Financial Education and Intertemporal Choice in Adolescence*

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Abstract

We study the impact of an established financial education program on intertemporal choice in adolescence. The program was randomly assigned to high-school students and intertemporal choices were elicited using an incentivized experiment. Students who participated in the program display lower aggregate present bias, and an increase in corner choices. Students' choices also display an increase in consistency with the law of demand. These results suggest that financial education affects how students view intertemporal choices. Increased consistency is in line with increased understanding, while increased time consistency and corner choices are suggestive of less narrow bracketing when choosing among time-dated payments.

JEL codes: D14, D91, C93.

Keywords: Intertemporal Choice, Financial Education, Experiment.

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1 Introduction

Intertemporal choices are ubiquitous and central to economic decision-making. Important examples include investment in education, mortgage borrowing and saving for retirement. Recent research suggests that intertemporal choices are important already at a young age. For example, intertemporal choices of children and adolescents, elicited with experimental tasks, predict contemporaneous and future financial outcomes, such as savings and wealth accumulation (Sutter et al., 2013, Moffitt et al., 2011). This research suggests that more impatient children and adolescents may face a worse economic outlook, and raises the question whether policy interventions at an early age, in particular, educational interventions can shape intertemporal choice.

In this paper we exploit the fact that financial education is increasingly offered at a young age, and deals extensively with spending and saving behavior, to study the effect of a randomly-assigned financial education program on intertemporal choice. We randomly assign high-school students to participate in an established financial education program. We use an incentivized tool, a version of the Convex Time Budget task (Andreoni and Sprenger, 2012), is used to measure intertemporal choices.

The program has two main effects on intertemporal choice. Choices made by treated students display less aggregate present bias, and the consistency of choices with the law of demand increases. The increase in quality of decision-making, as measured by consistency with the law of demand, is in line with the program leading to an increased understanding of intertemporal trade-offs. This is an important finding, in light of recent results suggesting that the quality of decision making, measured by consistency with utility maximization, is strongly positively correlated with wealth accumulation (Choi et al., 2014).[1]

[1] Choi et al. (2014) use a revealed preference method based on GARP and measure consistency using Afriat's (1972) Critical Cost Efficiency Index (CCEI).

The increase in time consistency displayed by treated subjects should be interpreted with care (Frederick et al., 2002, Sprenger, 2015). Changes in intertemporal choice could be explained by changes in external consumption opportunities (e.g., Carvalho, Meier and Wang, 2014, Dean and Sautmann, 2014) or changes in how the individual views experimental payments relative to outside lending and borrowing opportunities. We examine further aspects of choice and additional data to understand whether our findings display a pattern that is consistent with these two explanations. We do not observe a significant change in allowance money, spending and saving in response to participation in the program, suggesting that there was no change in external consumption opportunities that can explain the observed changes in intertemporal choices. Two other dimensions of choice change as a consequence of the program. First, the number of corner choices increases in the treatment group. Second, the estimated present bias parameter in the control group is significantly correlated with savings amounts, but this correlation decreases with the treatment. These findings suggests that the observed changes in intertemporal choice are consistent with a decrease in narrow bracketing when choosing among time-dated payments, e.g. integration of allowance money, among students who participated in the program.

This paper contributes to two main literatures. First, it contributes to a growing literature that measures intertemporal choices during adolescence and has demonstrated their correlation to a variety of field behaviors, ranging from saving to disciplinary referrals (e.g., Castillo et al., 2011, Sutter et al., 2013). We test the hypothesis that education may change intertemporal decision-making in adolescence (Becker and Mulligan,1997). Our results suggest that a short financial education intervention may change how students *view* experimental payments, both through an increase in their understanding of intertemporal trade-offs as well as their broader bracketing of payments offered to them.[2]

[2]In the short term, we do not find evidence that the program affects self-reported savings amounts

A second contribution of our paper is to show that education may increase the consistency of choices with the law of demand. We develop a structural model that allows for stochastic choices when estimating time preference parameters. Our descriptive results indicate both an increase in consistency with the law of demand as well as decrease in present bias. To estimate the time preference parameters implied by choices, we develop and estimate a structural model that takes into account the presence of inconsistency in intertemporal choice. This extends previous structural estimation approaches, developed in Andreoni and Sprenger (2012) and Andreoni et al. (2013), to allow for stochastic choices, including trembling-hand errors (Harless and Camerer, 1994) and Fechner errors (von Gaudecker, van Soest and Wengström, 2011).

A line of research to which our findings are also related is concerned with arbitrage in time-dated experimental payments. Only the early study by Coller and Williams (1999) examines the role of arbitrage opportunities, by providing information about market interest rates. Financial education programs generally discuss different sources of money available to students and may thereby broaden the consideration of alternatives when they are offered time-dated payments.[3]

Additionally, our paper is related to studies that examine the impact of financial education programs. Financial education has become increasingly common in recent years, reaching in 2013 an estimated 670 million US dollars spent annually in such programs (CFPB, 2013; Lusardi and Mitchell, 2014). However, the effects of financial education programs offered to adults are often found to be weak or inexistent (see Lynch, Fernandes and Netemeyer, 2014, for a review). The evidence regarding the

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and other related financial behaviors. This suggests that deep changes in time preferences may require lengthier and more intense interventions. Evidence in line with this conjecture is provided by Bruhn et al. (2014), who examine a two-year long financial education program in Brazilian high schools.

[3]Recent studies using the Convex Time Budget task have also found that intertemporal choices exhibit an increase in corner solutions in response to willpower manipulations (Kuhn, Kuhn and Villeval, 2013) and after the introduction of savings accounts (Carvalho, Prina and Sydnor, 2014), in line with our findings.

impact of programs provided in adolescence is also mixed (e.g., Bechetti, Caiazza and Coviello, 2013; Lührmann, Serra-Garcia and Winter, 2015). The present study uses a different approach compared to previous survey-based studies, by measuring the impact of financial education on incentivized intertemporal choices. Our findings suggest that financial education improves the quality of decision-making, measured as consistency with the law of demand. Despite its importance, quality of decision-making has not been previously explored in studies on the impact financial education.

The remainder of the paper proceeds as follows. In the next section, we describe the financial education program. In Section 3, we describe of the experimental task and methods used. Section 4 reports the descriptive results. In Section 5, we present the structural model, the estimation approach, and the estimates of preference parameters and error rates. Section 6 provides a discussion of the findings. Section 7 concludes.

2 The Financial Education Program

The financial education program is provided by a non-profit organization, My Finance Coach, which since its startup in October 2010 has offered financial education to over 35,000 German high school students, aged mainly between 13 and 15 years (My Finance Coach, 2012). We evaluate the impact of financial education offered through visits of "finance coaches" to schools. These coaches are employees of the (for-profit) firms that sponsor the (non-profit) provider, and they are not compensated for the training they provide to high-school students. They volunteer to conduct several visits of 90 minutes, for a total of 4.5 hours, each of which is dedicated to one of the training modules. The provider offers a set of materials for each module and trains the coaches; hence, visits are standardized.

This financial education program is well suited for studying the impact of educational interventions among adolescents. First, it is provided at schools, and hence all students in a class participate, avoiding selection problems (see, e.g., Meier and Sprenger, 2013). Second, the materials taught are standardized, have been developed by educational experts (ranging from education researchers to school directors), and have been extensively used in teaching for over four years in Germany. Third, this educational intervention is scalable.

We measure the joint impact of three training modules that are provided to all treated students: Shopping, Planning, and Saving. Each module deals with the following topics as described in the official materials of the provider. Detailed information of each module is provided in Table 1.[4] The Shopping module deals with acting as an informed consumer. It focuses on prioritizing spending ("needs and wants"), discusses criteria used in purchasing decisions and advertising. The Planning module addresses aspects of conscious planning. It presents the concepts of income and expenditure as the basis of financial planning, and trains budgeting skills. The last module, Saving, discusses different saving motives and various types of investment options. The training does not take a normative position on saving, but discusses *how* to save. Importantly, the training also does not involve any decision that directly resembles the tradeoffs in the Convex Time Budget task. In sum, the three training modules deal at length with intertemporal choices in the domain of financial decisions.

3 Experimental Design

3.1 Setting and Randomization

The schools in our study pertain to the two lower tracks of the German high school system. Students in these two tracks typically continue with vocational training after

[4]Further detailed information about the training materials can be found at `http://en.myfinancecoach.org/`.

Table 1: Summary of the Financial Education Program

Module	Topic	Activity
Shopping	Introduction	Brainstorming: words associated with "shopping"
	Discussion of shopping criteria	(a) Discussion: what did students buy last? Was it something they "needed" or "wanted"?
		(b) Comic strip: an adolescent receives money from his mother and spends it on unplanned expenses (chips and chocolate).
	Advertising	(a) Discussion: where do you see ads? Which instruments are used in advertising (emotions, logos, etc.)?
		(b) Typical messages in ads
	Buying a smartphone	(a) Discussion: what shopping criteria do you use?
		(b) Roleplay: adolescent wants to buy a smartphone, discussion with parents and friends.
Tips for students		*(1) Prioritize when making spending choices*
		(2) Be critical about advertising
		(3) Think about which criteria are important for you before buying
		(4) Compare different options before buying
Planning	Introduction	Brainstorming: words associated with "planning"
	Different kinds of plans	Exercise: linking different types of plans (e.g. school schedule) to their purposes
	Financial planning	(a) Discussion: why plan your expenses and income?
		(b) Discussion: where does your money come from and what do you spend it on?
		(c) Case study: Felix wants to buy a motorcycle; help him planning expenses, and discuss why Felix should not take on debt
Tips for students		*(1) Just as with other plans, you can plan your finances*
		(2) Have an overview of your income and expenses
		(3) A plan can help you reach your goals
		(4) Do not spend more money than you have
		(5) Purchases of durables can have running costs
Saving	Introduction	Brainstorming: words associated with "saving"
	Saving money	(a) Discussion: what do you do with money?
		(b) Discussion: how can you save money?
		(c) Discussion: why there are different savings products
		(d) Comic strip: savings choice by an adolescent
	Risk, return, liquidity	(a) Discussion: trade-off between risk, return and liquidity.
		(b) Case study: Paul (14 years old) receives money for his driving license (to be spent at 18), help him choose how to save it
	Definition of savings products	Find the product that matches the definition
Tips for students		*(1) Decide which is more important for you: return, risk or liquidity*
		(2) Do not choose the first offer made to you
		(3) Do not believe that one savings product can achieve everything (high return, low risk and high liquidity)
		(4) Decide which savings product fits best your objective

graduation (rather than attending college).[5] Dustmann (2004) shows that there is a

[5]The school system in Germany has three types of high schools, starting as of age 10. These tracks comprise schools in which students pursue vocational training (*Hauptschule*, *Sekundarschule*,

strong association between family background (parents' education as well as occupational status) and childrens' school track.

The randomization of classes to the control and treatment group was implemented through a web interface designed by the research team. Schools in the treatment group were assigned to receive the training earlier in the school year, while schools in the control group were assigned to receive the program towards the end of the school year. Randomization occurred at the school level to avoid spillover effects. Randomization was stratified by city, across the cities of Berlin, Düsseldorf and Munich in Germany, such that differences in the educational systems in the different areas are orthogonal to the treatment allocation. Since we were bound by scheduling constraints, including that all participating schools receive the training by the end of the school year, the time between treatment and time preference elicitation was between 4 and 10 weeks. We thus measure short- to medium-run effects of the program.

3.2 Method

The elicitation method used is the Convex Time Budget (CTB), developed by Andreoni and Sprenger (2012). This method asks individuals to allocate amounts of money to two points in time. The payment received at the earlier point in time, t, is x_t; the amount received at a later point in time, $t+k$, is x_{t+k}. The delay between payments is k. The amounts x_t and x_{t+k} satisfy the budget constraint $(1+r)x_t + x_{t+k} = m$, where $1+r$ is the gross interest rate. The CTB method allows for inner choices in addition to corner solutions.

We elicit choices using three different combinations of t and $t+k$; the tasks for each of these combinations are presented on a separate decision sheet. The first sheet offers

Mittelschule), combine both vocational training with the option of accessing university later on (*Realschule*, *Gesamtschule*, *Werkrealschule*) or focus on preparation for university studies (*Gymnasium*). All participating students in our study belong to the first two types of schools.

payments immediately after the CTB ($t = 0$, "today") and three weeks later, i.e., the delay is $k = 3$ weeks. The second sheet offers payments today and six weeks later, i.e., the delay is $k = 6$ weeks. The last sheet offers payments in three and in six weeks, i.e., the delay between payments is $k = 3$ weeks but there is also a "front-end delay" as $t > 0$. On each decision sheet, seven budget constraints – i.e., seven different interest rates – were presented to students. Going from top to bottom, the price for the earlier payment increases. An overview of the design is displayed in Table 2.[6]

Table 2: Elicitation of time preferences – Design

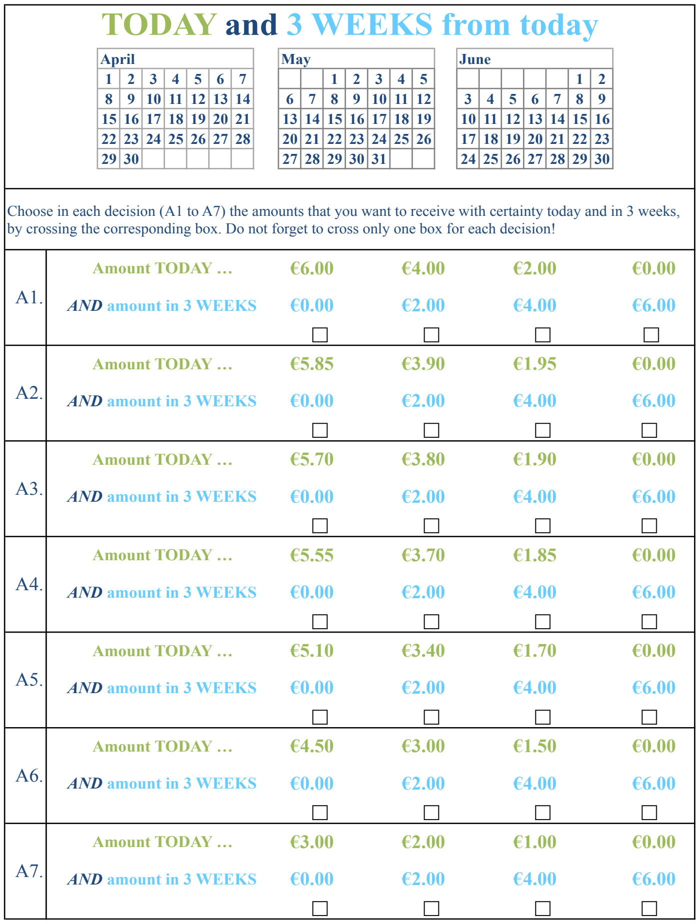
Decision sheet	Earlier payment (t)	Later payment	Delay (k)
(1)	Today	In 3 weeks	3 weeks
(2)	Today	In 6 weeks	6 weeks
(3)	In 3 weeks	In 6 weeks	3 weeks

Note: Within each decision sheet seven decisions were elicited with the following gross interest rates $(1 + r)$: 1.00, 1.025, 1.05, 1.08, 1.18, 1.33 and 2.00, on the budget constraint $(1 + r)x_t + x_{t+k} = m$.

We adapt the elicitation task to ensure that adolescents understands it. Andreoni and Sprenger (2012) offer a choice set with 100 choices within each budget. In a follow-up study, Andreoni, Kuhn and Sprenger (2013) limit the choice set to seven choices. Both studies were conducted among university students. To reduce complexity in our adolescent sample, we offer four combinations of sooner and later payments. In each choice situation, participants can either allocate 100%, 66.6%, 33.3% or 0% of the budget to the sooner point in time. To make the variation in the time horizons salient, color-coding was used for each point in time. Additionally, students saw a calendar at the top of each sheet on which the relevant payment dates were marked in the corresponding color. An example of a decision sheet is provided in Figure 1. We randomized the ordering of the three decision sheets across classes to balance any

[6]For example, for a delay of three weeks, the effective yearly interest rate, assuming quarterly compounding, ranges from 0%, for gross rate 1.00, to 752.9%, for gross rate 1.18, and goes up to 27128%, for gross rate 2.00.

potential order effects.



TODAY and 3 WEEKS from today

April						
1	2	3	4	5	6	7
8	9	10	11	12	13	14
15	16	17	18	19	20	21
22	23	24	25	26	27	28
29	30					

May						
		1	2	3	4	5
6	7	8	9	10	11	12
13	14	15	16	17	18	19
20	21	22	23	24	25	26
27	28	29	30	31		

June						
					1	2
3	4	5	6	7	8	9
10	11	12	13	14	15	16
17	18	19	20	21	22	23
24	25	26	27	28	29	30

Choose in each decision (A1 to A7) the amounts that you want to receive with certainty today and in 3 weeks, by crossing the corresponding box. Do not forget to cross only one box for each decision!

A1.	Amount TODAY …	€6.00	€4.00	€2.00	€0.00
	AND amount in 3 WEEKS	€0.00	€2.00	€4.00	€6.00
		☐	☐	☐	☐
A2.	Amount TODAY …	€5.85	€3.90	€1.95	€0.00
	AND amount in 3 WEEKS	€0.00	€2.00	€4.00	€6.00
		☐	☐	☐	☐
A3.	Amount TODAY …	€5.70	€3.80	€1.90	€0.00
	AND amount in 3 WEEKS	€0.00	€2.00	€4.00	€6.00
		☐	☐	☐	☐
A4.	Amount TODAY …	€5.55	€3.70	€1.85	€0.00
	AND amount in 3 WEEKS	€0.00	€2.00	€4.00	€6.00
		☐	☐	☐	☐
A5.	Amount TODAY …	€5.10	€3.40	€1.70	€0.00
	AND amount in 3 WEEKS	€0.00	€2.00	€4.00	€6.00
		☐	☐	☐	☐
A6.	Amount TODAY …	€4.50	€3.00	€1.50	€0.00
	AND amount in 3 WEEKS	€0.00	€2.00	€4.00	€6.00
		☐	☐	☐	☐
A7.	Amount TODAY …	€3.00	€2.00	€1.00	€0.00
	AND amount in 3 WEEKS	€0.00	€2.00	€4.00	€6.00
		☐	☐	☐	☐

Figure 1: Example of a decision sheet (translated from German)

3.3 Implementation of Payments

We followed a number of procedures to ensure trust and to address issues of risk and transaction costs that typically arise when implementing delayed payments. All procedures were explained in the instructions before any decisions were taken by the adolescents.

Transaction costs. Students were given a "participation" fee of 2 Euro to thank them for their participation. They were informed that the participation fee would be split equally across both payment dates. Hence, independent of the exact choice of each student, he or she received always at least one Euro at each point in time.

Record of payments. After students made their 21 (7×3) choices, one decision was drawn for payment. The random draw was performed by one volunteer student for the entire class and this draw was noted on the classroom board. Subsequently, based on the student's choice and the decision drawn for payment, each student received a payment card that recorded her exact payments and payment dates. Hence, students did not have to remember when the future payment would occur and how much they would receive. The payment card also served as a written confirmation of each students' payment entitlement. The card format was designed to fit into students' wallets, and students were requested to keep it there. At the same time, each student wrote her name onto a payment list, which also contained the payments she had chosen to receive at each point in time. This list was given to the teacher in the presence of the class. Both act as records for delayed payments and the payments list ensured that payments can be made even when individual payment cards are lost.

Delivery of payments. Payments were made in cash, in class, to each student individually. Immediate payments were made after the survey complementing the CTB experiment was completed, if today was drawn for payment. Delayed payments were made exactly three or six weeks later in class at the dates noted on the payment cards. The exact appointment for the future payment was discussed with the teacher and then announced in class. Our instructions clearly explained that we would come back into class once (or twice, depending on the draw) at the date(s) indicated on the calendars on their decision sheets and on payment cards to make the delayed payments. The teachers, were present in class, when we made this commitment.[7]

[7] They were however kept uninformed about student choices, except for the one choice that was

Consent. Only students whose parents had consented to participate are included in the study. The consent forms provided to parents included the researchers' contact information, which the teacher also obtained. Almost all students (97%) provided a signed consent form to participate in the study.

3.4 Procedures

In each session, the time preference elicitation task was conducted first, followed by a survey. The instructions for the time preference elicitation task were read aloud in front of the class. A copy of the instructions can be found in Online Appendix A. All class visits were conducted by the same two experimenters. One of them always presented the instructions in each session, using multiple examples. Students were asked to complete four control questions before starting to provide their choices. These questions were designed to test the understanding of the task. Each student's answers were checked by the experimenters before she could start making her 21 choices.

The presentation of the instructions took on average 25 minutes, while students made their decisions in 5 to 10 minutes. When they finished with the task, students were asked to complete a survey. We asked students for their gender and age, their math grade as well as three questions regarding their background. We elicited their household composition (i.e., who they live with), the language they speak at home and the amount of books at home. These are standard questions in the PISA survey (Frey et al., 2009). They are considered important family inputs into a student's education (for a review, see Hanuschek and Woessmann, 2011). Our survey also included four of Raven's progressive matrices (Raven, 1989), selected to capture heterogeneity in cognitive skills, based on a previous study in Germany by Heller et al. (1998). The survey also included several questions on financial knowledge and financial behavior.

chosen for payment.

The impact of the training on standard financial literacy questions is similar to the findings in Lührmann, Serra-Garcia and Winter (2015).[8] We also surveyed students regarding their allowance, spending and savings behavior.

In total sessions lasted between 45 and 60 minutes. In each city, all sessions were scheduled to take place during the same week, for both treatment and control groups.[9]

3.5 Sample

Our sample consists of 994 students from 55 classes in 25 schools (12 treatment, 13 control). We conducted the CTB task using pen and paper. When encoding the answers electronically, we found that 80 students provided one or multiple answers that could not be attributed a clear value. We focus on students (914) with complete answers.[10] The average age is 14.3 years and 39.8% of the students are female. Regarding the student's family situation, we find that a substantial share, 46.4%, speak a language other than German at home. Also, 24% live with a single parent and 60.2% report having less than 25 books at home. Individual characteristics were balanced across treatment and control, as shown by the t-tests presented in Table 3, supporting the randomization method.[11]

[8] We observe an increase in knowledge about what stocks are, as measured by the question designed by van Rooij et al. (2011), which is a subject dealt with in the educational program. We do not find spillover effects on to questions about interest compounding, the time value of money and risk diversification (based on standard financial literacy questions, see, e.g., Lusardi and Mitchell, 2014), concepts not taught in the program. Detailed results are available from the authors.

[9] To avoid any time effects, we scheduled the experiment to take place in each city during the same week in April. This was possible for 46 out of 55 classes. For a small group of nine classes the class was scheduled to be at a practical training out of school for the week, and hence we conducted the experiment 3 weeks later in eight classes and 6 weeks later for one class. We control for any potential time effects by adding a month dummy for April (as 46 out of 55 were scheduled in April) in our regression analysis.

[10] Results remain qualitatively the same if all students are included.

[11] Overall, nonresponse is very low, below 2.4% of the sample. The difference in nonresponse across treatment and control is not significant for any variables, except for books at home (t-test, p-value=0.04). Our results are robust to the inclusion of a dummy for nonresponse to this question.

Since the unit of randomization was the school, we cluster standard errors at the school level (Moulton, 1986) throughout.

Table 3: Individual characteristics in treatment and control group

	Control	Treatment	Treatment vs. Control t-test (p-value)
Girl	42.0%	37.2%	0.12
Grade 8	50.6%	52.1%	0.92
Cognition score	0.756	0.718	0.67
Math grade (relative)	0.012	0.010	0.91
Migrant background	47.1%	45.7%	0.87
Single parent	23.4%	25.1%	0.67
$<$ 25 books at home	60.4%	60.1%	0.95

Note: This table presents the mean of the individual characteristics by treatment and control. The third column reports the p-value of a t-test that the coefficient of the treatment dummy is equal to zero in a linear regression on each individual characteristic, using robust standard errors. Girl takes value 1 for female students, and grade 8 takes value 1 for students in that grade 8, 0 if in grade 7. Cognition score is the number of correct answers in 4 of Raven's progressive matrices. Math grade is defined relative to the average math grade in the class. A positive value indicates that the student performs better than the class average. Migrant background and single parent are dummy variables that take value 1 when the student speaks another language other than german at home and lives with a single parent, respectively. $<$ 25 books at home is a dummy that takes value one if the subject indicated the number of books at home was either 0-10 or 11-25 (below median), and zero if she indicated 26-100, 101-200, more than 200 books at home (above median).

4 Descriptive Results

4.1 Intertemporal Choices

We examine three key dimensions of intertemporal choice: i) the average allocation to the sooner payment — a measure of impatience –, ii) the difference in the allocation to the sooner payment when the sooner payment is immediate – a hallmark of time inconsistency –, and iii) the difference in the allocation to the sooner payment when the delay is increased – delay sensitivity. First, we do not observe a significant impact of the educational program on the average allocation to the sooner payment, as shown in

Table 4.[12] The average allocation is 54.9% to the sooner payment in the control group, while it is 55.26% in the treatment group.

Table 4: Determinants of allocation to sooner payment

	(1)	(2)
	Allocation to sooner payment	
	Coefficient	Std. Error
Treatment	4.210	[4.380]
Immediate payment	5.854**	[2.415]
Delay is 6 w.	-2.783	[2.030]
Gross interest	-25.125***	[1.908]
Gross interest * Immediate	-2.761*	[1.486]
Gross interest * Delay is 6 w.	1.470	[1.306]
Treatment * Immediate	-2.921*	[1.652]
Treatment * Delay is 6 w.	3.921***	[1.165]
Treatment * Gross interest	-2.146	[4.026]
Female	-2.989	[3.009]
Grade 8	-3.316	[3.730]
Cognition score	-3.637***	[1.321]
Math grade	-3.734***	[1.086]
Migrant background	-0.878	[2.950]
Single parent	0.072	[2.484]
<25 books at home	4.642**	[2.056]
Constant	78.572***	[5.931]
Observations	17,724	
Nr of left-censored observations	4579	
Nr. of right-censored observations	3547	
Nr. of interval observations	9598	
Pseudo-loglikelihood	-23720	

Note: Interval regression results with robust standard errors clustered at the school level (25 clusters). The dependent variable is the share of money allocated to the sooner payment data, ranging from 0 to 100. Immediate payment is a dummy variable that takes the value 1 if the sooner payment occurred immediately after the students completed the task and survey. Delay time = 6 weeks is a dummy variable that takes the value 1 if the delay between the earlier and later payment was 6 weeks and not 3 weeks. Individual characteristics are defined as in Table 3. Month and location fixed effects are included in all regressions. Robust standard errors are shown. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Second, the treatment group displays less present bias in their allocation choices

[12] The estimates are obtained using interval regressions to account for the fact that students were offered four budget choices. Results are robust to using a simple OLS regression model.

than the control group. The extent of present bias is measured by comparing allocation choices when the sooner payment is immediate versus in the future. Controlling for interest rates and interaction effects, students in this group increase their allocation by 5.85 percentage points when the sooner payment is immediate (p=0.015), as shown in Table 4. The effect of immediacy is reduced by 2.92 percentage points in the treatment group (p=0.077). A similar result is obtained by comparing the proportion of present-biased choices. In the control group, on average, individuals make present-biased choices in 22.2% of the cases. In the treatment group, this percentage is 19.9% (Mann-Whitney test, p=0.0288).[13]

Third, we observe an increase in delay sensitivity among treated students. Models of intertemporal choice typically assume that individuals discount the future, i.e., ceteris paribus they prefer payments sooner. This implies that allocations to the sooner payment are expected to increase as the delay between sooner and later payment dates increases. We find no increase in allocations to the sooner payment as delay increases in the control group, as shown in Table 4. With the treatment, delay sensitivity increases significantly (p=0.001).

The allocations chosen by the students vary with student characteristics in a similar way as found in previous results in studies of adolescents' intertemporal choice. For example, in line with Castillo et al. (2011) and Sutter et al. (2013), we find that students with higher math grades and cognition scores display more patience in their choices.

[13] At the same time, the frequency of time consistent choices increases from 58.2% to 61.5% (Mann-Whitney test, p=0.0799). In addition, there is a small non-significant decrease, from 19.7% to 18.6%, in the percentage of choices in which the students allocate less money when payments are immediate (Mann-Whitney test, p=0.1758).

4.2 Consistency and Corner Solutions

In addition to the allocations chosen in the CTB task, we examine the consistency of choices with the law of demand, and the rate with which they choose a corner solution. Consistency is measured as in Gine et al. (2012), by checking whether the student chooses a weakly smaller allocation to the sooner payment as the interest rate increases. Such a choice is consistent with the law of demand.[14] On average, 80.8% of choices in the control and 82.9% in the treatment group are consistent with the law of demand. These rates are very similar to those found by Gine et al. (2012) in individual interviews with farmers in Malawi (81%) and by Carvalho, Meier and Wang (2014) in the American Life Panel (82% before payday and 84% after payday). The educational program has a positive effect on consistency, as shown in Table 5, columns (1-2). Consistent with the idea that inconsistencies may reflect indifference between allocations, we observe an increase in consistency as the interest rate offered increases.

Simultaneously, we examine whether the program has an effect on the rate at which students choose corner solutions. While around 70% of the choices in Andreoni and Sprenger (2012) were corner solutions, we find that interior solutions predominate in our sample, with an average of 55.8% interior choices in the control group and 52% in the treatment group on average. The rate at which treated students choose corner solutions increases by 7 to 8%, as shown in Table 5, columns (3-4).

To sum up, we find that the educational program decreases present bias and increases in delay sensitivity; it also increases consistency and corner choices. A central question is the interpretation of such effects. As highlighted by Dean and Sautmann (2014) and Carvalho et al. (2014), changes in intertemporal allocations could be due to changes in external consumption opportunities. The survey administered to students measured

[14] Precisely, within each of the three decision sheets, students made seven choices. A choice is consistent if the allocation to the sooner payment date decreases or stays unchanged as the interest rate increases. By definition, the first choice in each sheet is excluded. Thus, the fraction of consistent choices is the sum of consistent choices over 18.

Table 5: Consistent choices and corner choices

	(1)	(2)	(3)	(4)
	Consistent choice		Corner choice	
Treatment	0.053*	0.054*	0.068*	0.078***
	[0.030]	[0.028]	[0.040]	[0.029]
Immediate payment	0.004	0.012	0.034*	0.047**
	[0.023]	[0.020]	[0.019]	[0.020]
Delay = 6 weeks	0.008	0.006	-0.007	-0.023
	[0.025]	[0.024]	[0.027]	[0.026]
Gross interest	0.045***	0.052***	0.009	0.015
	[0.017]	[0.017]	[0.014]	[0.013]
Gross interest * Immediate	0.003	-0.004	-0.004	-0.016
	[0.016]	[0.015]	[0.014]	[0.016]
Gross interest * Delay is 6 w.	-0.004	-0.002	0.003	0.015
	[0.020]	[0.020]	[0.019]	[0.019]
Treatment * Immediate	-0.001	0.001	-0.013	-0.016
	[0.010]	[0.011]	[0.013]	[0.012]
Treatment * Delay is 6 w.	-0.014	-0.013*	0.010	0.011
	[0.009]	[0.007]	[0.013]	[0.013]
Treatment * Gross interest	-0.020	-0.022	-0.018	-0.026
	[0.019]	[0.020]	[0.017]	[0.016]
Add. Controls	No	Yes	No	Yes
Observations	16,452	15,192	19,194	17,724

Note: Probit regression, marginal effects shown, with robust standard errors clustered at the school level (25 clusters). Consistent choice takes value 1 if the choice is consistent with the law of demand, 0 otherwise. Corner choice takes value 1 if the choice was to allocate 0 or 100% of the budget to the sooner payment date. Add. Controls is Yes when individual characteristics, defined as in Table 3 (gender, grade, cognition score, relative math grade, migrant background, single parent and books at home), and month and location fixed effects are included. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

the monthly allowance of each student and the amount of spending in a typical month. We find no significant effects of the treatment on these two measures (t-test from a regression with a treatment dummy and robust standard errors, p-val=0.414 and 0.489, respectively).[15] Thus, we find no changes in the external consumption opportunities of students across the treatment and control group, which could give rise to the treatment

[15]The results reported in Table 4 and 5 are also robust to including allowance or spending as controls.

effects established in this section.

Since there is an increase in consistency in choices, which occurs simultaneously with the change in intertemporal allocations, it is possible that treatment effects on intertemporal choices are confounded by the treatment effect on consistency. To address this problem, we estimate the time preference parameters implied by the allocation choices, using a model that allows for inconsistency in choices in what follows.

5 Estimation of Time Preferences

5.1 Theoretical Framework and Empirical Model

Following Andreoni and Sprenger (2012), we assume a time separable CRRA utility function within the $\beta - \delta$ model of quasi–hyperbolic discounting (e.g., Laibson, 1997),

$$U(x_t, x_{t+k}) = x_t^\alpha + \beta^{I_{t=0}} \delta^k x_{t+k}^\alpha \tag{1}$$

where the individual receives monetary amounts x_t and x_{t+k} at time t and $t+k$, and $I_{t=0}$ is an indicator variable that takes value one if payments are immediate. Individuals maximize utility subject to the budget constraint, $(1+r)x_t + x_{t+k} = m$. This yields the standard Euler equation, which can be written in logs as:

$$ln(\frac{x_t}{x_{t+k}}) = \frac{ln(\beta)}{\alpha - 1} I_{t=0} + \frac{ln(\delta)}{\alpha - 1} k + \frac{1}{\alpha - 1} ln(1+r), \tag{2}$$

The Euler equation establishes the optimal log ratio of payoffs across t and $t+k$ in decision j, given the vector of preference parameters $\mu = (\frac{ln(\beta)}{\alpha-1}, \frac{ln(\delta)}{\alpha-1}, \frac{1}{\alpha-1})$ and the vector of decision characteristics $X = (I_{t=0}, k, 1+r)$, which are varied experimentally. An individual i is offered four possible log ratios $s_m = ln(\frac{x_{m,t}}{x_{m,t+k}})$ in each decision problem j, where $m \in \{1, \ldots, M\}$ and $M = 4$. Hence, we estimate an interval data model

(Wooldridge, 2001, p. 509). Since we observe no effect of the educational program on consumption, and to simplify the estimation, we set background consumption equal to zero.

We expand the model to allow choices to be stochastic.[16] We introduce Fechner errors, $\tau\varepsilon$, where ε is assumed to be i.i.d. across choices and individuals, and to follow a standard logistic distribution. This stochastic specification allows that errors may be made when evaluating the distance between the optimal ratio of consumption and the available ratio. A larger τ implies that this distance is given less weight and hence that errors are more likely (von Gaudecker, van Soest and Wengström, 2011). Second, we introduce a trembling-hand error (Harless and Camerer, 1994), which allows for a probability ω that a random choice is made in a given decision. This implies that the likelihood that s_m is chosen, if it is an interior choice, is

$$\begin{aligned} \mathcal{L}(s = s_m | X, \mu, \omega, \tau, s) &= P(s_m > x^* > s_{m+1}) = P(s_m > X'\mu + \tau\varepsilon > s_{m+1}) \\ &= (1-\omega)(\Lambda(\frac{1}{\tau}(s_m - X'\mu)) - \Lambda(\frac{1}{\tau}(s_{m+1} - X'\mu))) + \frac{\omega}{4}. \quad (3) \end{aligned}$$

The likelihood is adapted correspondingly if s_m is a corner choice. We estimate all parameters jointly and, in what follows, focus on the time preference parameters β and δ and the error term ω.[17]

As a robustness check, we estimate preference parameters using a different model of stochastic decision-making, based on Luce (1959), and adopted by Andersen et al. (2008). A drawback of this model is that it does not allow choices that are inconsistent with the law of demand (Loomes, Moffatt and Sugden, 2002). Hence, the first model with trembling-hand errors is more adequate in our context.

[16] Details regarding this econometric model and the Luce Model, described below, are provided in Online Appendix B.

[17] Because of the discrete nature of the data, the parameter α is only identified up to a constant and thus this estimate is unlikely to be accurate (see, also, Andreoni, Kuhn and Sprenger, 2013).

5.2 Aggregate Parameters

We begin by presenting estimates obtained from the treatment and control groups, assuming homogenous preference parameters within each sample. Columns (1) and (2) of Table 6 display estimated parameters following equation (3), for the control and treatment group, respectively.[18] The estimated β is 0.928 in the control group, which is significantly different from one (χ^2-test, p<0.01). In contrast, in the treatment group, $\hat{\beta}$ is 0.994, and not significantly different from one (χ^2-test, p=0.695). The estimated β increases in the treatment group (t-test, p=0.019). Consistent with our previous result, the treatment leads to a significant decrease in present bias on average. Columns (3) and (4) of Table 6 display qualitatively similar results using the Luce probabilistic choice model.

The estimated value of β in the control group, between 0.928 and 0.943, indicates moderate present bias. It is slightly larger than β for effort choices in Augenblick et al. (2013), which is between 0.877 and 0.900. By contrast, the estimated β in the treatment group is similar to that estimated for money in Augenblick et al. (2013), which is between 0.974 and 0.988.

The estimated daily discount factor is between 0.989 and 0.997, in line with previous studies (e.g., Augenblick et al., 2013). There is a small, statistically significant decrease in the discount factor in the treatment group (t-test, p=0.046). It is consistent with the increased delay sensitivity, found at the descriptive level, since the discount factor is identified through changes in delay sensitivity.

[18]We assume Fechner errors to be homogeneous within each group and allow trembling-hand errors to be school-specific. The trembling-hand error should be estimated at the individual level, such that it accounts for noise specific to the decisions of an individual. We follow this approach in the next subsection. In this specification we allow it to vary at the school level, where there is a substantial degree of variation. Results remain robust to estimating a single trembling-hand error.

Table 6: Estimated Aggregate Time Preference Parameters, by Control and Treatment

	(1)	(2)	(3)	(4)
Model:	Interval regression		Luce model	
Group:	Control	Treatment	Control	Treatment
$\hat{\beta}$	0.9280	0.9942	0.9434	0.9886
	[0.0218]	[0.0148]	[0.0201]	[0.0162]
$\hat{\delta}$	0.9966	0.9933	0.9910	0.9896
	[0.0010]	[0.0012]	[0.0015]	[0.0024]
$\hat{\alpha}^{\dagger}$	0.5714	0.4527	0.8212	0.8758
	[0.0189]	[0.0931]	[0.0317]	[0.0573]
$\hat{\tau}^{\dagger}$	0.4993	0.6121		
	[0.0460]	[0.0382]		
$\hat{\mu}$			0.0503	0.0587
			[0.0053]	[0.0057]
Observations	10,332	8,862	10,332	8,862
$H_0 : \hat{\beta} = 1$ (p-value)	0.0009	0.6946	0.0048	0.4829

Note: Columns (1) and (2) report the estimated preference parameters from the interval data model based on eq. (3). We allow for a school-specific trembling-hand error to capture school heterogeneity. The predicted value of ω is 0.54 in the control group and 0.50 in the treatment group. Columns (3) and (4) report the estimated preference parameters from the probability choice model, based on Luce (1959) and used in Andersen et al. (2008). Details are provided in Online Appendix B. All parameters are computed as nonlinear combinations, using the Delta method, of parameters estimated using maximum likelihood. Robust standard errors are presented, clustered at the school level.

† The parameters α and τ cannot be separately identified in the interval regression model. See footnote 17.

5.3 Individual Parameters

In this section, we examine the treatment effects on time preference parameters, estimated at the individual level. This allows us to gain a deeper understanding of the source of the effects observed on aggregate parameters. It also allows for heterogeneity in estimated time preference parameters, which can be important when the sample exhibits substantial heterogeneity (Gollier and Zeckhauser, 2005).

Table 7 displays estimates of the present bias parameter, the discount factor and the trembling-hand error, at the individual level. These parameters are estimated using equation (3) at the individual level. Estimates are obtained for 815 students, 444 in the control and 371 in treatment group, out of 914 in the sample.[19,20]

Table 7: Descriptive statistics for the estimated individual parameters

	Median	5^{th} Percentile	25^{th} Percentile	75^{th} Percentile	95^{th} Percentile
Control					
$\hat{\beta}_i$	1.000	0.440	0.751	1.155	2.627
$\hat{\delta}_i$	1.002	0.962	0.997	1.018	1.056
$\hat{\omega}_i$	0.149	0.000	0.000	0.358	0.585
Treatment					
$\hat{\beta}_i$	0.998	0.464	0.782	1.140	2.075
$\hat{\delta}_i$	1.003	0.961	0.995	1.014	1.108
$\hat{\omega}_i$	0.000	0.000	0.000	0.189	0.581

Note: The subscript i indicates individual i. N=815.

Table 8 displays the treatment effects on individual parameters. We first examine whether the treatment increases the share of time consistent students, those with $0.99 < \hat{\beta}_i < 1.01$, as defined in Augenblick, Niederle and Sprenger (2013). We find a significant increase in the share of time consistent students in the treatment group, of between 8 and 10 percentage points. We also estimate a multivariate multiple regression model to examine the treatment effect on the parameters estimated using equation (3). The results reveal an insignificant decrease in $\hat{\beta}_i$. This result, together with the increase in time consistency, suggests that, when individual heterogeneity is allowed, both present bias and future bias may have decreased. The data indeed reveal a decrease in the share of strongly present biased individuals, with $\hat{\beta}_i < 0.6$ (χ^2-test, p=0.07), but no

[19] We cannot estimate the parameters for 77 of the subjects, since their choices exhibit zero variance across allocation choices. The estimation does not converge for six subjects, and extreme values of β, smaller than 0.01 and larger than 9.6, are obtained for 18 subjects (upper and bottom 1%). There is no difference in the distribution of subjects across treatment and control group (χ^2 test, p=0.559, for subjects exhibiting zero variance, and p=0.199, for extreme values of β.)

[20] The estimated individual parameters correlate significantly with the underlying choices, as one would expect. The Spearman rank correlation coefficient between $\hat{\beta}_i$ and the difference between the share allocated to the earlier date when the earlier date is immediate compared to delayed is $\rho = -0.1846$ (p<0.01). The Spearman rank correlation coefficient between $\hat{\delta}_i$ and share allocated to the earlier point in time is -0.0594 (p=0.09), and between the share of consistent choices and $\hat{\omega}_i$ is -0.1448 (p<0.01). Detailed results for the estimates of $\hat{\alpha}_i$ and $\hat{\tau}_i$ are presented in Online Appendix C.

significant decrease in the share of individuals that are classified as present biased, i.e. $\hat{\beta}_i < 0.99$. At the same time, there is no significant decrease in the share of strongly future biased individuals, with $\hat{\beta}_i > 1.4$, but a decrease in the share of future biased individuals, with $\hat{\beta}_i > 1.01$ (χ^2-test, p<0.01). This could in turn explain the decrease in the aggregate level of present bias.

Table 8: Treatment effect on time consistency and individual-level time preference parameters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Time consistency		Present bias parameter: $\hat{\beta}_i$		Discount factor: $\hat{\delta}_i$		Trembling-hand error: $\hat{\omega}_i$	
Treatment	0.084*	0.103***	-0.040	-0.034	0.006	0.012	-0.071***	-0.073***
	[0.046]	[0.038]	[0.051]	[0.063]	[0.006]	[0.008]	[0.014]	[0.016]
Constant			1.106***	1.000***	1.007***	0.967***	0.192***	0.254***
			[0.035]	[0.155]	[0.004]	[0.020]	[0.009]	[0.040]
Add. controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	815	647	815	647	815	647	815	647
Adj. R-squared			0.001	0.039	0.001	0.014	0.032	0.057

Note: Columns (1-2) reports the marginal effects of a probit model on the likelihood that an individual is time consistent, i.e., $\hat{\beta}_i$ falls within $0.99 < \hat{\beta}_i < 1.01$. Columns (3-6) report multivariate regression results on all estimated time preference parameters, using equation (3). Treatment is a dummy variable that takes value 1 if the student participated in the education program. Add. Controls is Yes when individual characteristics, defined as in Table 3 (gender, grade, cognition score, relative math grade, migrant background, single parent and books at home), and month and location fixed effects are included. Robust standard errors, clustered at the school level, are computed. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

Table 8 reveals that the treatment did not affect individual discount factors ($\hat{\delta}_i$). The treatment strongly decreased the estimated trembling-hand error ($\hat{\omega}_i$). This result is consistent with the increase in the share of choices consistent with the law of demand found in the descriptive analysis.

6 Discussion

The overall pattern of results indicates that the program had a strong and robust effect on consistency. This suggests that a first impact of the educational program was to improve understanding of intertemporal tradeoffs. Such a result is important, in light of the findings of Choi et al. (2014), who show that consistency in a risk preference elicitation task is correlated with wealth and other financial outcomes.

The second result that emerges is that the treatment induces an increase in time consistency. The absence of changes in the income or spending of students across treatment and control suggests that changes in external consumption opportunities cannot explain the observed changes in intertemporal choice. We consider two additional explanations in what follows. A first explanation is that the estimated present bias may not capture any underlying feature of students' time preferences. In that case, we would expect this estimated parameter to be uncorrelated with field behaviors, such as savings. We explore this hypothesis by relating the estimated parameters to several field behaviors reported in the survey conducted after the CTB task. We consider savings behavior, i.e. whether the student saves and, if so, how much. We additionally study self-reported impulsivity measures when shopping, based on Rook and Fisher (1995) and Valence et al. (1988). This measure is the average answer to four statements: I buy impulsively; before I buy something, I consider carefully whether I can afford it (reverse coded); before I buy something important, I compare prices in the Internet or several shops (reverse coded); and, sometimes I regret having bought something new. The answers were on a 5-item Likert scale, 1-strongly disagree to 5-strongly agree. We also include a measure on efficacy at achieving savings goals. This measure is the average answer to two statements: when I plan to buy something, I manage to save for it; I am good at reaching my saving goals. The answers were provided on the same 5-item Likert scale.

Table 9 displays the relationship between the estimated present bias parameter, $\hat{\beta}_i$, and these field behaviors. A higher $\hat{\beta}_i$, implying lower present bias, is related to increased savings amounts and a higher self-reported efficacy at achieving savings goals. The correlation between $\hat{\beta}_i$ and impulsivity is also of the expected sign. Additionally, $\hat{\delta}_i$ is related to savings amount as expected. Overall, these correlations suggest that the estimated time preference parameters are informative of students' behavior.

Table 9: Estimated parameters and field behaviors

	(1)	(2)	(3)	(4)
		If save=1		Achieve
	Save (0/1)	ln(save)	Impulsivity	saving goals
Present bias ($\hat{\beta}_i$)	0.072	0.319***	-0.066	0.107*
	[0.096]	[0.060]	[0.047]	[0.053]
Discount factor ($\hat{\delta}_i$)	-1.102	4.618**	0.278	0.975
	[2.663]	[2.021]	[1.108]	[1.024]
Trembling-hand error ($\hat{\omega}_i$)	0.189	0.000	-0.175	0.486*
	[0.324]	[0.431]	[0.240]	[0.283]
Treatment	-0.916	3.777	-0.108	1.810
	[2.753]	[2.379]	[1.195]	[1.165]
$\hat{\beta}_i$ * Treatment	0.043	-0.230*	-0.062	-0.076
	[0.121]	[0.117]	[0.083]	[0.087]
$\hat{\delta}_i$ * Treatment	0.774	-3.460	0.045	-1.616
	[2.687]	[2.348]	[1.157]	[1.076]
$\hat{\omega}_i$ * Treatment	0.021	-0.677	0.828*	-0.522
	[0.435]	[0.557]	[0.470]	[0.421]
Constant	0.988	-1.364	-0.179	-1.196
	[2.721]	[2.063]	[1.138]	[1.099]
Observations	815	390	784	782
Adj. R-squared		0.033	0.013	0.013

Note: Column (1) reports estimated marginal effects of a probit model on the likelihood that an individual saves. Columns (2-4) report OLS regression results on the natural logarithm of savings, conditional on savings, self-reported impulsivity and efficacy at achieving saving goals. The latter two measures are standardized. The parameters $\hat{\beta}_i$, $\hat{\delta}_i$ and $\hat{\omega}_i$ are obtained through the estimation of equation (3). Robust standard errors, clustered at the school level, are computed. ***, **, * indicate significance at the 1, 5 and 10 percent level, respectively.

The second explanation is that the treatment may have changed how students view time-dated experimental payments, in relation to other sources of money, or if these are considered at all. Adolescents in our sample receive an allowance from their parents, of 34.2 Euro per month on average, and may have thought about it more often when offered time-dated payments, after having participated in the training. The estimates in Table 9 for the interaction between $\hat{\beta}_i$ and the treatment provide suggestive evidence that the relationship between estimated parameters and field behaviors became weaker with the treatment. In particular, we observe a marginally significant decrease in the relationship between $\hat{\beta}_i$ and savings amount in the treatment group. The same sign is obtained for efficacy at achieving savings goals, though it is not significant. This finding, together with the increase in corner choices in the treatment group, provides evidence that is suggestive of a decrease in narrow bracketing in the CTB task among treated students.

7 Conclusion

This paper examines the effect of a financial education intervention on intertemporal choices in adolescence. Following random assignment to the intervention, we measure intertemporal choices using a controlled and incentivized experiment offering a variety of time-dated payments, across different time horizons.

The program leads to significant increase in consistency of choices with the law of demand. This suggests that a first effect of the program was to enhance the understanding of intertemporal tradeoffs. Further, the program leads to a significant decrease in the aggregate strength of present bias. When individual heterogeneity is allowed, we observe that this effect is driven by both a decrease in the share of students exhibiting strong present bias and a decrease in future bias.

An important question is how such changes in the extent of time inconsistency should be interpreted. Our analysis reveals that students in the control group who exhibit lower degrees of present bias also save larger sums of money on average, suggesting that choices in the control group are predictive of field behaviors. This correlation is weakened in the treatment group, providing indicative evidence that choices in the treatment group may have been less indicative of consumption-savings choices of students. At the same time, the program increases the share of corner solutions, i.e. choices that allocate the entire budget either to the sooner or later payment date. Both results can be explained by a change in how students viewed the time-dated experimental payments offered to them; in other words, they may have bracketed less narrowly when making intertemporal choices.

These results point to a new perspective regarding the impact of financial education. Most financial education programs, including the one we study, discuss savings choices extensively, and hence most studies that investigate the impact of financial education on behavior focus on outcomes such as saving. Our results suggest that short financial education programs may not affect savings (in the short term), but may change how individuals at a young age view intertemporal tradeoffs, enhancing both their understanding and broadening the set of alternatives that they consider when making such choices.

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