columns 3 and 4 imply no changes in the fraction of students who prefer 100 euro today to either 150 euros in three weeks' time or 180 euro in three weeks time. While exposure to financial literacy did not change the fraction of students with a strongest preference for current income, the fraction of earlier choices diminishes when treated students are confronted with relatively low interest rates. Column (5) in Panel B of Table 5 shows a specification where the responses to all 4 hypothetical choices are pooled (i.e., each student contributes four observations to the regression). Youths in treated schools are 2.6 percentage points less likely to prefer income on the day of the test (standard error: 1.2 percent).

A tentative conclusion is that exposure to financial literacy courses changes the distribution of attitudes in the short run. As a result of financial courses, we observe a fall in the fraction of students reporting little interest at home in financial matters and an overall preference for income today. The fall in preferences for current income means among treated students is consistent with at least two explanations. The first is that exposure

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

to financial literacy diminishes the true rate of time preference. The second is that, for some reason, students feel now less "credit constrained" -see Krupka and Stephens (2013) or Carvalho et al. (2016). Below we use information on the students' reported sources of income to disentangle between both explanations.

Asset holdings

In December 2014 (baseline) and March 2015, the student's questionnaire asked youths whether or not they had a bank account or a money card, if they engaged in saving and about their different income sources. Holding bank accounts or money cards are standard indicators of financial inclusion. Actual saving behavior is also a measure of a positive attitude towards finance, although it is not obvious that unconditional saving is the optimal course of action in general for those youths.

Table 6 Panels A and B present the responses of two saving outcomes. Students in treated and control schools do not differ much on the decision of holding a bank account or a money card. If anything, there is a 1% increase in the probability of holding a bank account, but the standard error is about 2 percentage points, so the results on that outcome are not conclusive. There is a small positive impact of the program on saving behavior, as the fraction of students who report that they do not save fell by 1.2 percentage points. However, the impact is statistically different from zero at the 11 percent confidence level only, and the population who does not save at baseline is only 6%. The saving behavior of at most a limited number of youths is affected by exposure to financial education.

Labor supply

The last attitudinal outcome we examine refers to the different income sources of youths. Columns (1)-(4) in Table 7 detail the impact of the program on each income source. The fraction of treated students reporting income in exchange of tasks at home increases by 4 percentage increase, relative to a baseline of 27%. The fraction of students who report working in the family business increases by 2.5 percentage points - from a baseline of 6%. Column (5) in Table 7 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 in markets, working for money in the family business or obtaining income in exchange of housing tasks). The variable takes value zero if the student only reports sources of income that do not involve an exchange of services, such an unconditional allowance. Students in treated schools are 3.6 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 is consistent with previous findings by Berry et al. (2015), who also document similar results among Ghanaian children following a financial literacy course, but not with those in Lührmann et al. (2016), who focus on disadvantaged German youths.[20]

We make three notes regarding the results obtained thus far. The first is that exposure to financial education increases the domestic labor supply of youths (as they become more likely to obtain money in exchange from chores at home or working in the family business) but does not generate "market" labor supply. That is a plausible short-run response because over a three-month horizon, doing household chores or working in the family business are less costly forms of labor supply than searching from jobs outside home or finding goods to sell in the market. Secondly, the increase in "domestic" labor supply is consistent with the change in attitudes reported in Table 5, where we documented an increase in the fraction of youths who discuss financial matters at home. A possible explanation is that part of financial matters discussed regards the exchange of services documented in Table 7. Finally, as mentioned above, the fall in the degree of preference for current income among treated students could be due either to an increase in the degree of patience or, alternatively, to a higher availability of resources that make present needs less pressing. The increase in domestic labor supply documented in Table 7 suggests that the decrease in the preference for current income could be associated with an increase in the sources of income available.

6 Outcomes in an incentivized saving task (June 2015)

A second measure of patience was elicited through incentivized saving tasks in June 2015 -three months after the end of the quarter when students treated between January and March received the course. Measuring outcomes at various points in time is important to establish how likely it is that attitudes change after exposure to financial education. In addition, we have analyzed hypothetical choices thus far, that may not reflect accurately actual preferences if there is not a payoff for responding accurately. Hence, we implemented in June 2015 an incentivized Convex Time Budget Choice (CTBC), a widely used

[20] As a robustness check, Tables W1 and W2 in the Web Appendix re-estimate selected Models in Tables 4-7 using Difference-in Differences estimators that hold constant either strata-specific dummies or student-specific fixed effects. 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.

task where subjects are given the choice of splitting resources between present and future consumption with varying interest rates and maturities.[21]

Youths were presented with nine sequential choices asking them to allocate 6 euros between payments at various dates and with varying interest rates. It was announced that the payment would take the form of USBs with different capacities in different moments in time, according to their choices. The choice of that sort of payoff was driven by the consideration that USBs are homogeneous goods whose attractiveness mainly varies along one dimension (storage capacity). We considered it is easier to frame choices of USBs at different moments in time as a saving decision than, for example, presenting students with baskets of goods in different moments in time. The choices were then between obtaining one (or more) USBs with a given storage capacity in the day of the task or a set of USBs with a larger capacity one or two weeks later. The USBs had the logo of the *Finance for All* program and their storage capacity ranged between 2 GB and 32 GB. Choices were framed in their equivalent monetary values.[22] While the choice of USBs as a payoff implies that our results must be interpreted as preferences over time for this particular durable good, Table 8 documents a positive correlation between the choices in the incentivized task and the baseline preferences for time in hypothetical choices.[23]

Given the limited period span imposed by the end of the academic year we chose very large interest rates: 100%, and 200%. The students had to allocate payoffs between the day of the task (today) and one week from that date. They were then presented similar choices between the day of the task and two weeks from that date and between one and two weeks from the day of the task (see Table A.4 for an example). After the application, one of the nine choices were chosen at random chosen and one randomly chosen student in the group would be awarded her choice. When the student's choice involved obtaining some USB in one or two weeks' time, the payoff was given to the teacher in an envelope with the delivery date written on it.

As by June 2015 all students had already gone through the course, we use students in 10th grade (4 ESO) as the control group in this experiment. While the median age of the control group is one year older than that treated students, other comparisons suggest

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

[22] We assigned to each USB a value in euros similar to market prices at the time of the task. In that manner, one 8GB pen-drive would be equivalent to 6 euros, and one 32GB was presented as 12 euro (similar to market prices)

[23] Lührmann et al. (2016) discuss that monetary payoffs only recover preferences for time if subjects in experiments in Convex Time Budget tasks incur in "narrow bracketing" (i.e., if they consider payoffs in the experiment separately from their own resources at home). Whether or not those concerns apply to USBs is unclear.

that 9th and 10th graders were similar. For example, when asked in December about their hypothetical preference for receiving 100 euro on the day of the survey or 120 euro three weeks later, 23% of students treated between January and March 2015 preferred 100 euro on the survey date, while the corresponding number among 10th graders was 27% (the difference is not statistically significant). When the payoff for waiting three weeks was increased to 150 euro, the fraction who chose the sooner hypothetical payment fell to 12%, and it was 6% when the future payoff increased to 180 euro -see Figure 2. According to that Figure, the behavior among 10th graders was remarkably similar, which reassured us in the use of the group as a control for 9th graders. The patterns are similar when we examine 9th graders treated between April and June 2015. Finally, we only report the results for Madrid because budgetary reasons prevented us from implementing the CTBC among 10th graders elsewhere.

Figure 3 plots the euros that treated and control students in Madrid allocated to the earlier date. The estimates are population means, unadjusted by strata composition. According to Panel A in Figure 3, controls behave according to economic theory. When the return on each euro saved is 100% (i.e., one euro saved increases consumption possibilities in one week by two euros) students in the control group allocated .99 cents out of the 6 euros to the sooner payment. That amount increased to 1.36 euro when the total interest is held at 100% but the future payment happened two weeks after the task, and diminished slightly to 1.32 cents when the students allocated payoffs between one and two weeks from the date of the task. The amount allocated to sooner payment is not higher when the decision is made on the day of the experiment than when it is one week in advance, contrary to the hypothesis of present bias.

Students treated between January and March 2015 allocated a lower amount to earlier payments than the full set of controls did. When the return of each euro saved was 100% between the day of the task and one week students who went through the financial literacy course between January and March 2015 allocated to the sooner payment 29 cents less than controls did (.29 =.99-.70) -see Figure 3, upper panel on the left. The difference between treated and controls is smaller in absolute value when the return to saving increases to 200% (each euro saved today results in three extra euro of future income) and it is about 7 cents (.07=.72 - .65). In the experiment where one euro saved today increases USB purchase capacity by two euros in two weeks' time, control students allocated 1.36 to the earlier payment, while treated students allocated to the earlier payment 42 cents less (.42=1.36-.94). Even when the interest rate increases to 200% over two weeks, students

treated in January allocated to the sooner payment 25 cents less than controls (.25=1.10-.85). Finally, the differences between treated and control students over early choices in the one vs two weeks experiment are qualitatively similar to those between today and one week. The pattern is also qualitatively similar to that detected in the hypothetical choices in Table 5, where early consumption choices are less common among treated students for intermediate values of the interest rate, but the case is less clear when interest rates are very large.[24]

Table 8 summarizes the evidence in regression format. The dependent variable in column (1) is the euros allocated to the earlier payment, while the key independent variable is an indicator of being a 9th grader in the set of schools that received the financial literacy course between January and March 2015 (i.e., 9th graders receiving the course between April and June 2015 are not included in this regression). We also include as regressors the interest rate in each choice, the lag between payments, three indicators with the strata the school belongs to and indicators expressing preference for sooner hypothetical payments in December 2014.[25] We cluster standard errors in this case at the school-grade level.[26] Across all choices, students receiving the material in January-March 2015 chose in June 2015 allocations that involved 27 cents lower early consumption than controls. Possibly due to the small sample size and the heterogeneity of responses across interest rate levels, the p-value of the estimated coefficient is 8.5 percent. The 27 cents reduction of the amount allocated to the sooner payment amounts to 18% of one standard deviation of the euros allocated to the sooner date (1.49 euro, shown at the bottom of Table 8).[27]

The second column in Table 8 compares the euros allocated to the sooner payment by students who went through the material between April and June 2015 to those chosen by the full group of controls -see panel B in Figure 3 for unadjusted differences. In this case, treated students also reduced the number of euros allocated to the earlier date, but

[24]Table A.5 examines the patterns in Figure 3 in a regression format controlling for strata dummies and the preference for current income in the December 2014 test.

[25]The constant reflects the euros chosen in the earlier date by students in public schools requesting the material before September 2014 (stratum 1 in Table A.1) who prefer 120 euro in two weeks to 100 euro today. Inclusion of choices at baseline improve precision, but do not alter the magnitude of the coefficients. Table A.5 presents regression results by each interest rate and the lag between payments.

[26]We chose that level of clustering as 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.

[27]Table W3 in the Web Appendix shows the full set of estimates in selected Models in Tables 4-7. In particular, the Table details the coefficient of each stratum and the lagged outcome variable, included in all models but not shown in the main Tables to save space.

the magnitude of the reduction is 12 cents, half that of the January-March group and not statistically different from zero.[28]

On the other hand, several studies establish that a violation of the law of demand occurs if, for a given payment lag and a previous choice a student chooses a *higher* sooner payment when interest rate *increases*. Namely, if youths choose an allocation of current and future consumption for a given interest rate R, an increase in that interest rate R cannot make saving less attractive, so students should not increase the amount allocated to the sooner payment. Using that definition, eleven percent of the choices of students in the control group involved a violation of the law of demand -alternatively, it can be considered an optimization error. Columns (4) and (5) in Table 8 examine whether financial education improved the quality of students' financial decision-making by regressing the presence of such errors on a dummy of $TREAT$, as well as strata dummies. We find little evidence that financial education reduces the probability of making such errors. Finally columns (6)-(9) in Table 8 re-examine the impact of financial education on the euros allocated to the sooner payment in a sample without inconsistent choices. The results are qualitatively similar to those shown in columns (1)-(3), but more precise.

Overall, students treated between January and March displayed more patient choices than controls at various interest rates and maturities, the results being more imprecise for the group that received the course between April and June 2015. While we do not have a good explanation for the lower response of the group that received the material closer to the date of the experiment, the results certainly do not support the notion that the impact of financial literacy programs on preferences vanishes three months after the program was delivered.

7 Differential responses by type of school

One of the features of our study is the wide variation in the characteristics of students participating in the program. The research design took advantage of this variation and randomized treatment by type of school and region of the school, as both variables correlate strongly with parental characteristics. This section partitions the sample between strata with public schools and non-public ones. Students in public schools are more likely to be born outside Spain (14% vs 7% in non-public schools), to have repeated a grade (28% vs 16% in non-public schools) and to expect leaving education earlier (71% expects

[28] For each experiment, students receiving the course in the third quarter allocated less cents to the earlier date than controls, but the magnitude is much *lower* than in the first group -compare rows 3 and 1 in Table A.5. In fact, when we pool all choices, treated students allocated 12 cents less to the earlier date than controls (standard error: 11 cents).

to finish college in public schools while the comparable estimate is 81% in non-public schools). Furthermore, students in public schools are also more likely to face worse economic conditions, as the proportion of fathers who don't work is higher than in non-public schools (14% vs 9%) -see Panel A in Table 9.[29]

We first compare the performance in the March test and answers to the surveys of 9th graders going through the course between January and March 2015 (treated) and 9th graders receiving the material between April and June 2015 (controls). Panel B.1 in Table 9 presents the impact of financial education on financial literacy test scores in March. Relative to controls of the same type of school, treated students in either public or non-public schools experience similar mean increases in the financial test score: about 17% of one standard deviation (note that the sample does not coincide with that in Table 4, as one stratum is not used in Table 9). However, the distribution of the responses differs across schools. The upper and lower Panels of Figure 4 show the predicted cumulative distribution function of the fraction of correct answers of treated and not treated students in each type of school. In public schools, the fraction of treated students achieving low scores -between 20% and 50% of correct answers- fell by between 5 and 6 percentage points relative to the control group. Conversely, within the set of non-public schools, the distribution of low scores is very similar among treated and control students and the mean 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%.and 5.8%, while the same fraction remain unaltered in private schools (see Table 9, Panel B.1, rows 1-4) In other words, financial education shifted upward the distribution of low scores in financial tests in public schools, but not in non-public ones.

Table 10 examines the issue more closely by further splitting the sample by the level of accumulated human capital -having repeated a grade or not- and by expected schooling -whether or not expect to leave after high school. Both groups (grade repeaters and students expecting to leave early the school system) performed relatively worse in the pre-test. The results in Table 10 confirm that after the course students with a lower level of human capital improved more in the financial test than the rest of students. For example, treated grade repeaters performed 28% of one standard deviation better than

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

grade repeaters in the control group. In contrast, treated non-repeaters outperformed non-repeater controls in the financial test by 10% of one standard deviation (i.e., the magnitude of the response is about one third that of repeaters). Treated students who expect to drop out right after upper secondary school (if not earlier) outperform controls who expect to drop out by that age by 20% of one standard deviation. The improvement in test scores among treated students who expect to finish college is 16% of one standard deviation. Those differences provide an explanation for the improvement in low scores detected in public schools, as they include a much higher share of grade repeaters or students expecting to drop out early (see the upper Panel of Table 9).

Turning to changes in the attitudes toward finances, and in keeping with the results of the main sample, treated students in each strata reported to be more likely to talk to their parents about economics, to choose income in the future when confronted with hypothetical choices and to report a higher probability of receiving income from tasks at home or in the family business -see panels B.2-B.3 of Table 9. However, all those responses are largest in absolute value and most precisely estimated in the sample of non-public schools[30]. In other words, in the strata where treated students appear most patient in hypothetical choices, they are also more likely to talk about economics and obtain additional sources of income from the family. That finding supports the hypothesis that, among students in the wealthier stratum, financial education increases the fraction of youths who talk to their parents, possibly linked to an exchange of services for money.[31]

Measuring preferences for time by means of incentivized saving tasks further illustrates the heterogeneity of responses. Figure 5 compares the euro allocated to the sooner payment in public and non-public schools separately. For each interest rate and delay, the gap between the euros allocated to the earlier payment by treated and controls in public schools is larger than the corresponding gap in non-public schools. For example, when there was a two-week delay between payments and an interest rate of 100%, treated students allocated 53 cents less to the sooner date than controls in public schools. Among students in non-public schools the corresponding difference was 14 cents, a response four times smaller (Figure 5, panel B). Qualitatively similar results hold when we compare students treated between April and June 2015 -shown in Figure 6. Panel B.4 in Table 9 shows

[30] A test of equality of coefficients rejects the null that the coefficient of treated is the same when the dependent variable is "money in exchange of chores at home". For the rest, we cannot reject the null of equality of coefficients.

[31] 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.

the results in regression format. Students in public schools treated between January and March allocated 37 cents less to the sooner payment than controls, while the corresponding difference among students in non-public schools is 6 cents. While we cannot reject the null of equal coefficients, the results suggest that when we measure preferences for time using an incentivized saving task, exposure to financial education changed financial decision-making of students in strata with a relatively poorer background.

Overall, the responses to financial education vary across strata with markedly different parental characteristics. In terms of financial knowledge, similar mean responses to financial education mask rather different shifts in the distribution of financial test scores. In particular, financial education diminished the fraction of low-achievers in financial knowledge in public schools but not in non-public ones. That is a novel result, as grade repeaters are typically very hard to motivate.[32] Secondly, treated students in the stratum with the poorest parental background opt for more patient choices in incentivized saving tasks than controls, while the evidence is less clear-cut among students in the wealthier stratum. Both findings lend support to the view that financial education also affects the financial knowledge and decisions of students with poorer backgrounds.

8 Interpretation of the results

How does financial education change individuals' financial knowledge or behavior? The results from the program we analyze do not suggest that financial education improve their intertemporal decision-making by helping students to elaborate a budget or to improve their ability to adhere to it. For example, the answers to the financial literacy tests do not show large gains in items involving such questions. The results are also inconsistent with the notion that financial education helps subjects in making less present-oriented choices. Were that the case, students would have reduced the euros allocated to sooner payments mainly when these are received the very same day of the task, because present-bias does not affect choices between future and distant future consumption -see Carvalho et al. (2016) or Lühmann et al. (2016). Instead, the fall in the euros allocated to the sooner payment is similar when the decision is made on the date of the first payment or one week in advance.[33] Those results depart from Lührmann et al. (2016), who document

[32]For example, in the context of the math curriculum mandate, Cole et al. (2015) document that exposure to those mandates increases income in adulthood among whites, but not among non-whites.

[33]For example, relative to controls, treated students allocate 31 cents less to the day of the task when the alternative is one week afterwards (Table A.5, row 1 column 1) and 29 cents when the choices are between one and two weeks after the day of the task (Table A.5, row 1, column 5). Such behavior is not consistent with financial education diminishing present bias.

that students receiving a short financial education did not change their degree of patience in a task using monetary payoffs.[34]

The results in our study are consistent with the notion that financial education changes the awareness of students about the value of resources and the future consequences of current choices. Supporting evidence in this respect comes from the analysis of financial knowledge tests, where students mainly improved in their understanding of the workings of simple banking products, such as the functioning of savings and borrowing products that make more salient the opportunity cost of transferring resources over time. In addition, the results from surveys and the saving incentivized task suggest that financial education changes student's attitudes through the budget constraint (as treated subjects report receiving more family income in exchange of chores after the course) and an increased preference for time. The latter result is observed in hypothetical choices between current and future income measured right after the course and three months afterwards, using an incentivized saving task. Those results are in line with those in Alan and Ertac (2016), who analyze an intervention aimed at emphasizing the future consequences of current actions that increases the degree of patience of much younger children. There are however two main differences between their study and ours. Firstly, we study 15 year-olds, who have extra alternatives to raise money to achieve higher income -money in exchange of chores, working in the family business and so on. A second difference is that youths at 14-15 years of age take crucial decisions related to their academic career, like continuing in the educational system or dropping out. Interventions at this critical age may have impacts that go beyond the time frame we study, specially if they mainly affect students with a lower motivation.

In terms of who is affected by financial education, several models predict that the incentive to invest in learning about finance increases with wealth (Lusardi et al., 2017, or Jappelli and Padula, 2013). According to those studies, if the alternative to receiving high school education was receiving information at home, we should expect larger impacts of financial courses among students with low-wealth parents, as they are unlikely to have invested in financial education themselves. The finding that the distribution of financial

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

knowledge becomes more equal in public schools after the course, coupled with a larger increase in knowledge among students who have either repeated a grade or who expect to drop out earlier is indeed consistent with higher impacts among students with less favourable background. In addition, the increase in the preference for time in incentivized tasks in the stratum of students in public schools is also consistent with the notion that financial education increases awareness of the value of waiting among students with a weakest background -at least in the period we consider.[35]

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

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

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Tables

Table 1: Evaluation calendar

	December 2014		March 2015		June 2015
9^{th} 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	
10^{th} 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: * Incentivized saving task conducted only in Madrid schools. November 2014: All teachers invited to Banco de España for a session on the purpose of the evaluation, timetable of the course and going over one of the lessons.*

Table 2: Program Implementation

	Total $N = 55$	Public $N = 33$	Concerted $N = 20$	Private $N = 2$
Number of hours:				
Minimum	4	4	9	–
25^{th} centile	10	8	10	–
Median	10	10	10	15
75^{th} centile	17	13	18	17
90^{th} centile	20	20	22	–
Number of lessons taught (out of 10)	7.0	6.8	7.9	6
Fraction that made independent evaluation	.39	.36	.40	.5
Fraction that assigned homework	.28	.27	.40	0
Subject where material was delivered:				
Math	.127	.061	.15	1
Social Sciences	.164	.122	.15	0
Weekly hour with tutor	.291	.307	.30	0
Citizenship	.109	.152	.05	0
Alternative to religion	.091	.091	.05	0
Other	.228	.267	.30	0
Teacher's specialization:				
Social Sciences	.345	.42	.30	0
Economics	.360	.36	.30	.5
Math	.127	.09	.15	.5
Computing science	.072	0	.20	0
Other	.096	.12	.05	0

Source: on-line surveys to 55 teachers in treated schools (overall number of teachers: 58).
Notes: As many teachers split the material across subjects, the unit of analysis is the class the student belongs to. Hence, the number of hours devoted to the program is the sum of hours across all subjects The subject where the material was delivered is the one using the largest number of hours.

Table 3: Student characteristics

	Treated $N = 35$ schools	Control $N = 43$ schools	p-value of the difference
Variables used in stratification:			
Madrid	.304	.324	
Public school	.601	.663	
Concerted school	.297	.308	
Private school	.099	.029	
Fraction of correct answers in pre-test	.583	.590	.876
Demographic characteristics:			
Female	.524	.495	.192
Foreign born	.136	.111	.440
Older than normal progression	.300	.222	.472
Expected age finish school	21.08	21.42	**.080**
Expect to finish at 18 or earlier	.178	.138	.270
Sources of income:			
Family business/allowance home duties	.352	.328	.695
Unconditional allowances	.791	.771	.134
Occasional jobs	.224	.204	.285
Hypothetical preferences:			
Prefers 100 euro today to 120 in three weeks	.276	.276	.705
Prefers 100 euro today to 150 in three weeks	.151	.130	.169
Prefers 100 euro today to 180 in three weeks	.076	.078	.654
Talk to parents about economics:			
Never	.258	.247	.42
Less than once a week	.306	.312	.88
Once a week	.217	.221	.91
More than once a week	.219	.220	.26
Labor status of father:			
Employee	.566	.569	.946
Self-employed	.269	.276	.566
Unemployed	.091	.091	.380
Does not work/other	.090	.070	.743
Labor status of mother:			
Employee	.525	.500	.678
Self-employed	.156	.160	.581
Unemployed	.091	.091	.524
Does not work/other	.250	.226	.736

Source: Information about demographics comes from the December survey to students. Information about grade repetition (date of birth), stratum and type of school comes from school records.
Notes: Sample of 3,055 students in 78 schools (all in 9th grade). Students with some special educational need or who did not take the December test are excluded.

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

	Unbalanced panel		Balanced panel	
	No strata	Strata dummies	Strata dummies	Strata dummies*
	(1)	(2)	(3)	(4)
Panel A: Treated students vs controls (9^{th} graders). March				
Treated	.138	.160	.157	.167
(S.E)	(.070)	(.075)	(.068)	(.065)
[p-value]	**[.053]**	**[.036]**	**[.023]**	**[.014]**
Fraction correct in pre-test	.55	.55	.47	.47
R^2	.158	.129	.330	.332
Number of students (schools)	3,070 (77)		2,734 (77)	
Panel B: Non-treated students in treated schools vs those in control schools (10^{th} graders). March				
"Treated"	-.087	-.045	-.094	-.104
(S.E)	(.092)	(.048)	(.088)	(.084)
[p-value]	[.346]	[.345]	[.290]	[.220]
R^2	.240	.267	.330	.340
Number of students (schools)	1,569 (77)		1,366 (77)	
Panel C: Treated students vs controls (9^{th} graders). June				
"Treated"	-.007	.025	.019	
(S.E)	(.066)	(.058)	(.054)	
[p-value]	[.915]	[.660]	[.720]	
Number of students (schools)	2,851 (77)		2,567 (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 (S.E) are corrected for heteroscedasticity and arbitrary correlation at the school level.*

Table 5: The effect of the financial literacy program on attitudes

Panel A: Frequency talks to parents about economics					
Talks:	More than once a week	Once a week	Less than once a week	Never	Overall*
Treated	.018	.024	-.002	-.040	.122
(S.E)	(.018)	(.019)	(.015)	(.017)	(.053)
[p-value]	[.336]	[.203]	[.890]	**[.026]**	**[.029]**
Mean dependent variable	.25	.24	.29	.21	
Number of students (schools)		2,714 (77)			2,714 (77)
Panel B: Hypothetical saving choices, prefers euros today vs euros in 3-6 weeks					
Prefers:	100 euro now to 120 in 3 weeks	100 euro now to 120 in 6 weeks	100 euro now to 150 in 3 weeks	100 euro now to 180 in 3 weeks	Earlier choice† (pooled)
Treated	-.045	-.051	-.010	-.007	-.026
(S.E)	(.018)	(.019)	(.014)	(.008)	(.012)
[p-value]	**[.015]**	**[.012]**	[.477]	[.415]	**[.029]**
Mean dependent variable	.29	.66	.12	.061	
Number of students (schools)		2,714 (77)			10,760

*Notes: All models estimated by OLS, including stratification dummies and lagged values of a similar hypothetical choice in December 2014, except * Overall that is the latent index coefficient of an ordered Probit, with outcomes (1)-(4) as different levels of the dependent variable. † Earlier choice pools the four choices and controls for three dummies that indicate the particular choice. The variable treated measures if present always chosen regardless of the future reward. Standard errors (S.E) are clustered at the school level (77 schools).*

Table 6: The effect of the financial literacy program on financial inclusion and savings

Panel A: Financial inclusion					
Holds:	Bank account or money card			Bank account	Money card
Treated		.010		.015	-.014
(S.E)		(.020)		(.020)	(.022)
[p-value]		[.588]		[.442]	[.210]
Mean dependent variable		.57		.51	.10
Number of students (schools)			2,734 (77)		
Panel B: Reported saving behavior					
Saves:	Yes, always same amount	Yes, amount varies	Occasionally	Not	Not enough money to save
Treated	-.004	.005	.005	-.013	.006
(S.E)	(.014)	(.020)	(.017)	(.008)	(.008)
[p-value]	[.744]	[.810]	[.760]	[.110]	[.425]
Mean dependent variable	.16	.48	.23	.06	.06
Number of students (schools)			2,734 (77)		

Notes: All models estimated by OLS, including stratification dummies and lagged values of the dependent variable as of December 2014. Standard errors (S.E) are clustered at the school level (77 schools).

Table 7: The effect of the financial literacy program on sources of income

	Occasional jobs	Sells things (Internet, markets)	Money for tasks at home	Work in family business	Any source of labor income
Treated	.001	-.010	.039	.025	.036
(S.E)	(.013)	(.009)	(.019)	(.011)	(.020)
[p-value]	[.980]	[.304]	**[.046]**	**[.019]**	**[.076]**
Mean dependent variable	.15	.12	.27	.06	.41
Number of students (schools)	2,714 (77)				2,714 (77)

Notes: All models estimated by OLS, including stratification dummies and lagged values of the dependent variable as of December 2014. Standard errors (S.E) are clustered at the school level (77 schools).

Table 8: Results from an incentivized saving task (Convex Time Budget Task)

Dependent variable:	EUR allocated to earlier date			Inconsistent choices		EUR allocated to earlier date, consistent choices		
Sample of treated:	Jan.-March	April-June	All	Jan.-March	April-June	Jan.-March	April-June	All
Treated	-.268	-.123	-.178	-.020	-.024	-.201	-.055	-.114
	(.150)*	(.111)	(.115)	(.029)	(.021)	(.098)**	(.068)	(.072)
Immediate payment	-.278	-.248	-.250	-.005	.005	-.265	-.259	-.260
	(.041)	(.049)	(.040)	(.012)	(.013)	(.049)	(.051)	(.042)
Interest rate	-.205	-.258	-.220	.064	.044	-.420	-.350	-.350
	(.040)	(.027)	(.028)	(.013)	(.006)	(.045)	(.029)	(.027)
Delay	.297	.357	.310	.020	-.010	.340	.469	.320
	(.054)	(.049)	(.047)	(.014)	(.012)	(.065)	(.043)	(.044)
Prefers 100 euro today	.307	.580	.449			.397	.537	.459
to 120 in 3 weeks (Dec)	(.155)	(.096)	(.100)			(.130)	(.100)	(.091)
Prefers 100 euro today	.037	-.220	-.092			.030	-.217	-.114
to 150 in 3 weeks (Dec)	(.270)	(.180)	(.180)			(.180)	(.161)	(.138)
Prefers 100 euro today	.258	.514	.283			.180	.420	.215
to 180 in 3 weeks (Dec)	(.320)	(.201)	(.201)			(.260)	(.173)	(.153)
Sample size	3,534	4,290	6,036	3,534	4,290	3,081	3,780	5,093
SD dep. var.	1.49	1.53	1.51			1.16	1.25	1.21
R^2	.036	.054	.038	0.011	0.014	.075	.078	.066

*Notes: Sample of 1,005 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 10^{th} graders. OLS regressions using as the dependent variable the number of euros (EUR) allocated to the earlier date (columns 1, 2, 5 and 6) and an indicator of choice inconsistent with revealed preference (if euros allocated to earlier date increase when interest rate increases). Stratification dummies included. Standard errors (S.E) are clustered at the school level. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.*

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

	Public	Non-public	
Panel A: Sample composition by stratum			
Foreign born	.14	.07	
Older than normal progression	.28	.16	
Father is self-employed	.24	.32	
Father works as an employee	.57	.55	
Father does not work	.14	.09	
Expects to leave at 16	.05	.02	
Expects to finish at most HS academic track	.12	.07	
Expects to finish at most HS vocational training	.11	.08	
Expects to finish college	.71	.82	
Number of students (schools)†	1,646 (42)	671 (24)	
Panel B: Effects of the financial literacy program on knowledge and choices by stratum			
			H_0: equal coeffs. [p-value]
B.1. Score in financial knowledge tests:			
1. Standardized test score	.170	.173	
	(.096)*	(.075)**	[.971]
2. Fraction with less than 25% questions correct	-.040	.003	
	(.018)***	(.009)	**[.043]**
3. Fraction with less than 35% questions correct	-.058	.020	
	(.030)**	(.027)	**[.059]**
4. Fraction with less than 50% questions correct	-.052	-.070	
	(.037)	(.043)*	[.610]
B.2. Attitudes:			
5. Talks to parents about economics	.079	.152	
	(.077)	(.072)**	[.356]
6. Hypothetical choices (pooled): prefers delayed hypothetical income	.021	.038	
	(.016)	(.016)**	[.441]
B.3. Sources of income:			
7. Money from chores at home/family business	.026	.086	
	(.031)	(.032)**	**[.069]**
8. Money from occasional jobs outside home	-.009	.037	
	(.016)	(.027)	[.505]
9. Any source of income	-.00	-0.00	
	(.016)	(.041)	[.248]
B.4. Incentivized saving task (Madrid):			
10. EUR allocated to the earlier date	-.370	-.065	
	(.200)*	(.140)	[.271]

Notes: † *The sample excludes one stratum that mixes 1 public and 2 non-public centers. That stratum originally grouped high schools who intended to teach the course to* 7^{th} *or* 8^{th} *graders (ESO1 or ESO2). In panel B, each cell shows the estimate and standard error (S.E) 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 coefficients estimated by OLS but the one in row 6, that is the latent coefficient of an ordered Probit, and standard errors are clustered at the school level.* *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.1.

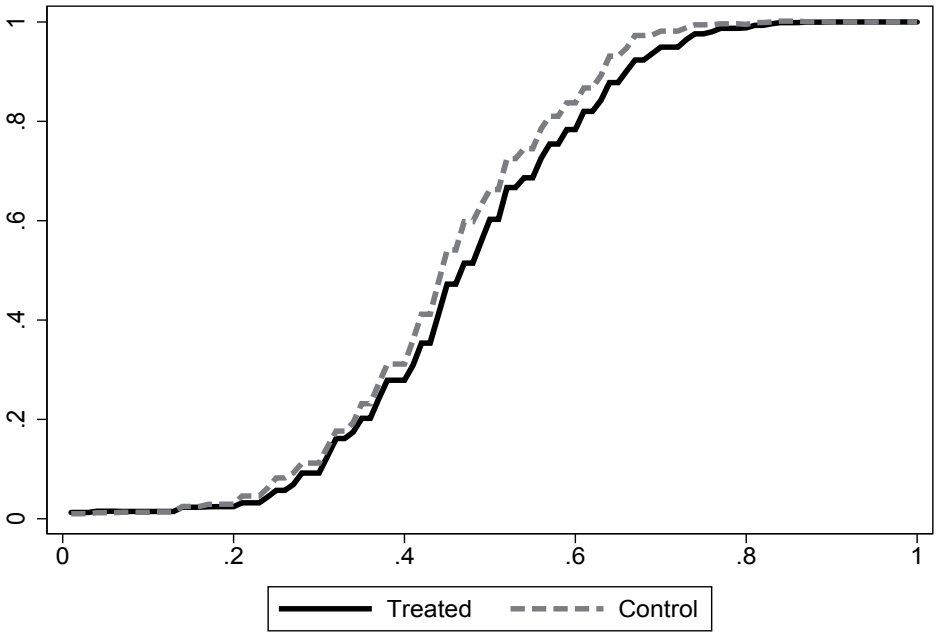
Table 10: The effect of the financial literacy program on attitudes by student progression

	Normal progression	Older than normal progression	Expects to drop-out after 18	Expects to drop-out at 18 or before
Score in financial knowledge tests:				
1. Correct answers in test	.102	.283	.162	.201
	(.063)*	(.057)***	(.057)**	(.094)**
Attitudes:				
2. Never talks to parents about economics	-.055	.005	-.044	-.028
	(.019)**	(.036)	(.016)**	(.049)
3. Hypothetical choices (pooled): prefers delayed hypothetical income	.034	.019	.034	-.002
	(.013)**	(.027)	(.010)**	(0.027)
Sources of income:				
4. Money from chores at home/family business	.059	.017	.067	-.039
	(.021)**	(.042)	(.021)**	(.022)
5. Money from occasional jobs outside home	.003	-.046	.008	-.081
	(.016)	(.030)*	(.016)	(.036)**

Notes: Each cell shows the estimate and standard error (S.E) 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 coefficients estimated by OLS and standard errors are clustered at the school level. *** *p-value* < 0.01, ** *p-value* < 0.05, * *p-value* < 0.1.

Figures

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

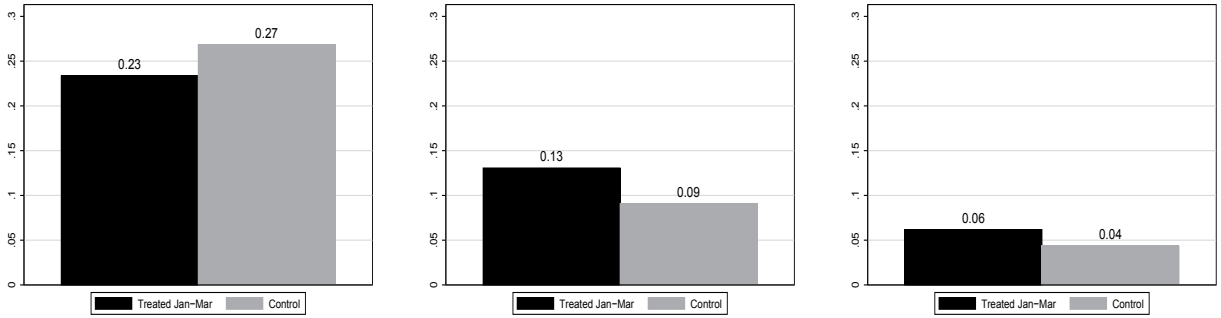


Notes: The horizontal axis shows the fraction of correct answers, while the vertical axis contains the fraction of students. Each point is the predicted proportion of students with correct answers that are equal or below the value in the horizontal axis. Predictions are obtained from OLS regressions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

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

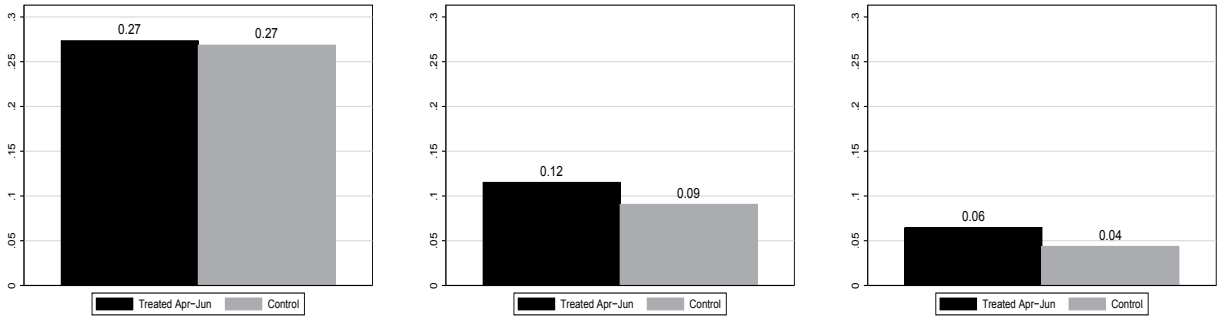
Panel A. Treated in January-March (9^{th} graders) and controls (10^{th} graders)

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 (9^{th} graders) and controls (10^{th} graders)

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

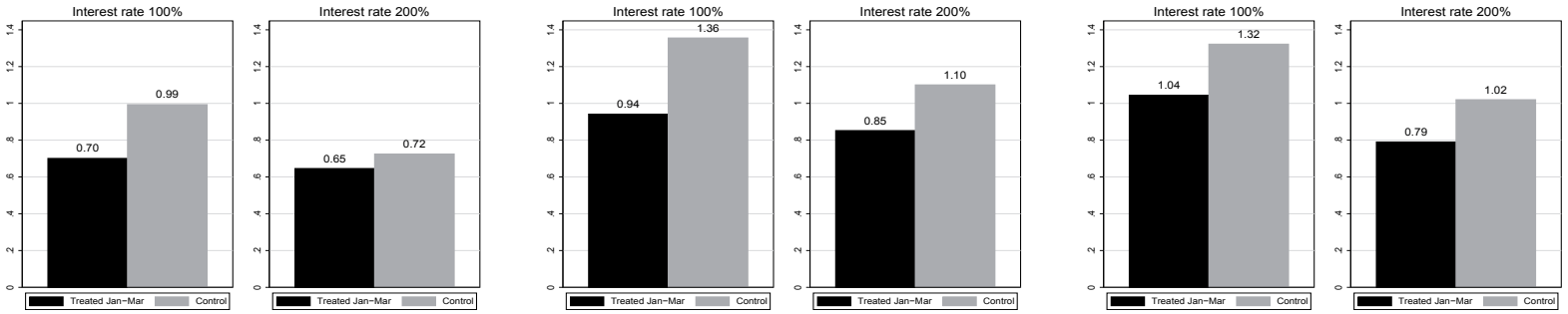


Notes: In panel A (B), treated students are 9^{th} graders in Madrid receiving the course between January and March (April and June). Controls are all 10^{th} graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.1). The black (gray) bars represents the fraction of 9^{th} (10^{th}) graders choosing 100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

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

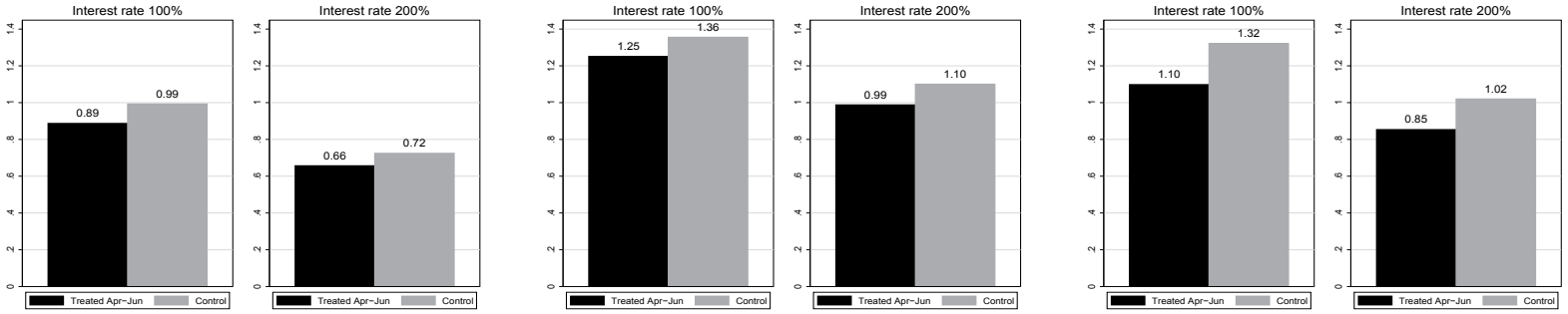
Panel A. Treated in January-March (9^{th} graders) and controls (10^{th} graders)

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



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

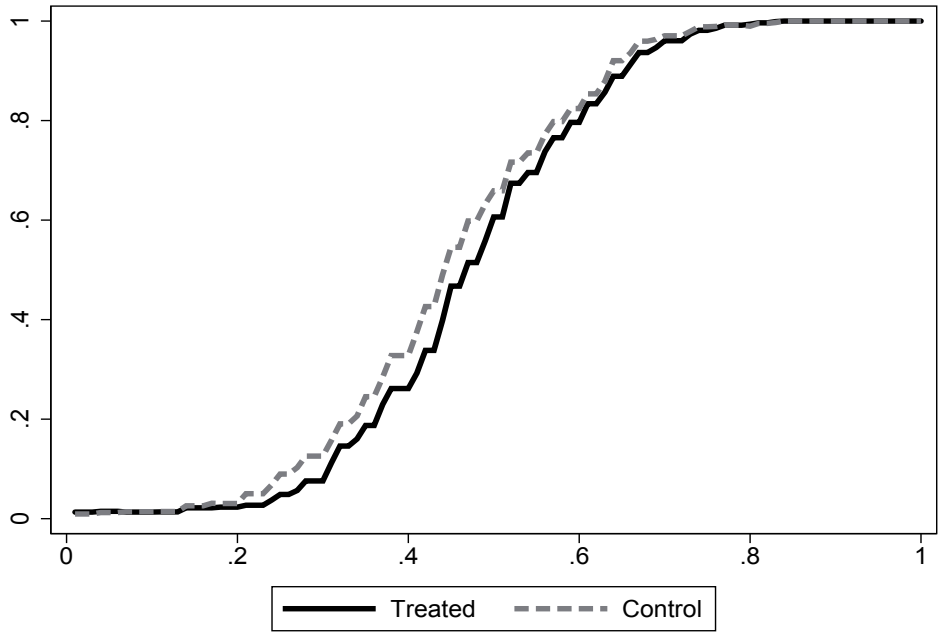
Today *vs.* One week | Today *vs.* Two weeks | One week *vs.* Two weeks



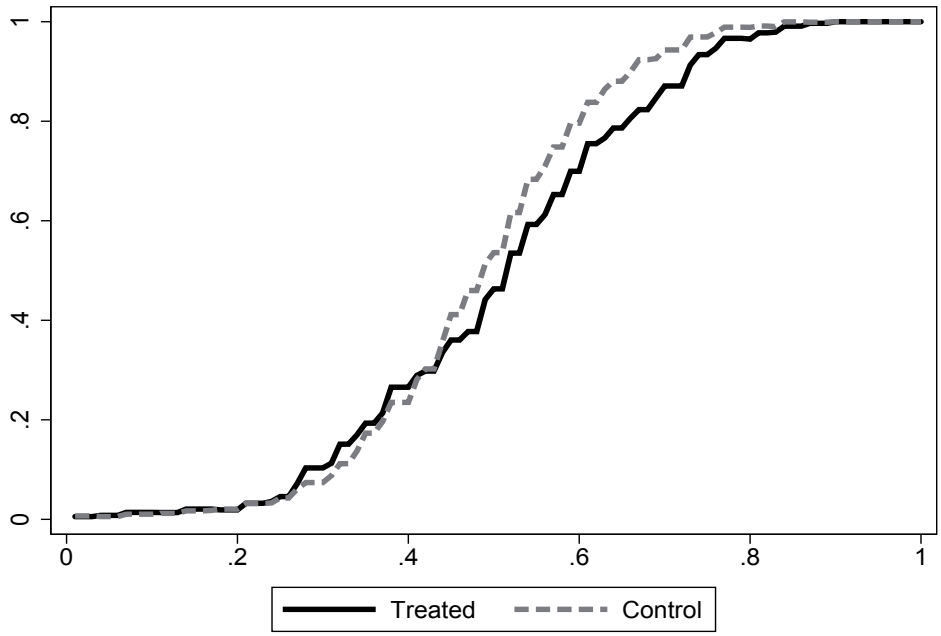
Notes: In panel A (B), treated students are 9^{th} graders in Madrid receiving the course between January and March (April and June). Controls are all 10^{th} graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.1). Estimates are means, unadjusted by covariates or strata dummies. Choices inconsistent with the law of demand are included - see text. Table A.5 shows adjusted estimates.

Figure 4: CDF of the raw scores in the March test by strata

Public schools



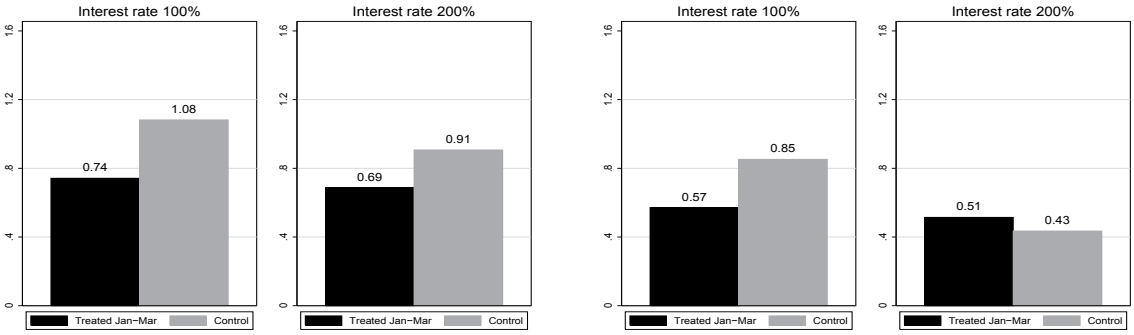
Non-public schools



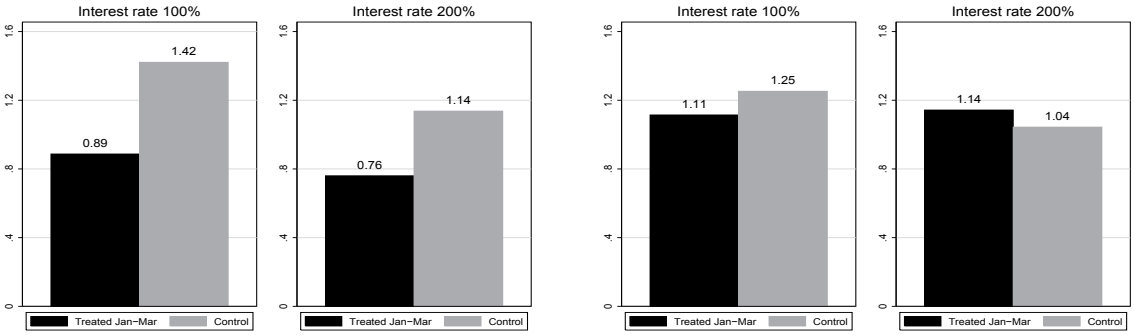
Notes: The horizontal axis shows the fraction of correct answers, while the vertical axis contains the fraction of students. Each point is the predicted proportion of students with correct answers that are equal or below the value in the horizontal axis. Predictions are obtained from OLS regressions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

Figure 5: Euros allocated to sooner payment in the incentivized saving task by strata

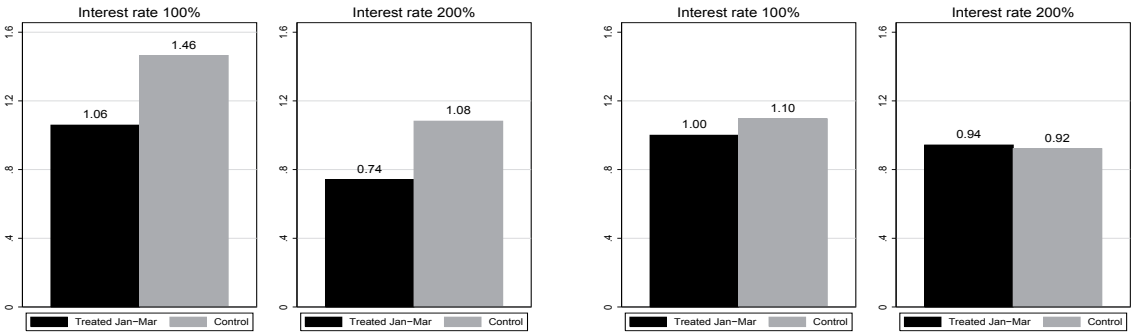
Panel A. Today *vs.* One week



Panel B. Today *vs.* Two weeks



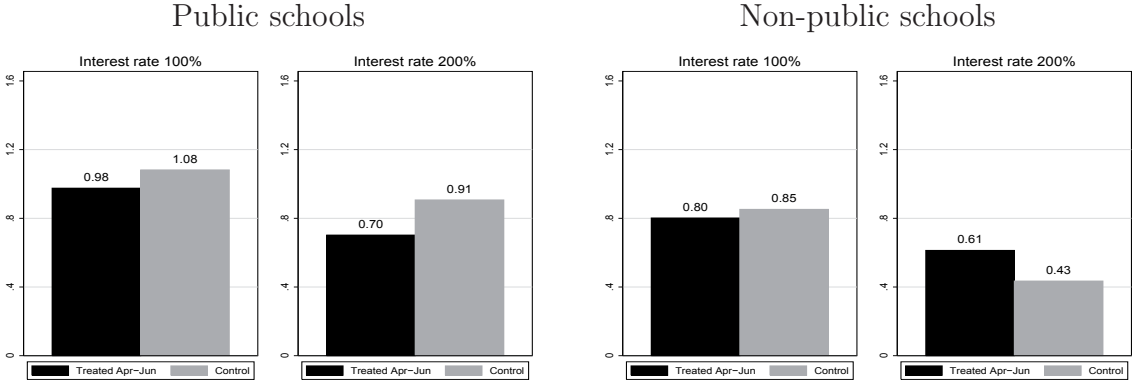
Panel C. One week *vs.* Two weeks



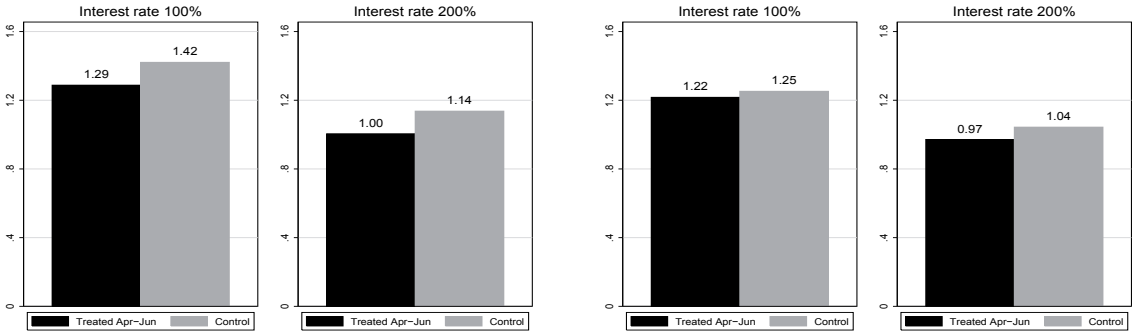
Notes: Treated students are 9^{th} graders in Madrid receiving the course between January and March. Controls are all 10^{th} graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.1). Estimates are means, unadjusted by covariates or strata dummies. Choices inconsistent with the law of demand are included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

Figure 6: Euros allocated to sooner payment in the incentivized saving task by strata

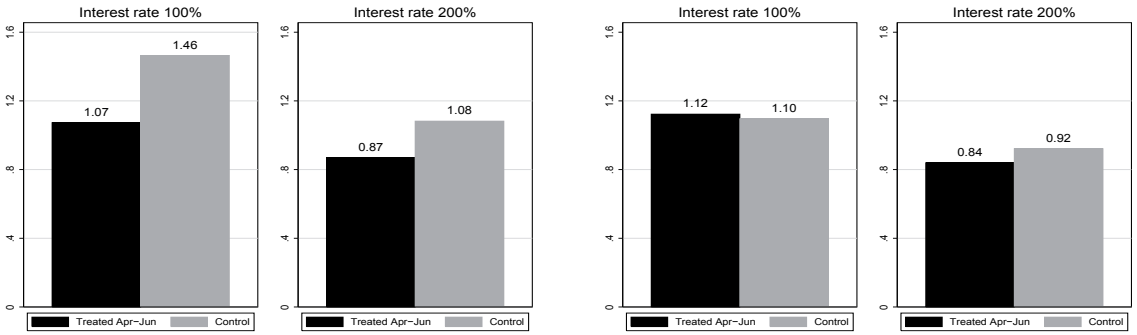
Panel A. Today *vs.* One week



Panel B. Today *vs.* Two weeks



Panel C. One week *vs.* Two weeks



Notes: Treated students are 9^{th} graders in Madrid receiving the course between April and June. Controls are all 10^{th} graders in Madrid (strata 1, 2, 3, 7 and 8 in Table A.1). Estimates are means, unadjusted by covariates or strata dummies. Choices inconsistent with the law of demand are included - see text. Table A.5 shows adjusted estimates.

A Appendix

Table A.1: Description of the strata

		Originally contacted		Accept participating	
		Total	Treated	Total	Treated
Applying before August 2014:					
Stratum 1:	Public schools, Madrid	15	8	9	5
Stratum 2:	Concerted schools, Madrid	10	6	4	2
Stratum 3:	Private schools, Madrid	5	2	2	0
Stratum 4:	Public schools, rest	18	9	6	2
				[12]	[8]
Stratum 5:	Concerted schools, rest	9	4	6	2
Stratum 6:	Private schools, rest	4	2	3	1
Applying September 2014:					
Stratum 7:	Public schools, Madrid	9	4	3	1
				[4]	[2]
Stratum 8:	Private schools, Madrid	2	1	2	1
Stratum 9:	Public schools, rest	30	15	14	5
				[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	6	3
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	78	35
Percentage participants (%)				46.15	42.17

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

Table A.2: Number of students and sample selection criteria

Panel A: 9th grade participants in the December 2014 test	
1. Total number of students registered in the school in December 2014	3,344
2. Did not attend the test	289
3. Left the test early	10
4. Incomplete, problems during application	108
5. Fully completed the test (5=1-2-3-4)	2,937
6. Sample used in balancing tests in Table 3 (6=3+4+5)	3,055

Panel B: 9th grade participants in the December 2014 and March 2015 tests

		March test						
		1. Left school	2. Did not attend the test	3. Left early	4. Incomplete	5. Completed	6. Refused	Total
December test	1. Not in school in December (new)	0	0	0	0	23	0	23
	2. Did not attend the test	3	70	0	6	204	6	289
	3. Left the test early	0	2	0	0	8	0	10
	4. Incomplete	1	12	0	1	94	0	108
	5. Fully completed the test	6	182	1	37	2,696	15	2,937
Total		10	266	1	44	3,025	21	3,367

Sample in March ((total; columns 3+4+5)-(new students)): 3,047
Balanced sample in March (row 5; columns 3+4+5): 2,734

Notes: We use test scores of students who left early or whose tests were incomplete because they answered the test (but not the survey). 3,047 is the number of 9th graders without any special need who (i) fully completed the March test or (ii) completed partially -either because they left early or problems during application. Of those, 2,734 students also completed the test both in December 2014.

Table A.3: The effect of the financial literacy program on the March tests scores by topic

	Saving	Means of payment	Banking relationships	Sustainable consumption
	(1)	(2)	(3)	(4)
1. Treated	-.000	.019	.031	.025
(S.E.)	(.014)	(.012)	(.010)	(.018)
[p-value]	[.984]	[.110]	**[.003]**	[.179]
2. Score in the pre-test	.725	.549	.417	.800
	(.033)	(.030)	(.018)	(.047)
3. Constant	.525	.644	.300	.930
	(.017)	(.016)	(.011)	(.026)
Fraction correct in pre-test	.55	.60	.47	.28
R^2	.183	.152	.237	.139
Number of students (schools)			2,734 (77)	

Notes: The dependent variable is the fraction of correct answers in the March test, and it is not normalized by its standard deviation. The estimation method is OLS, and all models control for stratification dummies (stratum 1 excluded). The standard errors (S.E) are corrected for heteroscedasticity and arbitrary correlation at the school level (77 schools).

Table A.4: Choice in Convex Time Budget Task

SHEET 1: WHAT DO YOU PREFER?				
		Get __ euros TODAY...	And __ euros 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 __ euros IN ONE WEEK...	And __ euros 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 __ euros IN ONE WEEK...	And __ euros 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.5: The effect of the financial literacy program on early consumption (incentivized saving task)

Panel A: Impact on early consumption, early treatment students							
	Treated students January-March (9^{th} graders) vs control students (10^{th} graders)						
	Today vs 1 week		Today vs 2 weeks		1 week vs 2 weeks		Earlier choice
	Interest rate=100%	Interest rate=200%	Interest rate=100%	Interest rate=200%	Interest rate=100%	Interest rate=200%	Pooling all choices
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1. Treated	-.312	-.122	-.405	-.228	-.292	-.251	-.270
(S.E.)	(.171)	(.162)	(.166)	(.163)	(.155)	(.182)	(.150)
[p-value]	**[.075]**	[.450]	**[.022]**	[.172]	**[.072]**	[.179]	**[.085]**
2. Constant	.965	.816	1.210	.994	1.270	1.028	
	(.162)	(.140)	(.174)	(.147)	(.140)	(.210)	
Number of students (schools)	589 (19)						3,543 (19)
Panel B: Impact on early consumption, late treatment students							
	Treated students April-June (9^{th} graders) vs control students (10^{th} graders)						
	Today vs 1 week		Today vs 2 weeks		1 week vs 2 weeks		Earlier choice
	Interest rate=100%	Interest rate=200%	Interest rate=100%	Interest rate=200%	Interest rate=100%	Interest rate=200%	Pooling all choices
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1. Treated	-.102	-.048	-.097	-.112	-.235	-.144	-.120
(S.E.)	(.139)	(.138)	(.130)	(.105)	(.143)	(.133)	(.110)
[p-value]	[.470]	[.730]	[.520]	[.290]	[.112]	[.285]	
2. Constant	.880	.800	1.187	.956	1.160	.937	
	(.152)	(.140)	(.140)	(.130)	(.142)	(.189)	
Number of students (schools)	715 (20)						4,290 (20)

Notes: All models control for strata fixed-effects and for the lagged values of hypothetical choice in December 2014. When we pool all choices in Column 7 we also include separate intercept for each of the six choices. Standard errors (S.E) allow for arbitrary correlation at the school-class level.

B Web Appendix

Table W1: Diffs-in-diffs estimates of 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 (9^{th} graders). March				
Treated*After	.158	.158	.157	.157
(S.E)	(.063)	(.062)	(.059)	(.059)
[p-value]	**[.014]**	**[.012]**	**[.009]**	**[.009]**
Fraction correct in pre-test	.55	.55	.47	.47
R^2	.002	.049	.002	.050
Number of students (schools)	5,907 (77)		5,468 (77)	
Panel B: Non-treated students in treated schools vs those in control schools (10^{th} graders). March				
"Treated"*After	-.051	-.056	-.108	-.108
(S.E)	(.084)	(.084)	(.077)	(.078)
[p-value]	[.545]	[.508]	[.167]	[.220]
Fraction correct in pre-test	.63	.63		
R^2	.002	.042	.004	.043
Number of students (schools)	2,966 (77)		2,732 (77)	

*Notes: the sample pools the sample of students in the December 2014 and March 2015 tests. The dependent variable is the normalized score in each test (the March score in the March sample and the pre-test in the December sample). Models 2 and 3 include strata dummies. *Model 4 merges two strata where no school assigned to treatment accepted to participate. Estimation method: Differences-in-Differences. Covariates include the variable* After *(an indicator variable taking value 1 for the March sample) and the variable* Treated *(a dummy taking value 1 for students in treated schools). The standard errors (S.E) are corrected for heteroscedasticity and arbitrary correlation at the school level.*

Table W2: Diffs-in-diffs estimates of the effect of the financial literacy program on several outcomes

Panel A: Responses of home support for saving, hypothetical saving choices, and labor supply among 9^{th} graders. March						
	Talks to parents about economics (From 1 to 5; 1: never, 5: every day)		Hypothetical saving choices (0: later choice, 1: earlier choice)		Money for tasks at home (0: does not, 1: does)	
	Diffs-in-diffs	+ fixed effects	Diffs-in-diffs	+ fixed effects	Diffs-in-diffs	+ fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*After	.104	.111	-.031	-.032	.041	.041
(S.E)	(.043)	(.061)	(.017)	(.019)	(.020)	(.029)
[p-value]	**[.018]**	**[.074]**	**[.076]**	**[.095]**	**[.046]**	[.154]
R^2	.050	.748	.201	.426	.003	.714
Number of students (schools)	5,468 (77)		16,157 (77)		5,468 (77)	
Panel B: Responses of financial inclusion (holding a bank account, money card) and saving behavior among 9^{th} graders. March						
	Holding a bank account (0: no, 1: yes)		Holding a money card (0: no, 1: yes)		Saves (From 2 to 5; 2: no, 5: same amount each week)	
	Diffs-in-diffs	+ fixed effects	Diffs-in-diffs	+ fixed effects	Diffs-in-diffs	+ fixed effects
	(1)	(2)	(3)	(4)	(5)	(6)
Treated*After	.031	.031	-.011	-.011	-.026	-.040
(S.E)	(.023)	(.033)	(.016)	(.023)	(.041)	(.053)
[p-value]	[.181]	[.341]	[.502]	[.634]	[.498]	[.444]
R^2	.023	.783	.005	.629	.028	.481
Number of students (schools)	5,468 (77)		5,468 (77)		4,983 (77)	

*Notes: the sample pools the balanced sample of students in the December 2014 and March 2015 tests. The dependent variable is the outcome in each survey (the March answer in the March sample and the December answer in the December sample). Models 2 and 3 include strata dummies. *Model 4 merges two strata where no school assigned to treatment accepted to participate. Estimation method: Differences-in-Differences (odd-numbered columns) and Differences-in-Differences with a student specific fixed effect (even-numbered columns). Covariates include the variable* After *(an indicator variable taking value 1 for the March sample) and the variable* Treated *(a dummy taking value 1 for students in treated schools). The standard errors (S.E) are corrected for heteroscedasticity and arbitrary correlation at the school level. In Panel A, Columns (3) and (4), the number of cases is 16,157 stacked student-choice-surveys (=2,734 students*2 surveys*3 choices minus 19 cases of non response). The choice between 100 euros today vs. 120 in six weeks was not included in the December survey and hence is not included for the Diffs-in-Diffs specification.*

Table W3: The effect of the treatment on selected outcomes, full specification

	Tests scores (correct answers)	Talks to parents about economics	Earlier choice in hypothetical choices	Holds bank account	Money in exchange tasks at home	Incentivized saving task
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	.167	.121	-.027	.015	.051	-.269
	(.064)	(.054)	(.012)	(.019)	(.022)	(.150)
Stratum 1: Public schools, Madrid,						
1st round of applications [omitted]						
Stratum 2: Concerted schools, Madrid	.074	-0.172	.032	-.079	.018	.049
	(.140)	(.128)	(.021)	(.051)	(.045)	(.179)
Stratum 3: Private schools, Madrid (*)	.413	-.150	-.037	.080	-.001	-.249
	(.130)	(.065)	(.022)	(.039)	(.031)	(.141)
Stratum 4: Public schools, rest,	-.075	.027	-.004	.027	.033	
1st round of applications	(.146)	(.103)	(.019)	(.034)	(.040)	
Stratum 5: Concerted schools, rest,	.071	.013	-.004	-.005	.039	
1st round of applications	(.107)	(.104)	(.027)	(.044)	(.039)	
Stratum 6: Private schools, rest	-.242	-.071	.004	.063	-.001	
	(.110)	(.109)	(.015)	(.051)	(.052)	
Stratum 7: Public schools, Madrid,	.064	.095	.013	.053	.035	-.083
2nd round of applications	(.079)	(.175)	(.044)	(.034)	(.053)	(.159)
Stratum 9: Public schools, rest,	.016	-.020	.008	.071	.025	
2nd round of applications	(.106)	(.084)	(.024)	(.030)	(.043)	
Stratum 10: Concerted schools, rest,	.246	-.003	.025	.070	-.013	
2nd round of applications	(.092)	(.067)	(.026)	(.037)	(.036)	
Stratum 11: Public schools,	-.141	-.118	-.009	.036	.030	
3rd round of applications	(.112)	(.175)	(.022)	(.067)	(.054)	
Stratum 12: Concerted schools,	.159	-.127	-.040	.011	.010	
3rd round of applications	(.105)	(.057)	(.023)	(.045)	(.050)	
Stratum 13: Public schools,	.044	.182	.021	.151	.028	
3rd round of applications	(.089)	(.116)	(.039)	(.032)	(.028)	
Stratum 14: Intended to give the	.042	.092	.054	.054	-0.044	
material in 7th/8th grade (**)	(.086)	(.173)	(.023)	(.028)	(.043)	
Stratum 16: Intended to give the material	-.065	.025	-.012	-.033	-.009	
in 1st year of upper secondary (all public)	(.113)	(.066)	(.021)	(.043)	(.032)	
Choice btw 100€ and 120 in 3 weeks			-.156			
			(.010)			
Choice btw 100€ and 150 in 3 weeks			-.217			
			(.010)			
Choice btw 100€ and 180 in 3 weeks			.364			
			(.010)			
Immediate payment						-.278
						(.041)
Interest rate						-.205
						(.040)
Delay						.297
						(.054)
Outcomes measured at the baseline (December 2014)	.567				.364	
	(.020)				(.017)	
Baseline choice btw 100€ and 120 in 3 weeks			-.201			-.307
			(.012)			(.156)
Baseline choice btw 100€ and 150 in 3 weeks			-.097			.037
			(.019)			(.275)
Baseline choice btw 100€ and 180 in 3 weeks			-.027			-.259
			(.030)			(.325)
Baseline talks to parents: every day		1.187				
		(.063)				
Baseline talks to parents: once a week		.502				
		(.047)				
Baseline talks to parents: less than once a week		-.664				
		(.059)				
Baseline talks to parents: never		-.289				
		(.169)				
Baseline holds bank account				.567		
				(.018)		
Baseline holds money card				.075		
				(.026)		
Constant	-.121		.541	.201	.144	1.144
	(.083)		(.033)	(.026)	(.030)	(.178)

Notes: (*) two strata of private schools in Madrid were pooled because no treated schools accepted to participate in one of those two rounds. (**) Strata of schools applying in November 2014 to give the course in 7th and 8th grades were pooled because no treated school (intended to teach in 8th grade) accepted to participate on time and no control school (intended to teach in 7th grade) accepted to participate on time.

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