Self-Control Awareness and Intertemporal Choice Behavior: Evidence from a Randomized Experiment

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Abstract

This paper investigates how awareness about self-control problems (i.e., sophistication) affects intertemporal choice behavior. In a randomized experiment, the treatment group was provided information about self-control problems. In an incentivized task, study participants intertemporally divided $500 into two payments (the amount allocated to the later payment was paid with interest). The results show the treatment was more willing to delay gratification, choosing a later reward 8% to 16% larger than the control. We provide evidence that the results are due to greater self-control awareness.

Keywords: Self-Control, Naïveté, Sophistication, Present Bias, Intertemporal Choice, Randomized Experiment

I. Introduction

Recent work has argued that awareness about self-control problems (i.e., sophistication) is a central issue to the welfare implications of self-control. Sophisticated time-inconsistent individuals may be able to improve their welfare by voluntarily taking up commitment devices (Laibson 1997; Ashraf, Karlan and Yin 2006; Beshears, Choi, Laibson and Madrian 2011). Moreover, firms may take advantage of consumers' naïveté (i.e., their over-optimism about their future self-control) by designing exploitative

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contracts that redistribute surplus from naïve consumers to firms (e.g., DellaVigna and Malmendier 2004; Eliaz and Spiegler 2006; Heidhues and Koszegi 2010).[2] Despite the importance of the topic, there is very little empirical evidence of how naïveté affects behavior. One exception is Wong (2008), who finds mixed evidence of the effects of naiveté on educational outcomes.[3]

This paper reports the results of a randomized experiment in which the treatment group was provided information about self-control problems (the control group was not provided any information). The experiment was conducted with 3,400 participants of an Internet-based longitudinal study, the RAND American Life Panel. All participants completed an incentivized intertemporal choice task in which they had to divide $500 into two checks; the amount allocated to the second check was paid with interest and was mailed one month after the first check. A fraction of participants were randomly selected to be paid according to their choices; they were sent two checks in the amounts chosen by them in the intertemporal choice task (the total amount of the two checks was greater or equal to $500). Our outcome of interest is the "later reward", the amount in dollars of the second check.

The "treatment information" was based on the results of a study conducted by Daniel Read and Barbara van Leeuwen (1998). Their study (henceforth, the Snacks Study) showed that when faced with a choice between a healthy and an unhealthy snack *that would be delivered a week later* participants were equally likely to choose either of them. However, the same participants had approximately an 80% chance of choosing an unhealthy snack *if they could eat their choice right away*.

[2]The literature on self-control originally focused on two extreme cases: "Sophisticated" people are fully aware of their self-control problems and correctly predict how their future selves will behave (Laibson 1997), and "naïve" people are fully unaware of their self-control problems and incorrectly believe their future selves will behave exactly how they would like them to behave (O'Donoghue and Rabin 1999a, 1999b). O'Donoghue and Rabin (2001a) introduced the idea that individuals may be "partly naïve"—they are aware of their self-control problems but they underestimate the magnitude of these problems.

[3]The author elicits university students' ideal study plan to a midterm examination and asks them to predict how much they will delay in initiating their plan – predicted delay is interpreted as a measure of sophistication and unpredicted delay as a measure of naïveté. The results show that both measures have negative effects on several measures of class performance.

In our study, participants assigned to treatment were asked to make similar hypothetical choices (for a week later and for right away) between a healthy, an apple, and an unhealthy snack, a bag of Doritos. After making their choices, they were told about the results of the original study, which were presented as an illustration of self-control failure.

The results show that the treatment increased the willingness to delay gratification. When faced with an experimental monthly interest rate of 0.5%, participants assigned to the treatment group choose a delayed reward $27-$50 larger than participants assigned to the control group, an 8%-16% increase relative to the control mean. We present evidence that the results are due to an increase in self-control awareness by documenting that the treatment effect is larger for participants who more likely understood the experiment information.

To address concerns that the results could be due to experimenter demand effects (i.e., changes in behavior due to cues to what constitutes appropriate behavior), a second treatment arm was not shown the last information screen, which would potentially have the largest experimenter demand effects.[4] The results show that participants who saw the last information screen were more willing to delay gratification than those who saw all the information screens but the last one; however this difference was not statistically significant.

To the best of our knowledge, the current study represents the first attempt to manipulate self-control awareness using information. Under the assumption that the treatment increased awareness, the results indicate that at least some fraction of the population is naïve or partly naïve and that, therefore, there are limits to the effectiveness of commitment contracts in helping individuals overcome their self-control problems.

One possible take-away of the results is that it may be possible to stimulate the demand for commitment by providing information about self-control. The choices made in the experiment task were made under commitment; participants would have no

[4] The text in the last screen included: "*Present bias is often seen as undesirable because what people end up doing (saving less than planned) is not what they think is best for their future (saving for retirement).*" See section II.1.

opportunity to change their allocations later on. Thus, they may have used the experimental allocation as a commitment device to set money aside for the future. We lay out a framework that formalizes this interpretation of the results.

The organization of the paper is as follows. Section 2 explains the experimental design and the intertemporal choice task and describes the data. The empirical results are presented in section 3. Section 4 lays out a model that provides an interpretation for the empirical results. Section 5 concludes.

II. Experimental Design and Data

We collected data for this study using the RAND American Life Panel (ALP), an ongoing Internet panel with respondents ages 18 and over living in the U.S. Respondents with no Internet access at the time of recruitment are provided laptops, including an Internet access subscription. Because Internet access is not a prerequisite for joining the panel, the sample does not suffer from selection due to a lack of Internet access. About twice a month, respondents receive an email with a request to visit the ALP site and complete questionnaires. Post-stratification weights are provided for estimating nationally representative results.[5]

Approximately 3,400 panel respondents (corresponding to a response rate of 80%) participated in ALP module number 212, which was particularly designed for the current project.[6] Participants were randomly selected into one control and two treatment groups, with probabilities 40% (control), 40% (first treatment arm) and 20% (second treatment arm). The second treatment arm was designed to study experimenter demand effects, as we explain below. The core questionnaire, which was administered to all participants, started with an incentivized intertemporal choice task. The treatment groups received information about self-control immediately before such task. At the very end of the survey, the treatment groups were asked open-ended questions about the information they had received.

[5]The weights are constructed to make the distributions of age, sex, ethnicity, education and income estimated using the ALP sample approximate the distributions in the Current Population Survey.
[6]The data of the module are publicly available at https://mmicdata.rand.org/alp/. The survey can be accessed at https://mmic.rand.org/mmic/playground/ms212/index.php.

II.1. Experiment

The treatment groups were provided information about self-control problems (the control group was not provided such information) illustrated by the results of a study conducted by Read and van Leeuwen (1998). Participants saw a total of 6 or 7 "information screens." In the first screen, they were asked to make a hypothetical choice between an apple and a bag of Doritos that they would receive one week later. In the second screen, they were asked what their choice would be if instead they could have their choice right away. The third screen informed that the questions that had been asked were based in a real-life research study and more details were given:

"*In the study participants were visited at their workplace and told that free snacks would be given away at their workplace* ***one week later****. They were asked to choose between healthy (an apple) and unhealthy snacks (Doritos), and informed that their choice would be delivered* ***one week later****. 5 out of 10 people chose a healthy snack* ***one week later****."* [end of screen #3]

"*One week later after the first visit the same participants were revisited at their workplace. Snacks were being given away, and the study participants were asked to choose between healthy (an apple) and unhealthy snacks (Doritos), which they could have* ***right away****. Only 2 out of 10 people chose a healthy snack* ***immediately****."* [end of screen #4]

Respondents were then prompted to think about the results of the study:

"*Why did some people change their minds? What was the study trying to show? When people make plans for the future, they often end up not doing what they had originally planned. When asked to choose which snack they would like* ***next week****, half of the people chose the healthy snack (the apple) because they care about their health. When asked what they would prefer* ***right now,*** *8 out of 10 people chose the unhealthy snack because they could not resist the temptation of having the unhealthy snack (the Doritos).*" [end of screen #5]

In screen #6, participants were reminded that self-control issues do not apply only to eating:

"*This behavior is not limited to eating. It also applies to other behaviors such as exercising, quitting smoking and saving. In the case of savings, someone may recognize the importance of saving for retirement, and plan on stepping up her savings starting with the* ***next paycheck****. But when the next paycheck arrives and it is time to start saving more, the person faces a dilemma: she wants to save more, but saving more requires spending less. She cannot resist the temptation to consume and ends up saving less than originally planned.*" [end of screen #6]

Finally, the first treatment arm was shown an additional screen, **which was not seen by the second treatment arm**, presenting present bias as undesirable:

"*When thinking about how much they want to be saving in the months to come, people are foresighted and plan to save more. But when they have to decide how much to save this month, they are shortsighted and save less than planned. This type of behavior is often referred to as present biased. The term refers to the phenomenon that people tend to choose whatever gives them immediate satisfaction (to spend), even though this is not what they think it is good for their future (to save more). Present bias is often seen as undesirable because people end up doing (saving less than planned) is not what they think is best for their future (saving for retirement).*" [end of screen #7]

The Appendix shows screen shots of the information screens seen by individuals in the treatment group. They included not only the text above but also pictures to ease comprehension and retain attention. Individuals selected into the no-information/control group did not see any of the information treatment screens presented above.

II.2. Intertemporal Choice Task

Participants completed an incentivized intertemporal choice task designed using the methods of Andreoni and Sprenger (forthcoming). Intertemporal choice tasks are commonly used to measure willingness to delay gratification (Frederick, Loewenstein and O'Donoghue 2002).[7] Individuals were asked to intertemporally allocate an experimental budget of $500 between two payments one month apart. The amount saved for the second check was paid with interest. Participants were asked to make six of these choices, in which we varied the monthly interest rate (r = 0.5%, 1% and 2.5%) and the mailing date of the first payment (today or in 1 year).[8] Whenever participants were prompted to make a choice, two checks—identical to the checks ALP participants receive every quarter for participating in the survey—were displayed, showing the dates and the amounts of each one of the two checks, and the name of the survey participant (see screen shot in the Appendix).[9] The amount of the second check included the interest paid on the amount saved for the second check, such that the participant did not have to calculate the interest herself.[10] Approximately 0.1% of participants were selected at random to be paid one of their choices (the choice for which the participant was paid was randomly selected among the six choices the participant had made).[11] These participants were sent (one or) two checks (at the dates specified in the task) in the amounts chosen by them in the task; the total amount of the two checks was greater or equal to $500.

[7]We use the term "willingness to delay" broadly to include – not only time preferences – but any reasons one may care less for future consequences—for example, uncertainty about whether the delayed reward will be paid.

[8]The order of the time frames was randomized. Given a time frame, the interest rate increased monotonically.

[9]To mitigate the concern that subjects may choose to receive money sooner because they are uncertain about whether the later payment will be delivered—a common criticism to choice tasks used to elicit time discounting (Frederick, Loewenstein and O'Donoghue 2002), we used images of the ALP checks respondents receive every quarter with the intent of taking advantage of the built trust between the respondents and the survey staff.

[10]Participants were asked to choose how much they wanted to save for the second check by either clicking on a ruler or by entering the amount in a box below the ruler (see screen shot in the Appendix). As the individual entered her choice, the amounts of the first and the second checks were automatically updated. As the participant moved the cursor left and right, she could try out different allocations along the experimental budget constraint, getting a feel for the trade-off between receiving the money sooner and waiting to receive more money. The practice round that preceded the actual choices instructed participants to move the cursor around to see how the amounts of the first and second checks varied according to their choices of how much to save for the second check.

[11]Participants were told that "*Ten survey participants will be selected at random to earn real money.*" They were not given information about how many survey participants were completing the survey.

III. Results

III.1. Summary Statistics and Balance Check

Table 1 shows summary statistics of baseline characteristics, separately for the three experimental groups.[12] The results suggest the groups are balanced along most background characteristics. The table displays means and standard deviations (SD) for each group, as well as p-values of two-way tests of the difference in means across the groups. We can only reject the null hypothesis of equality in 8 out of the 84 tests (or 9.5% percent of the times). For most of the analysis, we will restrict our attention to comparisons between the control group and the first treatment arm, who saw all 7 treatment screens. In Section III.4, we will look at experimenter demand effects by comparing the choices of the first and second treatment arms.

[Table 1 here]

III.2. Main Results

Figure 1 shows the cumulative distribution of the later reward—i.e., the amount in dollars of the second check—separately for treatment and control.[13] The mass of the distribution of treatment is shifted to the right relative to the distribution of the control group: the treatment group chooses on average a larger delayed reward.

[Figure 1 here]

Table 2 presents ordinary least squares (OLS) estimates of the treatment effect on the later reward, separately by interest rate and time frame. Panel A reports results for the "today x in 1 month" time frame (i.e., the mailing dates of the 1st and 2nd checks were "Today" and "In 1 month"). Panel B reports results for the "In 1 year x In 1 year and 1 month" time frame. The experimental (monthly) interest rate varies across columns. The

[12]The baseline characteristics come from the household information survey of the ALP that collects demographic and socioeconomic data on all ALP participants. We dropped 28 observations for whom at least one of the control variables was missing. The reported statistics are unweighted. See Appendix Table 1 for weighted estimates.

[13]The ruler allowed respondents to delay to the second check any amount multiple of $5. If the respondent used the box instead, she could choose any amount multiple of $1. The stepwise form of the cumulative distribution is due to respondents' preferences for choices that were multiples of $50.

results suggest that the allocations tend to adhere to the law of demand: the higher the interest rate, the larger the delayed reward.

The results in Table 2 show that the treatment increased the later reward. Participants who received the information chose on average a later reward from $5 to $30 larger than the average later reward of participants who received no information (1% to 10% of the control average).

We look next at how the treatment effects vary across the different time frames and interest rates. The treatment may in principle have two effects: it may change one's expectations about how present biased she will be in the future and it may also affect one's (current) present bias. As we explain in Section IV, the second effect is of second order when one is deciding how to allocate $500 that will be paid 12 months later.[14] In fact, the results are similar across the different time frames and indeed one cannot reject the hypothesis that they are the same.[15]

In terms of differences across interest rates, a central issue is censoring. In the intertemporal choice task participants could not borrow nor lend at the experimental interest rate. Consequently, the choice of later reward is left censored at $0 for participants who would have liked to borrow (i.e., a sooner reward greater than $500). Similarly, the choice of later reward is right censored at $500 times the gross interest rate for participants would have liked to lend (i.e., a sooner reward smaller than $0). According to the law of demand, right (left) censoring should be an increasing (decreasing) function of the experimental interest rate. Because right censoring is more common in our data than left censoring (at the 2.5% interest rate more than 40% of control group chose to delay the maximum amount of $500 while only 4% chose a later reward of $0), the treatment effects are smaller for the higher interest rates.

[Table 2 here]

[14]In our framework the effect of a change in the present bias is a priori ambiguous under the "today x in 1 month" time frame.

[15]One possible interpretation is that there was no present bias when respondents allocated $500 between two checks to be mailed "today" and "in 1 month", because they would not receive the money immediately.

For the remainder of the paper, we will focus on the choices made in the "In 1 year x In 1 year and 1 month" time frame under a 0.5% monthly interest. Even though the results are not much different across the time frames, focusing on the "In 1 year x In 1 year and 1 month" time frame allows us to as rule out that the results are due to an effect of the information on the present bias itself (rather than on the expectations about one's present bias in the future), as discussed above. We choose the 0.5% interest rate because there is less censoring at this rate.

Table 3 takes a closer look at the issue of censoring.[16] The OLS results are reproduced in the first column. The second and third columns show the effects on the probabilities of delaying $0 and $500 in the experiment. The last two columns show results from models that take into account the censoring at 0 and 500. These models estimate the effect on the latent later reward, i.e., the later reward that would have been observed had participants been allowed to borrow or lend in the intertemporal choice task at the experimental interest rate. We use a trimming procedure proposed by David Lee (2009) to produce the last column estimate (see Appendix for derivation of this estimator).[17] The treatment and control distributions of later reward are trimmed such that the distribution of unobservables is the same across treatment and control. The advantage of this procedure over the Tobit estimator is that it requires no assumptions about the distribution of the unobservable. As a robustness check, Panel B presents results in which we add controls.[18]

Consistent with the treatment leading to an increase in the willingness to delay gratification, there is a decrease in the probability of delaying $0 and an increase in the

[16]Estimates in Table 2 and Table 3 are unweighted. See Appendix Tables 2 and 3 for weighted estimates.
[17]David Lee (2009) uses the trimming procedure to estimate bounds on treatment effects of Job Corps on wages when the treatment affects the probability that one's wage will be observed. In our case, we can take advantage that we have more information about the latent dependent variable—that is, we know that the latent later reward is smaller than 0 if the observed later reward is left censored at 0 and that the latent later reward is greater than 502.5 (502.5 = 500 x 1.005) if the observed later reward is right censored at 502.5—and use the trimming procedure to estimate point estimates rather than bounds.
[18]Controls include all the variables on Table 1, namely: gender; age; date of birth; born in US; US region of birth; race; marital status; education; employment status; disability; US region of residence; household size and income.

probability of delaying $500. The estimators that take into account the censoring produce estimates that are much larger than the OLS. They indicate that the treatment increased the later reward in $50, which correspond to roughly 16% of the control mean. Overall, the results show that the information provision increased participants' willingness to delay gratification.

[Table 3 here]

III.3. Treatment Effects and Understanding of the Snacks Study

In what follows, we provide evidence that the experimental results are (at least partly) explained by an increase in individuals' awareness of self-control problems. We show that the treatment effect is larger for participants who seem to have had a better understanding of the experiment information. We assess participants' understanding from their answers to the following open-ended question that was asked at the very end of the survey (to treatment participants only): "*Did you learn something from the Apples or Doritos real-life study? Could you explain in your own words what you learned*?" In total 856 respondents (approximately 65% of the participants randomly selected into the first treatment arm) answered the question.[19] The answers were reviewed to identify themes and terms that showed up frequently and corresponding categories were created. The categories were not exclusive, such that one answer could be classified into multiple categories.[20]

Table 4 presents the percentage of answers that were coded into each category. For example, 17.9% of the respondents used the term "instant gratification" or similar variants, such as "immediate satisfaction" or "immediate pleasure." Approximately 12% made reference to the idea that individuals tend to have good intentions, but do not act as they intended. 7.7% pointed that "[t]*he decisions people make for the future are not the same ones they make for today*." Roughly 10% explained why they had chosen an apple

[19]Appendix Table 4 shows which personal characteristics are associated with not answering the open ended questions.
[20]To test for coding reliability, the answers were coded by two independent researchers.

or a bag of Doritos; 6.4% mentioned that people should eat healthier; and 4.6% stated that people prefer junk food.

[Table 4 here]

We interpret that respondents whose answers were coded into any of the following categories had a better understanding that participants in Read and van Leeuwen (1998)'s Snacks Study switched to an (immediate) unhealthy snack because of self-control problems: *instant gratification; people have good intentions but don't act as intended; different decisions for now and later; temptation; impulsivity; self-control; present bias; procrastination; people prefer now than later; people overestimate their self-control; and willpower*.[21] With this choice of categories, we were hoping to identify respondents who tapped into at any of the following five ideas associated with self-control failure: i) individuals have a preference for immediate gratification; ii) it takes self-control/willpower to resist temptations; iii) individuals will make different decisions for now and for later; iv) individuals make plans for the future, but end up not following them; and v) individuals procrastinate costly actions. Henceforth, we will use the term "understanding" to refer to responses that have been categorized into at least one of these categories. 38.5% of participants who answered the question (25% of participants assigned to the first treatment arm) were classified as having understood the experiment information.[22]

Panel B of Table 5 shows which baseline characteristics are associated with having a better understanding among the treatment group. Being more educated and having a higher income are positively associated with understanding. Understanding is also

[21]Notice that we cannot rule out that participants whose answers were not coded into these categories (including those who did not answer the question) may have understood the experiment information. We intentionally did not interpret as having understood the experiment information categories that that could be alternatively interpreted as time consistent preferences, such as *patience; people cannot delay gratification; people prefer sooner than later; and people live in the moment.* The results are very similar if we include those. We opted for remaining agnostic about ambiguous answers such as *people change their minds* and *people should stick to their plans.*

[22]The correlation between the coding of two independent researchers for "understanding" was 0.95.

positively associated with financial literacy (last two columns).[23] The table reports results from a multinomial logit regression with three possible outcomes: understanding, not answering the question and all other answers. The table displays the estimated coefficients for understanding (see Appendix for estimated coefficients of not answering). These estimated coefficients can then be used to estimate the predicted probability that control (and treatment) participants would have understood the experiment information had they received it.[24] Using these estimated probabilities, we can investigate whether the treatment effect is larger among those with a higher predicted probability of understanding. Panel A shows results from a regression of the later reward on the predicted probability, the treatment dummy, and the interaction of the predicted probability with treatment status. We estimate bootstrapped standard errors to take into account that the predicted probability is estimated.

The results in Panel A of Table 5 suggest that the information had the largest effect for those with a higher probability of understanding. For example, the estimate in the first column indicates that a 1 percentage point increase in the probability of understanding corresponds to an increase in the treatment effect of $1.35. Interestingly, it is not possible to reject the hypothesis that there is no treatment effect for those with a zero probability of understanding.

[23]The three questions about financial literacy had been asked in a previous round of the ALP (ms189): "*Suppose you had $100 in a savings account and the interest rate is 20% per year and you never withdraw money or interest payments. After 5 years, how much would you have in this account in total? More than $200, exactly $200 or less than $200?*"; "*Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account? More than today, exactly the same or less than today?*"*;* and "*When an investor spreads his money among different assets, does the risk of losing money increase, decrease or stay the same?"* We lose about 270 observations because some respondents who participated in ALP ms212 did not participate in ALP ms189.

[24]We can compare within the treatment group the observed outcome (understood, did not answer, or other answers) to the respective predicted probabilities to assess how well the model performs in identifying respondents who understood the experiment information. 40% (model 1) and 48.9% (model 2) of respondents classified into the understanding group had a predicted probability of understanding higher than 1/3. 78.8% (model 1) and 79.1% (model 2) of those who did not answer the question had a predicted probability of not answering higher than 1/3. Finally, 65.3% (model 1) and 63.4% (model 2) of respondents who answered the question but were not classified into the understanding group had a predicted probability of other answers higher than 1/3.

[Table 5 here]

Figure 2 presents similar evidence in a graphical form. It shows the conditional expectation of the later reward as a function of the predicted probability, separately for the control and treatment groups. The conditional expectations are calculated using a locally weighted regression smoother (Fan 1992), which allows the data to determine the shape of the function, rather than imposing some functional form. The straights lines show OLS estimates.

Immediately apparent from Figure 2 is that the curve for the treatment group is positively sloped: Treatment participants that are more likely to have understood the information chose on average a larger later reward. The curve for the control group suggests that the positive relationship within treatment is not due to any baseline differences between those with lower and higher probabilities of understanding; the relationship is slightly negative within the control group. The gap between the two curves provides an estimate of the treatment effect conditional on the probability of understanding. The figure shows that the gap between the two curves widens as the probability of understanding increases, which again suggests that the treatment effect is larger for those with a higher probability of understanding.[25]

[Figure 2 here]

III.4. Experimenter Demand Effects

One potential concern about the results presented is that they could be due to experimenter demand effects—i.e., changes in behavior due to cues to what constitutes appropriate behavior. To address this concern, a second treatment arm was intentionally not shown the last information screen, which would potentially have the largest experimenter demand effects: "*Present bias is often seen as undesirable because what*

[25]The predicted probability in Figure 2 ranges from 0.15 to 0.4 because approximately 70% of respondents have predicted probabilities in this range.

people end up doing (saving less than planned) is not what they think is best for their future (saving for retirement)."[26]

Table 6 shows estimates of the experimenter demand effects. We restricted the sample to treatment participants and compared the intertemporal choices of those who saw the 6 "core" screens and those who saw the 6 core screens plus the additional seventh screen.[27] The table reports results from regressions of some later reward measure on a constant and an indicator variable identifying respondents who were randomly selected to see the additional seventh screen. The coefficient of the constant (in Panel A) shows the average later reward for the comparison group who saw the 6 core screens only. The coefficient of the indicator variable estimates the effect of the seventh screen.

The results in Table 6 show that there is a $8-$14 increase in the later reward associated to the seventh screen. However, the effects are imprecisely estimated and they are not statistically different from zero. Moreover, the effect of the seventh screen is much smaller than the effect of seeing all 7 screens (the estimated treatment effect in Table 3 obtained by comparing those who saw 7 screens to those who did not see any of the information screens). Indeed, we can reject the hypothesis that the effects in Tables 3 and 6 are the same. Finally, it is worth noting that experimenter demand effects tend to be less of a concern in Internet-based surveys, which are anonymous, self-reported, and in which the subject/respondent does not interact with an experimenter/interviewer (Zizzo 2010).

IV. Model

[26]The entire text in the last screen read: "*When thinking about how much they want to be saving in the months to come, people are foresighted and plan to save more. But when they have to decide how much to save this month, they are shortsighted and save less than planned. This type of behavior is often referred to as present biased. The term refers to the phenomenon that people tend to choose whatever gives them immediate satisfaction, even though this is not what they think is good for their future. "Present bias is often seen as undesirable because what people end up doing (saving less than planned) is not what they think is best for their future (saving for retirement)."*

[27]For the control group and the first treatment arm, it was randomized which time frame they received first: "Today x 1 month" or "1 year x 1 year and 1 month". Because of sample size concerns, all participants assigned to the second treatment arm received the "Today x 1 month" time frame first. To avoid confounding the "time frame effect" with the "information provision effect", the analysis only includes participants from the first treatment arm who received the "Today x 1 month" time frame first.

A prediction of most self-control models is that sophisticated agents will resort to external commitment devices to help them fulfill their plans for the future. Our results are consistent with greater sophistication having increased the demand for commitment. The choices made in the experiment task were made under commitment; participants would have no opportunity to change their allocations later on.[28] Thus, they may have used their experimental allocation as a commitment device to set money aside for the future. In the words of one of the study participants: *"I like to get more money later because it is like I "already have it saved" in my mind. I can spend the first check but the rest I have to put into savings."* In this section, we lay out a framework that formalizes this interpretation of the results.

The model considers the decision participants faced of intertemporally allocating an experimental budget of $500 prize between two periods. The (experiment) budget constraint the agent faces is:

$$SR + \frac{LR}{1+r} = 500 \qquad \text{and} \qquad 500 \geq \frac{LR}{1+r} \geq 0, \tag{1}$$

where SR (sooner reward) is the dollar amount of the first check, r is the experimental interest rate, and LR (later reward) is the dollar amount of the second check. For simplicity the market interest rate, the experimental interest rate and the discount rate are assumed to be all equal to zero.

Following Andersen et al (2008) and Andreoni and Sprenger (forthcoming), the model assumes that the experimental rewards are integrated with background consumption; the sooner reward is integrated with the 12 months background

[28]The results of Giné et al (2012) suggest that our study participants would have reallocated money to the first check if they were given the option a year later to revise their allocations. Using a similar intertemporal choice task, they show that individuals who exhibited "preference reversals"—i.e., they chose to save more for the later payment when both payments were moved further into the future (while the time delay between the two payment was held constant)— tend to reallocate more money toward sooner when given an opportunity to revise their choices.

consumption c_{12} while the later reward is integrated with the 13 months background consumption c_{13}:[29]

$$\max_{SR,LR} \beta u(c_{12} + SR) + \beta u(c_{13} + LR) \qquad \text{s.t. (1)}, \tag{2}$$

where β is the present bias, and $u(\cdot)$ has diminishing marginal utility. Andersen et al (2008) define background consumption as "*the optimized consumption stream based on wealth and income that is [perfectly] anticipated before allowing for the effects of the money offered in the experimental tasks.*"[30] In the context of the current model, individuals may however not correctly anticipate what their (background) consumption will be 12 months and 13 months later if they are naïve or partly naïve and have incorrect beliefs about how present biased they will be in the future. It is anticipated background consumption—and not actual background consumption—that matters for the choice the individual makes about how to intertemporally allocate her experimental budget. Henceforth, we will use the term background consumption to refer to the background consumption the individual anticipates she will have 12 and 13 months later, c_{12} and c_{13}, when she will receive the experimental rewards.

The solution to the problem is given by the following first order condition:[31]

$$u'(c_{12} + SR) = u'(c_{13} + LR). \tag{3}$$

The individual equalizes the marginal utilities of the sooner and the later rewards. The hypothesis of diminishing marginal utility implies that the individual will choose to

[29]Here we focus on the 12th months versus 13th months time frame because it corresponds to the "in 1 year x 1 year and 1 month" time frame of most of our empirical results. In footnote 31 we discuss the intertemporal allocation problem for the case when the participant has to allocate the $500 between two checks to be mailed "today" and "in 1 month."

[30]Notice there is an assumption, which is the standard in the literature, that the agent chooses the optimal background consumption without taking the experimental rewards into account, such that the agent does not re-optimize if there is any reallocation of the experimental rewards.

[31]In the case of the "today x 1 month" time frame, we would have:

$$u'(c_0 + SR) = \beta u'(c_1 + LR), \tag{3'}$$

$$ln\, u'(c_0) \; - ln\, u'(c_1) = ln\left[1 - \left(1 - \widehat{\beta}\,\right)\frac{\partial c_1}{\partial x_1}\right] + ln\beta - ln\widehat{\beta}\,. \tag{6'}$$

The effect of sophistication on the later reward may be positive or negative depending on the degree of risk aversion. See Tobcman (2007) for derivation of (6').

equalize overall consumption – i.e., the sum of background consumption plus the experimental reward – across periods:

$$c_{12} + SR = c_{13} + LR. \tag{4}$$

Thus, the later reward is a function of the profile of background consumption:

$$LR = 250 + (c_{12} - c_{13})/2. \tag{5}$$

If the profile of background consumption is negative sloped (i.e., background consumption decreases over time), the individual will delay more than $250 in order to equalize the overall consumption across the two periods. The steeper the (negative) profile is, the more the individual will choose to delay for the second check to be mailed in 13 months.

The profile of background consumption in turn depends on one's beliefs about how present biased she will be in the future. Agents choose their background consumption by optimally allocating their real-life resources—wealth and income—before allowing for the effects of the money offered in the experimental tasks. We refer the reader to the Appendix for a formal derivation of the background allocation problem. Here we discuss the implications of its solution for the choice of how to intertemporally allocate the $500 experimental budget.

The individual anticipates that the profile will be steeper (with a larger reduction in consumption between 12 and 13 months) the more present biased she expects to be in the future (Harris and Laibson 2001): [32]

$$ln\, u'(c_{12}) \; - ln\, u'(c_{13}) = ln\left[1 - \left(1 - \widehat{\beta}\,\right)\frac{\partial c_{13}}{\partial x_{13}}\right]. \tag{6}$$

[32]We assume that the individual expects that in the future she will be sophisticated about her present bias $\widehat{\beta}$. If instead the individual expects her future selves will be completely naïve about the present bias $\widehat{\beta}$, then

$$ln\, u'(c_{12}) \; - ln\, u'(c_{13}) = \frac{1}{\gamma} ln\left(\widehat{\beta}\,\right).$$

where $\widehat{\beta}$ is one's beliefs about her future self-control problems (i.e., her beliefs about what her present bias will be in the future). The parameter $\widehat{\beta}$ governs naïveté; a reduction in $\widehat{\beta}$ corresponds to a reduction in naïveté. A naïve person believes she will not have self-control problems in the future: $\widehat{\beta} = 1$. A sophisticated person knows exactly her future self-control problems—i.e., $\widehat{\beta} = \beta$, where β is the true present bias. A partially naïve person has beliefs $\widehat{\beta} \in (\beta, 1)$. The term $\frac{\partial c_{13}}{\partial x_{13}}$ is the marginal propensity of consumption (out of cash-on-hand x_{13}) at 13 months.

The combination of equations (5) and (6) imply that – for reasonable levels of $\widehat{\beta}$ – the later reward increases with sophistication. From today's perspective the individual values consumption in 12 and 13 months equally and would prefer a flat consumption profile. However, she anticipates that at 12 months her "future self" will discount 13 months consumption at $\widehat{\beta}$ and thus choose a negative profile of background consumption. The "current self" commits to having a flatter profile of overall consumption by choosing to delay more than $250 to 13 months. A reduction in naïveté $\widehat{\beta}$ leads the individual to realize that the profile of background consumption will be even steeper than she initially anticipated and consequently she will choose to increase the later reward and commit even more resources to the 13th month.

The model predicts that the optimal later reward is very sensitive to one's naïveté $\widehat{\beta}$ but is almost invariant to the true present bias β. As the appendix shows, the true present bias β only affects the choice of current consumption; the (incorrect) predictions of the current self about how much she will consume in the future depends on how present biased she expects to be in the future, $\widehat{\beta}$. Even though the choice of current consumption in theory affects how much cash-on-hand will be available in future periods, the effect is negligible 12 months later. Thus, the effect of awareness on demand for commitment, such as commitment savings, is large relative to the effect of having self-control problems. This result contrasts with predictions of consumption models without a

commitment device, in which the effect of awareness is small relative to the effect of being present biased (O'Donoghue and Rabin 2001b).

V. Conclusion

This paper investigated how awareness about self-control problems (i.e., sophistication) affects intertemporal choice behavior. In a randomized experiment, the treatment group was provided information about self-control problems; the control group was not provided any information. All participants completed a choice task in which they had to intertemporally allocate an experimental budget. The results show that the treatment increased the willingness to delay gratification. We present evidence that the results are due to an increase in self-control awareness and that they cannot be explained by experimenter demand effects.

To the best of our knowledge, this is the first study to attempt to manipulate self-control awareness using information. Its results contradict the idea that self-control awareness may be an immutable trait. It is also one of the few studies to provide evidence suggesting that self-control awareness affects economic behavior.

One possible interpretation of the results is that increased sophistication stimulated the demand for commitment. Because the choices made in the experiment task were made under commitment (participants would have no opportunity to change their allocations later on), they may have used the experimental allocation as a commitment device to set money aside for the future, as formalized in our model. Further testing this hypothesis is an important topic for future research. Future research should also study the dynamics of the effects of information provision. Our results imply important immediate effects, but it remains unclear how lasting these effects are.

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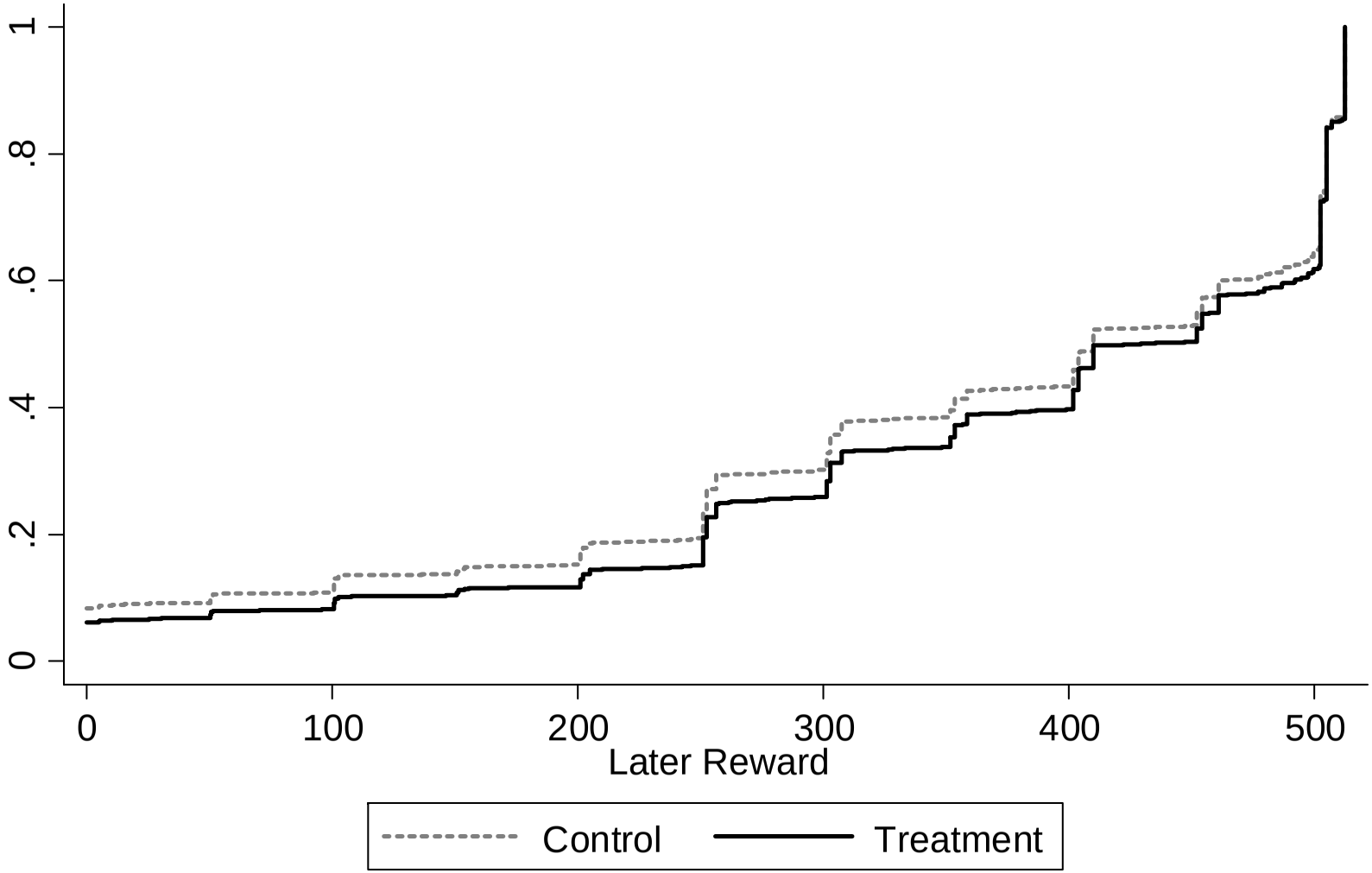
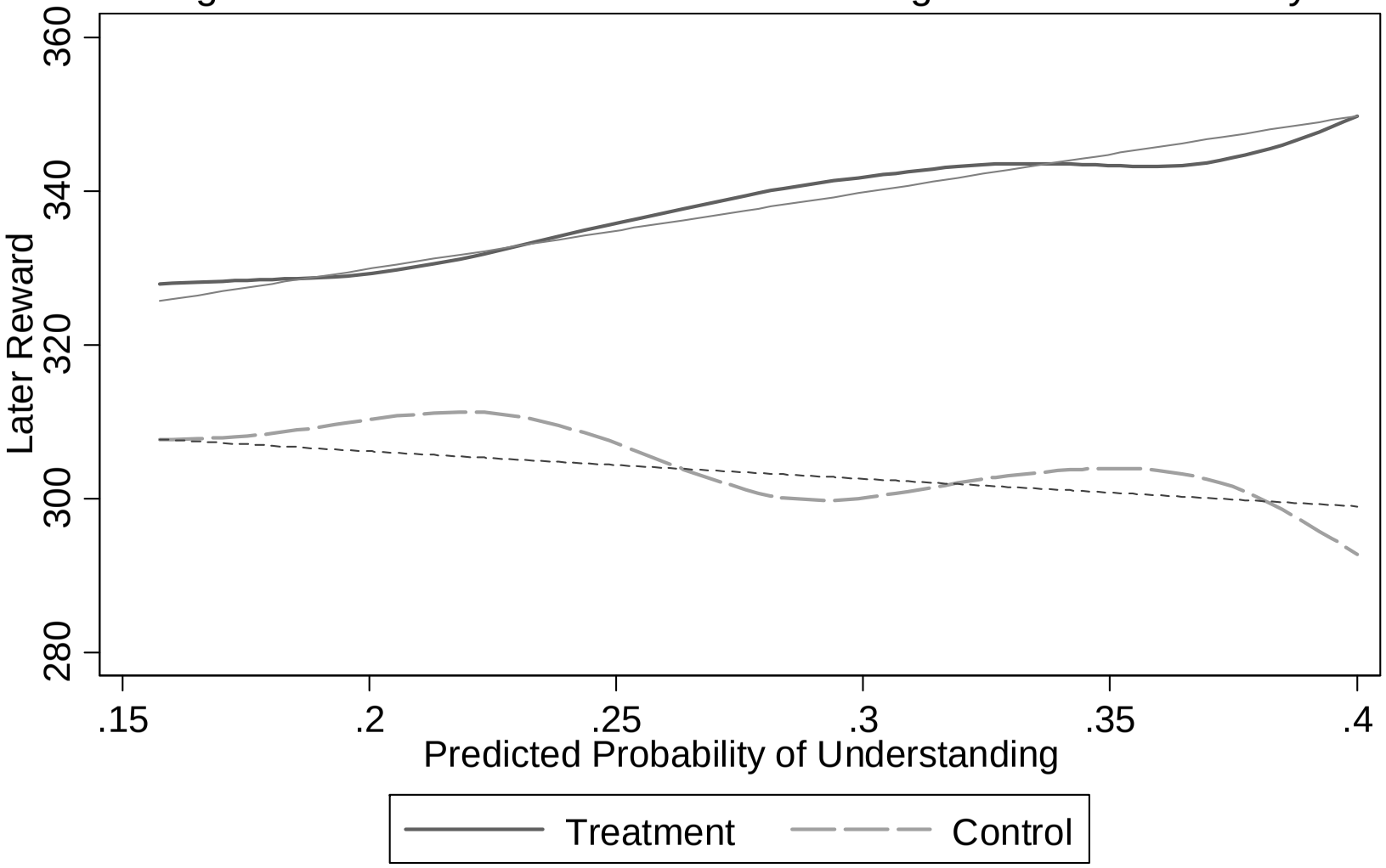
Figure 1: Cumulative Distribution of Later Reward



Note: The figure shows the cumulative distribution of the later reward (i.e., the dollar amount of the 2nd check) by treatment status.

Figure 2: Later Reward and Understanding of the Snacks Study



Note: The curves show the expectation of the later reward conditional on the predicted probability of understanding the experiment information, estimated using a locally weighted regression smoother. The lines show the ordinary least squares estimates.

Table 1: Summary Statistics and Balance Check

	Control		Treatment Arm #1		Treatment Arm #2				
	(1)	(2)	(3)	(4)	(5)	(6)	**Test (1) = (3)**	**Test (1) = (5)**	**Test (3) = (5)**
	Means	SD	Means	SD	Means	SD	P-value	P-value	P-value
Individual Characteristics									
Male	41.4%	49.3%	41.8%	49.3%	38.6%	48.7%	0.86	0.22	0.17
Age	48.6	15.1	49.2	15.4	48.2	14.4	0.36	0.53	0.16
Year of Birth	1962.0	15.1	1961.4	15.4	1962.4	14.4	0.37	0.54	0.17
Month of Birth	6.6	3.4	6.5	3.4	6.7	3.4	0.44	0.55	0.21
Birth Day	15.6	8.8	15.3	8.9	16.0	8.8	0.45	0.31	0.10
Born in US	92.7%	26.1%	91.5%	27.9%	92.8%	25.8%	0.26	0.90	0.28
Born in the Midwest	26.9%	44.4%	27.0%	44.4%	28.0%	44.9%	0.99	0.63	0.63
Born in the South	26.0%	43.9%	27.6%	44.7%	26.1%	44.0%	0.35	0.94	0.48
Born in the West	20.7%	40.5%	19.4%	39.5%	17.4%	38.0%	0.40	0.07*	0.28
White	81.4%	38.9%	78.8%	40.9%	82.3%	38.2%	0.09*	0.62	0.06*
Black	8.9%	28.5%	11.2%	31.5%	9.1%	28.8%	0.05*	0.85	0.14
Married	61.3%	48.7%	59.6%	49.1%	62.5%	48.5%	0.36	0.61	0.20
Divorced or Separated	17.4%	37.9%	17.5%	38.0%	15.0%	35.8%	0.96	0.16	0.15
Never Married	16.9%	37.5%	18.2%	38.6%	18.1%	38.5%	0.39	0.51	0.95
College Graduate	51.9%	50.0%	52.2%	50.0%	51.7%	50.0%	0.85	0.94	0.81
Some College	25.6%	43.7%	25.5%	43.6%	27.5%	44.7%	0.96	0.35	0.33
High School	19.0%	39.3%	16.9%	37.5%	17.0%	37.6%	0.14	0.25	0.94
Working	61.9%	48.6%	56.7%	49.6%	62.6%	48.4%	0.01***	0.73	0.01***
Unemployed	10.8%	31.1%	11.3%	31.7%	9.8%	29.8%	0.69	0.48	0.30
Disabled	6.5%	24.7%	8.0%	27.1%	7.0%	25.6%	0.14	0.65	0.43
Characteristics of the Household									
Resides in the Midwest	21.6%	41.2%	23.4%	42.3%	22.9%	42.0%	0.28	0.52	0.80
Resides in the South	34.5%	47.6%	32.0%	46.7%	34.4%	47.5%	0.17	0.98	0.27
Resides in the West	28.4%	45.1%	28.9%	45.3%	26.4%	44.1%	0.78	0.34	0.24
Lives Alone	44.8%	49.7%	47.5%	50.0%	46.1%	49.9%	0.17	0.58	0.55
Household Size	2.2	1.5	2.2	1.5	2.2	1.5	0.40	0.94	0.44
$30,000 <= Income Last 12 Months < $50,000	22.0%	41.5%	23.2%	42.2%	26.5%	44.2%	0.48	0.02**	0.09*
$50,000 <= Income Last 12 Months < $75,000	20.7%	40.5%	21.0%	40.7%	20.8%	40.6%	0.86	0.95	0.93
Income Last 12 Months >= $75,000	27.7%	44.8%	26.7%	44.2%	28.2%	45.0%	0.55	0.79	0.45
# Observations	1,340		1,317		712				

Note: Column (1), (3) and (5) report separate means for the control and treatment groups. Columns (2), (4) and (6) report the standard deviations. The last three columns report the p-value of two-ways tests of the equality of the means across the groups.

Table 2. Treatment Effect by Interest Rate and Time Frame

	(1)	(2)	(3)	Test Differences Across Interest Rates	
	Monthly Interest Rates				
Dependent Variable: Later Reward	0.5%	1%	2.5%	Test	P-value
Panel A: Today x In 1 Month					
Treatment	26.65	16.59	8.82	(1) = (2)	0.258
	[6.63]***	[5.92]***	[5.12]*	(1) = (3)	0.033
Constant	315.96	361.47	414.43	(2) = (3)	0.321
	[4.85]***	[4.36]***	[3.74]***		
Observations	2,655	2,654	2,654		
Panel B: In 1 Year x In 1 Year and 1 Month					
Treatment	30.75	13.95	5.49	(1) = (2)	0.069
	[6.78]***	[6.28]**	[5.49]	(1) = (3)	0.004
Constant	304.21	350.25	406.71	(2) = (3)	0.310
	[4.90]***	[4.55]***	[3.91]***		
Observations	2,652	2,652	2,652		
P-value Test Differences Across Time Frames	0.666	0.760	0.657		

Note : Robust standard errors in brackets. The table reports OLS estimates of the treatment effect on the later reward for different monthly interest rates and time frames. The later reward (LR) is the amount in dollars of the second check. Respondents were asked to make six intertemporal choices, in which we varied the monthly interest rate (0.5%, 1% and 2.5%) and the time frame. Panel A shows results for the intertemporal choices made when the check mailing dates were "today" and "in 1 month". Panel B reports results for choices made when the mailing dates were "in 1 year" and "in 1 year and 1 month." The monthly interest varies across columns. The order of the time frames was randomized. Given a time frame, the interest rate increased monotonically.

Table 3. Treatment Effects

	OLS				
	LR	LR = 0	LR = 502.5	LR (Tobit)	LR (Trimmed)
Panel A: No Controls					
Treatment	30.75	-0.04	0.06	50.03	49.72
	[6.78]***	[0.01]***	[0.02]***	[10.93]***	(10.78)***
Constant	304.21	0.14	0.25	326.31	270.33
	[4.90]***	[0.01]***	[0.01]***	[7.72]***	(7.57)***
Observations	2,652	2,652	2,652	2,652	1,459
Panel B: With Controls					
Treatment	30.90	-0.04	0.05	50.22	-
	[6.79]***	[0.01]***	[0.02]***	[10.90]***	
Constant	326.18	0.00	0.19	361.08	-
	[26.97]***	[0.04]	[0.07]***	[42.07]***	
Observations	2,652	2,652	2,652	2,652	-

Note : Robust standard errors in brackets. Bootstrapped standard errors in parentheses. The first column reports ordinary least least square results. The dependent variable is the later reward (LR), the amount in dollars of the second check. The mailing dates of the 1st and 2nd checks were 1 year and 1 year and 1 month, respectively. The monthly interest rate was 0.5%. The dependent variables in the second and third columns are indicators for whether the later reward was $0 or $502.5, respectively. The last two columns report results from models that take into account that the later reward is censored at $0 and $502.5. Panel A report results without controls. Panel B reports results with controls (all variables presented in Table 1).

Table 4. Understanding of the Snacks Study

Category	%	Example
instant gratification*	17.9%	"People like instant gratrification."
people have good intentions but don't act as intended*	12.0%	"Many people have good intentions about the future, but make different (or not as wise) decisions."
I chose apples/doritos because ….	9.6%	"I chose Doritos because they cost more than an apple."
people make bad choices for now	9.2%	" When asked to make a choice that will affect them in the present they often pick the one that is the least beneficial"
people make good choices for the future	8.2%	"when people are asked to make a choice that will affect them in the future they usually pick the "best" choice."
different decisions for now and for later*	7.7%	“The decisions people make for the future are not the same ones they make for today.”
people should eat healthier	6.4%	"Eat healthy!"
temptation*	5.1%	"most people can not resist temptation"
people prefer junk food	4.6%	"People fundamentally like junk food"
I will delay if the interest is high enough	4.1%	“as long as the money offered can be delayed with better than average returns - I'm very happy to delay it”
people change their minds	3.6%	"that people do change their minds."
apples are healthier than doritos	3.2%	"an apple a day keeps the doctor away"
save	2.8%	“the more you save the more you have when its retirement.”
people should plan for the future	2.6%	"Think about the future!"
people decide without thinking	2.3%	"People make bad choices without thinking about it."
impulsivity*	2.3%	"People act on impulse"
I can wait	2.2%	“I learned that I do Have Patience To wait for extra cash”
people should delay gratification	2.2%	"it is better to delay gratification"
people cannot delay gratification	2.1%	"Most people cannot delay gratification."
people should save	1.9%	"Save your money for a rainy day."
people should stick to their plans	1.8%	"It is important to stick with plans in life"
choices are circumstancial	1.6%	“Then too, it depends on your mood at the time.”
self-control*	1.5%	"People have no self control."
choices depend on whether one needs cash	1.4%	"What is saved or earned depends on each persons financial situation at the time."
present bias*	1.1%	“people suffer of present bias”
procrastination*	0.9%	"people tend to put off what should be done now, not tomorrow."
patience	0.8%	“I learned that I do Have Patience To wait for extra cash and some people do not.”
people prefer now than later*	0.5%	“people want more now rather than later”
people overestimate their self-control*	0.5%	"People like to imagine themselves as more self disciplined than they are"
people prefer sooner than later	0.4%	"Most people like to receive things sooner than later."
willpower*	0.4%	"people have lofty ambitions but low willpower"
people live in the moment	0.2%	"People tend to live in the present"
Non-missing Observations	856	

Note: The table shows the distribution of responses to the following open-ended question: "*Did you learn something from the Apples or Doritos real-life study? Could you explain* in your own words what you learned?” The answers to these questions were first reviewed to determine categories that could characterize the answers and then coded into the created categories. The categories are not exclusive, such that a response from one respondent may be coded into more than one category. The third colum shows verbatim examples of typical answers. The categories marked with * denote the categories that are associated with having understood the experiment information.

Table 5. Later Reward and Understanding of the Snacks Study

	Panel A: Treatment Effect and the Predicted Probability of Understanding Dependent Variable: Later Reward			
	Model 1		Model 2	
	OLS	Tobit	OLS	Tobit
Predicted Probability * Treatment	134.96	274.44	125.02	247.43
	[60.88]**	[100.27]***	[57.46]**	[99.55]**
Treatment	-3.26	-17.87	-4.71	-14.36
	[15.55]	[24.31]	[15.45]	[25.21]
Predicted Probability of Understanding	-35.85	-19.13	-50.32	-51.24
	[43.77]	[70.57]	[40.66]	[69.17]
Constant	313.33	331.08	320.34	345.81
	[11.03]***	[17.13]***	[10.86]***	[17.53]***
Observations	2,652	2,652	2,117	2,117

	Panel B: Modeling the Probability of Understanding Among the Treatment Dependent Variable: Understands the Experiment Information (Multinomial Logit)			
	Model 1		Model 2	
	Coefficient	Standard Error	Coefficient	Standard Error
Financial Literacy				
Compound Interest			0.38	[0.21]*
Risk Diversification			0.76	[0.24]***
Inflation			0.37	[0.25]
Controls from Table 1				
Male	0.04	[0.15]	-0.22	[0.17]
Age	0.22	[0.23]	0.25	[0.27]
Year of Birth	0.25	[0.23]	0.29	[0.28]
Month of Birth	0.05	[0.03]	0.06	[0.04]
Birth Day	0.00	[0.01]	0.00	[0.01]
Born in US	-0.26	[0.35]	-0.08	[0.40]
Born in the Midwest	0.31	[0.31]	0.31	[0.33]
Born in the South	-0.10	[0.30]	0.04	[0.33]
Born in the West	0.28	[0.33]	0.26	[0.36]
White	0.77	[0.31]**	0.39	[0.37]
Black	0.28	[0.39]	-0.04	[0.47]
Married	-0.41	[0.37]	-0.72	[0.42]*
Divorced or Separated	-0.54	[0.40]	-0.76	[0.45]*
Never Married	-0.25	[0.42]	-0.41	[0.48]
College Graduate	0.97	[0.48]**	0.03	[0.57]
Some College	0.64	[0.49]	-0.09	[0.57]
High School	0.56	[0.50]	0.10	[0.59]
Working	-0.34	[0.19]*	-0.34	[0.22]
Unemployed	-0.78	[0.30]**	-0.34	[0.35]
Disabled	-0.50	[0.36]	-0.25	[0.43]
Resides in the Midwest	0.07	[0.33]	0.10	[0.36]
Resides in the South	0.27	[0.30]	0.27	[0.33]
Resides in the West	0.11	[0.31]	0.23	[0.34]
Lives Alone	-0.49	[0.24]**	-0.46	[0.28]*
Household Size	-0.12	[0.09]	-0.09	[0.10]
$30,000 <= Income Last 12 Months < $50,000	0.73	[0.23]***	0.97	[0.27]***
$50,000 <= Income Last 12 Months < $75,000	0.36	[0.24]	0.48	[0.28]*
Income Last 12 Months >= $75,000	1.01	[0.25]***	1.20	[0.30]***
Observations	1,314		1,044	

Note: Robust standard errors in brackets. The table shows how the treatment effect depends on the predicted probability of understanding the Snacks Study. Panel B reports the results from a multinomial logit regression of understanding the experiment information on baseline characteristics (the third outcome is not having answered). In model 1 the probability is predicted using the control variables from Table 1 . Model 2 uses these variables plus answers to three financial literacy questions asked in a previous survey module of the ALP: "*Suppose you had $100 in a savings account and the interest rate is 20% per year and you never withdraw money or interest payments. After 5 years. how much would you have in this account in total*?"; "*Imagine that there interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would be able to buy with the money in this account?*" and "*When a investor spreads his money among different assets, does the risk of losing money increase, decrease or stay the same?*". These estimated coefficients were then used to estimate the predicted probability of understanding. Panel A report results from OLS and Tobit regressions of the later reward on the predicted probability, an interaction of the predicted probability with treatment status, and the treatment status indicator.

Table 6. Experimenter Demand Effects

	LR (OLS)	LR = 0 (OLS)	LR = 502.5 (OLS)	LR (Tobit)	LR (Trimmed)
Panel A: No Controls					
Effect of Screen #7	8.06	0.00	0.01	11.08	14.59
	[9.65]	[0.02]	[0.02]	[15.86]	(12.58)
Constant (6 Screens)	307.01	0.12	0.28	340.3	268.91
	[6.67]***	[0.01]***	[0.02]***	[11.04]***	(8.80)***
Observations	1,383	1,383	1,383	1,383	808
Panel B: With Controls					
Effect of Screen #7	8.36	0.00	0.01	11.65	-
	[9.71]	[0.02]	[0.02]	[15.89]	
Constant (6 Screens)	334.04	-0.02	0.25	389.71	-
	[37.96]***	[0.06]	[0.10]**	[61.20]***	
Observations	1,383	1,383	1,383	1,383	-
	P-value Test Difference Between Effect of Screen #7 and Benchmark Treatment Effect Estimates (Table 3)				
Panel A	0.015	0.014	0.061	0.009	
Panel B	0.012	0.010	0.051	0.007	

Note : Robust standard errors in brackets. Bootstrapped standard errors in parentheses. The table estimates the effect of seeing screen #7 on the later reward. The sample is restricted to participants who were randomly selected into treatment and to receive first the "today frame" (the mailing date of first check), all of whom saw at least 6 screens. Approximately half of this sample was randomly selected to see a 7th screen, which would potentially induce experimenter demand effects. The estimated results are from regressions of some measure of the later reward on a constant (corresponding to the mean for the group who saw 6 screens only) and an indicator variable identifying respondents who saw the 7th screen . See footnote of Table 3 for an explanation of the difference across columns. Panel A report results without controls. Panel B reports results with controls (all variables presented in Table 1).

The Appendix

is not intended

for publication.

Information Screens Seen by Treatment Group

(the control group did not see any of these screens)

Imagine you were told that free snacks would be given away. The free snacks would be given to you **one week from today**, but you have to choose **today** which snack you would like: an apple or a bag of Doritos. Which one would you choose?

- An apple
- A bag of Doritos

OR

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RAND American Life Panel

How about if you were told that you could have your free snack **now**? If the options were either an apple or a bag of Doritos, which one would you choose?

- An apple
- A bag of Doritos

OR

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RAND American Life Panel

The questions you were just asked were based in a real-life research study.

In the study participants were visited at their workplace and told that free snacks would be given away at their workplace **one week later**.

They were asked to choose between healthy (an apple) and unhealthy snacks (Doritos), and informed that their choice would be delivered **one week later**.

5 out of 10 people chose a healthy snack **one week later**.

Which snack people chose to have one week later?

Healthy	Unhealthy
	

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RAND American Life Panel

One week later after the first visit the same participants were revisited at their workplace.

Snacks were being given away, and the study participants were asked to choose between healthy (an apple) and unhealthy snacks (Doritos), which they could have **right away**.

Only 2 out of 10 people chose a healthy snack **immediately**.

Which snack people chose to have right away?

Healthy	Unhealthy
	

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RAND American Life Panel

Why did some people change their minds? What was the study trying to show?

When people make plans for the future, they often end up not doing what they had originally planned.

When asked to choose which snack they would like **next week**, half of the people chose the healthy snack (the apple) because they care about their health.

When asked what they would prefer **right now**, 8 out of 10 people chose the unhealthy snack because they could not resist the temptation of having the unhealthy snack (the Doritos).

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This behavior is not limited to eating. It also applies to other behaviors such as exercising, dieting (I'll start on Monday), quitting smoking and saving.

In the case of savings, someone may recognize the importance of saving for retirement, and plan on stepping up her savings starting with the **next paycheck**.

But when the next paycheck arrives and it is time to start saving more, the person faces a dilemma: she wants to save more, **but saving more requires spending less**.

She cannot resist the temptation to consume and ends up saving less than originally planned.

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RAND American Life Panel

Information Screen #7

This screen was seen by the first treatment arm only (the second treatment arm did not see it).

When thinking about how much they want to be saving in the months to come, people are *foresighted* and plan to save more. But when they have to decide how much to save this month, they are *shortsighted* and save less than planned.

This type of behavior is often referred to as *present biased*.

The term refers to the phenomenon that people tend to choose whatever gives them *immediate* satisfaction (to spend), even though this is not what they think is good for their future (to save more).

Present bias is often seen as undesirable because what people end up doing (saving less than planned) is not what they think is best for their future (saving for retirement).

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RAND American Life Panel

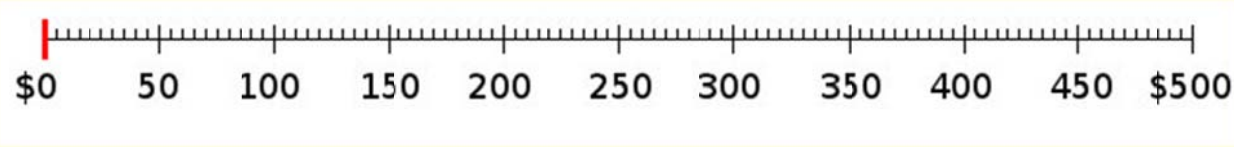
Intertemporal Choice Task

This is the screen in which respondents entered their intertemporal choices.

Imagine you were given the following information about your prize:

- the 1st check will be sent to you in 1 year from today
- the 2nd check will be sent to you 1 month after the 1st check
- a 0.5% monthly interest rate (corresponding to 6.2% annual interest rate) will be paid on the amount you save for the 2nd check

How much do you want to save for the 2nd check?



You can also enter your answer here: $ 0 .00 update

Please make your decision carefully. This may be the decision for which you will be paid for.

1st Check

2nd Check

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RAND American Life Panel

Appendix: Construction of Controls and Other Covariates

Table 1 Controls: All respondents of the RAND American Life Panel (ALP) answer a special module that collects basic socioeconomic and demographic data, namely: gender, age, date of birth, race, born in US, state of birth, state of residence, living situation (i.e., never married, married, widowed, divorced or separated), household income and household size, schooling, disability and employment status.

Financial Literacy: The financial literacy questions were asked in module ms189 of the ALP. Based on these questions, we created indicator variables identifying respondents who correctly answered the questions. The questions were the following:

(Compound Interest) *Suppose you had $100 in a savings account and the interest rate is 20% per year and you never withdraw money or interest payments. After 5 years, how much would you have in this account in total?*

- More than $200
- Exactly $200
- Less than $200
- I don't know

(Inflation) *Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account?*

- More than today
- Exactly the same
- Less than today
- I don't know

(Risk Diversification) *When an investor spreads his money among different assets, does the risk of losing money:*

- Increase
- Decrease
- Stay the same
- I don't know

Appendix Table 1: Summary Statistics and Balance Check (Weighted Estimates)

	Control		Treatment Arm #1		Treatment Arm #2				
	(1)	(2)	(3)	(4)	(5)	(6)	**Test (1) = (3)**	**Test (1) = (5)**	**Test (3) = (5)**
	Means	SD	Means	SD	Means	SD	P-value	P-value	P-value
Individual Characteristics									
Male	48.6%	50.0%	50.8%	50.0%	45.5%	49.8%	0.37	0.30	0.076*
Age	46.6	16.3	46.5	17.0	45.8	15.7	0.91	0.43	0.52
Year of Birth	1964.0	16.3	1964.2	17.0	1964.8	15.7	0.88	0.45	0.55
Month of Birth	6.6	3.4	6.4	3.4	6.8	3.4	0.16	0.47	0.065*
Birth Day	15.4	8.9	15.3	8.8	15.8	9.0	0.83	0.47	0.37
Born in US	92.5%	26.4%	91.1%	28.4%	90.9%	28.8%	0.33	0.37	0.89
Born in the Midwest	25.4%	43.6%	25.2%	43.4%	24.8%	43.2%	0.92	0.79	0.86
Born in the South	27.2%	44.5%	27.3%	44.6%	27.1%	44.5%	0.95	0.98	0.94
Born in the West	21.8%	41.3%	19.7%	39.8%	17.6%	38.1%	0.32	0.086*	0.38
White	78.9%	40.9%	74.9%	43.4%	78.8%	40.9%	0.066*	0.98	0.14
Black	10.0%	30.0%	12.4%	33.0%	8.4%	27.8%	0.14	0.36	0.023**
Married	63.4%	48.2%	62.3%	48.5%	67.2%	47.0%	0.65	0.16	0.077*
Divorced or Separated	13.5%	34.2%	13.9%	34.7%	11.1%	31.4%	0.78	0.14	0.090*
Never Married	18.5%	38.8%	19.8%	39.9%	17.4%	37.9%	0.52	0.62	0.30
College Graduate	36.3%	48.1%	36.1%	48.1%	37.7%	48.5%	0.93	0.58	0.54
Some College	18.7%	39.0%	18.4%	38.8%	19.6%	39.7%	0.83	0.65	0.53
High School	38.3%	48.6%	35.0%	47.7%	34.5%	47.6%	0.22	0.23	0.87
Working	60.1%	49.0%	57.3%	49.5%	61.8%	48.6%	0.26	0.55	0.13
Unemployed	11.2%	31.6%	11.8%	32.3%	10.9%	31.2%	0.71	0.88	0.65
Disabled	6.6%	24.8%	8.8%	28.3%	6.6%	24.8%	0.10	1.00	0.15
Characteristics of the Household									
Resides in the Midwest	22.0%	41.4%	21.7%	41.2%	21.0%	40.8%	0.90	0.69	0.78
Resides in the South	35.1%	47.8%	32.4%	46.8%	34.6%	47.6%	0.24	0.86	0.43
Resides in the West	28.0%	44.9%	27.8%	44.8%	26.2%	44.0%	0.93	0.50	0.54
Lives Alone	39.0%	48.8%	38.9%	48.8%	41.3%	49.3%	0.98	0.41	0.40
Household Size	2.4	1.5	2.4	1.6	2.5	1.8	0.88	0.49	0.56
$30,000 <= Income Last 12 Months < $50,000	22.0%	41.4%	23.0%	42.1%	23.4%	42.4%	0.61	0.56	0.89
$50,000 <= Income Last 12 Months < $75,000	17.2%	37.8%	19.6%	39.7%	19.8%	39.9%	0.20	0.25	0.92
Income Last 12 Months >= $75,000	28.5%	45.2%	27.9%	44.9%	31.0%	46.3%	0.78	0.35	0.25
# Observations	1,340		1,317		712				

Note : The summary statistics are estimated using post-stratification weights designed for estimating nationally representative results. Column (1), (3) and (5) report separate means for the control and treatment groups. Columns (2), (4) and (6) report the standard deviations. The last three columns report the p-value of two-ways tests of the equality of the means across the groups.

Appendix Table 2. Treatment Effect by Interest Rate and Time Frame (Weighted Estimates)

	(1)	(2)	(3)	Test Differences Across Interest Rates	
	Monthly Interest Rates				
	0.5%	1%	2.5%	Test	P-value
Panel A: Today x In 1 Month					
Treatment	23.62	15.36	9.64	(1) = (2)	0.474
	[8.41]***	[7.89]*	[7.00]	(1) = (3)	0.201
Constant	314.14	354.05	404.73	(2) = (3)	0.587
	[5.90]***	[5.43]***	[5.00]***		
Observations	2,655	2,654	2,654		
Panel B: In 1 Year x In 1 Year and 1 Month					
Treatment	21.45	4.61	3.54	(1) = (2)	0.152
	[8.55]**	[8.06]	[7.40]	(1) = (3)	0.113
Constant	308.61	349.65	399.77	(2) = (3)	0.922
	[5.90]***	[5.40]***	[4.94]***		
Observations	2,652	2,652	2,652		
P-value Test Differences Across Time Frames	0.857	0.340	0.549		

Note : The results are estimated using post-stratification weights designed for estimating nationally representative results. Robust standard errors in brackets. The table reports OLS estimates of the treatment effect on the later reward for different monthly interest rates and time frames. The later reward (LR) is the amount in dollars of the second check. Respondents were asked to make six intertemporal choices, in which we varied the monthly interest rate (0.5%, 1% and 2.5%) and the time frame. Panel A shows results for the intertemporal choices made when the check mailing dates were "today" and "in 1 month". Panel B reports results for choices made when the mailing dates were "in 1 year" and "in 1 year and 1 month." The monthly interest varies across columns. The order of the time frames was randomized. Given a time frame, the interest rate increased monotonically.

Appendix Table 3. Treatment Effects (Weighted Estimates)

		OLS		
	LR	LR = 0	LR = 502.5	LR (Tobit)
Panel A: No Controls				
Treatment	21.45	-0.03	0.04	36.92
	[8.55]**	[0.02]*	[0.02]**	[13.30]***
Constant	308.61	0.13	0.24	329.23
	[5.90]***	[0.01]***	[0.01]***	[9.00]***
Observations	2,652	2,652	2,652	2,652
Panel B: With Controls				
Treatment	20.93	-0.03	0.04	36.9
	[8.55]**	[0.02]*	[0.02]**	[13.27]***
Constant	318.06	0.04	0.12	333.22
	[32.92]***	[0.06]	[0.07]	[48.70]***
Observations	2,652	2,652	2,652	2,652

Note: The results are estimated using post-stratification weights designed for estimating nationally representative results. Robust standard errors in brackets. Bootstrapped standard errors in parentheses. The first column reports ordinary least least square results. The dependent variable is the later reward (LR), the amount in dollars of the second check. The mailing dates of the 1st and 2nd checks were 1 year and 1 year and 1 month, respectively. The monthly interest rate was 0.5%. The dependent variables in the second and third columns are indicators for whether the later reward was $0 or $502.5, respectively. The last two columns report results from models that take into account that the later reward is censored at $0 and $502.5. Panel A report results without controls. Panel B reports results with controls (all variables presented in Table 1).

Appendix Table 4. Not Answering the Question on Learning from Experiment Information

	Modeling the Probability of Not Answering Among the Treatment Dependent Variable: Not Answering (Multinomial Logit)			
	Model 1		Model 2	
	Coefficient	Standard Error	Coefficient	Standard Error
Financial Literacy				
Compound Interest			0.10	[0.18]
Risk Diversification			0.15	[0.20]
Inflation			-0.28	[0.20]
Controls from Table 1				
Male	-0.06	[0.14]	0.01	[0.16]
Age	0.24	[0.21]	0.27	[0.25]
Year of Birth	0.27	[0.21]	0.30	[0.25]
Month of Birth	0.05	[0.03]	0.06	[0.04]*
Birth Day	0.00	[0.01]	0.00	[0.01]
Born in US	0.36	[0.32]	0.24	[0.37]
Born in the Midwest	-0.30	[0.29]	-0.28	[0.31]
Born in the South	-0.58	[0.26]**	-0.35	[0.29]
Born in the West	-0.49	[0.30]	-0.60	[0.34]*
White	0.01	[0.24]	-0.15	[0.29]
Black	0.06	[0.29]	-0.20	[0.36]
Married	-0.75	[0.35]**	-1.17	[0.42]***
Divorced or Separated	-0.76	[0.37]**	-1.12	[0.43]***
Never Married	-0.55	[0.39]	-0.80	[0.46]*
College Graduate	-0.35	[0.30]	-0.62	[0.39]
Some College	-0.19	[0.30]	-0.43	[0.39]
High School	-0.08	[0.31]	-0.12	[0.39]
Working	-0.06	[0.18]	0.05	[0.21]
Unemployed	-0.37	[0.25]	-0.20	[0.30]
Disabled	0.15	[0.26]	0.55	[0.31]*
Resides in the Midwest	0.39	[0.31]	0.62	[0.35]*
Resides in the South	0.90	[0.27]***	0.83	[0.31]***
Resides in the West	0.32	[0.29]	0.49	[0.34]
Lives Alone	-0.29	[0.21]	-0.06	[0.25]
Household Size	0.04	[0.07]	0.10	[0.09]
$30,000 <= Income Last 12 Months < $50,000	0.26	[0.20]	0.48	[0.24]**
$50,000 <= Income Last 12 Months < $75,000	0.07	[0.21]	0.25	[0.24]
Income Last 12 Months >= $75,000	0.65	[0.22]***	0.86	[0.27]***
Constant	-542.24	[417.65]	-601.30	[497.99]
Observations	1,314		1,044	

Note: Robust standard errors in brackets. The table reports the results from a multinomial logit regression of whether the participant answered the following question: *"Did you learn something from the Apples or Doritos real-life study? Could you explain in your own words what you learned*?". In model 1 the probability is predicted using the control variables from Table 1 . Model 2 uses these variables plus answers to three financial literacy questions asked in a previous survey module of the ALP: "*Suppose you had $100 in a savings account and the interest rate is 20% per year and you never withdraw money or interest payments. After 5 years. how much would you have in this account in total*?"; "*Imagine that there interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would be able to buy with the money in this account*?" and "*When a investor spreads his money among different assets, does the risk of losing money increase, decrease or stay the same*?".

Appendix: Trimming Estimator

Here we derive an estimator that uses the trimming procedure proposed by David Lee (2009) for estimating treatment effects in the presence of sample selection. We are interested in estimating the impact of the treatment on the later reward (henceforth, LR).

One issue, however, is that the LR is censored. In the intertemporal choice task participants could not borrow nor lend at the experimental interest rate. Consequently, the choice of later reward is left censored at \$0 for participants who would have liked to borrow (i.e., a sooner reward greater than \$500). Similarly, the choice of later reward is right censored at \$500 times the gross interest rate for participants would have liked to lend (i.e., a sooner reward smaller than \$0). For ease of exposition we will assume that the interest is equal to zero.

Let LR^* be the latent LR that participants would have chosen if they could lend and borrow at the experimental interest rate:

$$LR^* = \alpha + D\beta + U,$$

where α is a constant, D is an indicator variable of receiving treatment and U is an unobservable component. β is the treatment effect of interest. The observed LR is censored at \$0 and \$500:

$$LR = \begin{cases} LR^* & \text{if } 500 > LR^* > 0, \\ 0 & \text{if } LR^* \leqslant 0, \\ 500 & \text{if } LR^* \geqslant 500. \end{cases}$$

We cannot estimate β by simple comparing the average LR of treatment and control for those who choose $LR^* \epsilon (0, 500)$ because the treatment affects the chances of one choosing either \$0 or \$500 (in other words, the treatment and control have different unobservables). The trimming procedure trimms the control and treatment distributions such that treatment and control have the same distribution of unobservables.

We will assume that $\beta > 0$. Suppose we could estimate the following:

$$E\left[LR|D = 1, 500 - \alpha - \beta > U > -\alpha\right] = \alpha + \beta + E\left[U|500 - \alpha - \beta > U > -\alpha\right].$$

and

$$E\left[LR|D = 0, 500 - \alpha - \beta > U > -\alpha\right] = \alpha + \quad + E\left[U|500 - \alpha - \beta > U > -\alpha\right].$$

The subraction of the two terms above would identify β:

$$E\left[LR|D = 1, 500 - \alpha - \beta > U > -\alpha\right] - E\left[LR|D = 0, 500 - \alpha - \beta > U > -\alpha\right] = \beta.$$

Thus, our goal will be to estimate the average LR for this target group of individuals for whom $500 - \alpha - \beta > U > -\alpha$.

For the treatment group, the necessary and sufficient condition for an interior solution is:

$$500 > LR^* > 0 | D = 1 \Leftrightarrow 500 - \alpha - \beta > U > -\alpha - \beta.$$

Notice that our target group $(500 - \alpha - \beta > U > -\alpha)$ corresponds to the p-th upper tail of the treatment distribution, where

$$p = \frac{\Pr\left(500 - \alpha - \beta > U > -\alpha\right)}{\Pr\left(500 - \alpha - \beta > U > -\alpha - \beta\right)}. \tag{1}$$

[APPENDIX FIGURE 1 HERE]

Thus, in order to restrict the sample to our target group, we will trim the 1-pth lower tail of the treatment distribution.

Similarly, the necessary and sufficient condition for an interior solution for the control group is:

$$500 > LR^* > 0 | D = 0 \Leftrightarrow 500 - \alpha > U > -\alpha.$$

Notice that our target group $(500 - \alpha - \beta > U > -\alpha)$ corresponds to the q-th lower tail of the control, where

$$q = \frac{\Pr\left(500 - \alpha - \beta > U > -\alpha\right)}{\Pr\left(500 - \alpha > U > -\alpha\right)}. \tag{2}$$

[APPENDIX FIGURE 2 HERE]

Therefore, in order to restrict the sample to our target group, we will trim the 1-qth upper tail of the control distribution.

Finally, we want to estimate the proportions p and q. We have:

$$\Pr\left(LR < 500 | D = 1\right) = \Pr\left(U < 500 - \alpha - \beta\right) = \Pr\left(500 - \alpha - \beta > U > -\alpha\right) + \Pr\left(U < -\alpha\right),$$

and

$$\Pr\left(LR > 0 | D = 0\right) = \Pr\left(U > -\alpha\right),$$

such that

$$\Pr\left(LR < 500 | D = 1\right) + \Pr\left(LR > 0 | D = 0\right) = 1 + \Pr\left(500 - \alpha - \beta > U > -\alpha\right). \tag{3}$$

[APPENDIX FIGURE 3 HERE]

Rewriting (3), we get the mass distribution of the target group:

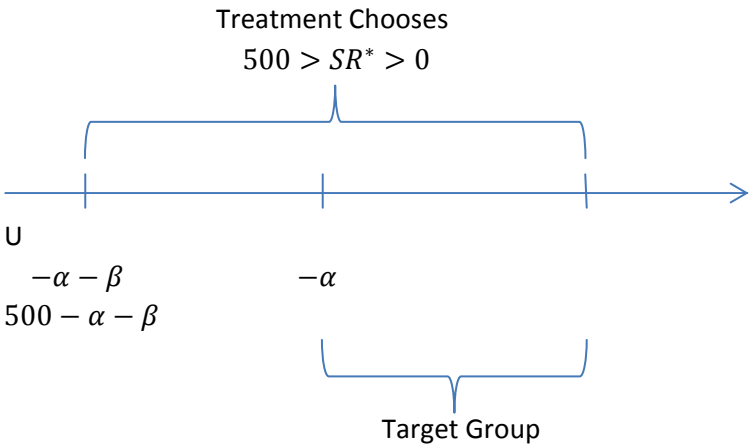
$$\Pr\left(500 - \alpha - \beta > U > -\alpha\right) = \Pr\left(LR < 500 | D = 1\right) + \Pr\left(LR > 0 | D = 0\right) - 1.$$

Substituing (3) in (1) and (2), we obtain:

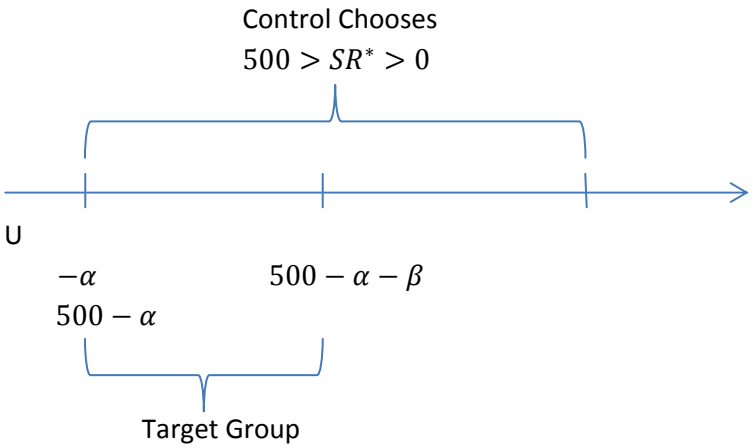
$$p = \frac{\Pr\left(LR < 500 | D = 1\right) + \Pr\left(LR > 0 | D = 0\right) - 1}{\Pr\left(500 > LR > 0 | D = 1\right)}$$

$$q = \frac{\Pr\left(LR < 500 | D = 1\right) + \Pr\left(LR > 0 | D = 0\right) - 1}{\Pr\left(500 > LR > 0 | D = 0\right)}.$$

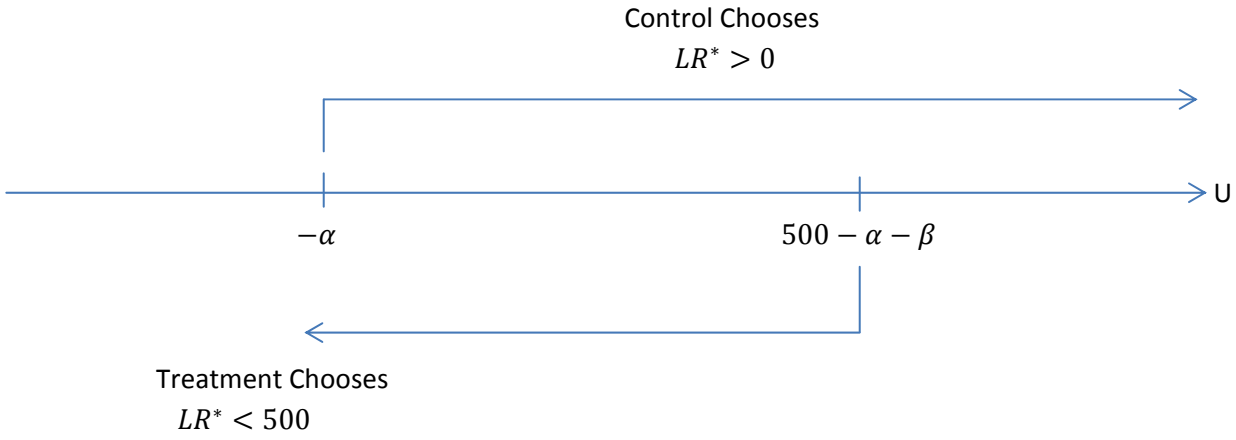
APPENDIX FIGURE 1



APPENDIX FIGURE 2



APPENDIX FIGURE 3



Appendix: Model

The model considers the decision participants face of intertemporally allocating an experimental budget of $500 prize between periods *t* and *t+1*. The (experiment) budget constraint the agent faces is:

$$SR_t + \frac{LR_{t+1}}{1+r_e} = 500 \quad \text{and} \quad 500 \geq \frac{LR_{t+1}}{1+r_e} \geq 0, \tag{A1}$$

where SR (sooner reward) is the dollar amount of the first check, LR (later reward) is the dollar amount of the second check, and r_e is the experimental interest rate.

Following Andersen et al (2008) and Andreoni and Sprenger (forthcoming), the model assumes that the experimental rewards are integrated with background consumption; the sooner reward is integrated with period *t* background consumption c_t^* while the later reward is integrated with period *t*+1 background consumption c_{t+1}^*:

$$\max_{SR,LR} u(c_t^* + SR_t) + \beta^{I\{t=0\}}\delta u(c_{t+1}^* + LR_{t+1}) \quad \text{s.t. (A1)}, \tag{A2}$$

where β is the present bias. Notice that the present bias drops out if $t \neq 0$.

The solution to the problem is given by the following first order condition:

$$u'(c_t^* + [1+r_e][500 - LR_{t+1}^*]) = \beta^{I\{t=0\}}\delta[1+r_e]u'(c_{t+1}^* + LR_{t+1}^*). \tag{A3}$$

In allocating the experimental $500 budget, the individual must anticipate what her background consumption will be in the future. Her "future selves" will choose their background consumption by optimally allocating their "real-life" cash-on-hand x_t before allowing for the effects of the money offered in the experimental tasks.

Let's consider first the case in which the participant must decide how to allocate \$500 between today and one month later.[1] She assumes that her future selves will have present bias $\widehat{\beta}$ and that they will choose background consumption by maximizing their lifetime utility:

$$\max_{\{c_{t+k}\}_{k=0}^{T}} u(c_t) + \widehat{\beta} \sum_{k=1}^{T} \delta^k u(c_{t+k}) \qquad \forall t \geq 1 \qquad \text{(A4)}$$

subject to the following budget constraint:

$$x_{t+1} = [1 + r_m][x_t - c_t], \qquad \text{(A5)}$$

where x_t is the cash-on-hand in period t and r_m is the market interest rate. The current self assumes that her future selves will be "sophisticated"—i.e., they will be fully aware that they (supposedly) have present bias $\widehat{\beta}$.[2] These assumptions imply that the current self anticipates (incorrectly) that the background consumption choices of her future selves can be described by the Strong Euler Equation (Harris and Laibson 2001):

$$u'(c_t^*) = \delta[1 + r_m]\left\{\left[1 - \frac{\partial c_{t+1}}{\partial x_{t+1}}\right] + \widehat{\beta}\,\frac{\partial c_{t+1}}{\partial x_{t+1}}\right\} u'(c_{t+1}^*) \qquad \forall t \geq 1 \qquad \text{(A6)}$$

Finally, the current self chooses current background consumption by maximizing the following:

$$\max_{\{c_k\}_{k=0}^{T}} u(c_0) + \beta \sum_{k=1}^{T} \delta^k u(c_{0+k}) \text{ s.t. (A5) and (A6).} \qquad \text{(A7)}$$

Tobacman (2007) shows that the solution to (A7) is:

$$u'(c_0^*) = \delta[1 + r_m]\left\{\frac{\beta}{\widehat{\beta}}\left[1 - \frac{\partial c_1}{\partial x_1}\right] + \beta\,\frac{\partial c_1}{\partial x_1}\right\} u'(c_1^*). \qquad \text{(A8)}$$

(A6) and (A8) jointly determine the optimal current background consumption c_0^* and the optimal background consumption one month later c_1^*, which in turn determine LR_1^* in (A3).

[1] To match the delay time of the intertemporal task, we assume that individuals solve their background allocation problem on a monthly basis.

[2] Notice that in fact the future selves will be partly naïve because they will not recognize that their true present bias is β and not $\widehat{\beta}$.

Consider now the choice of how to allocate \$500 between 12 months and 13 months. In this case, the current self anticipates (incorrectly) that the background consumption choices of her current selves can be described by (A6), which determines c_{12}^* and c_{13}^* and consequently LR_{13}^*.

Notice that in this second case the true present bias β does not enter in neither (A3) nor (A6). Thus, the present bias can only affect LR_{13}^* through the intertemporal budget constraint (A5). A reduction in β increases current background consumption c_0^* in (A8). Consequently, there will be fewer resources for all future selves, since $x_1 = [1 + r_m][x_0 - c_0^*]$. However, this effect is small if the marginal propensity of consumption is sufficiently high (and the interest rate low). Let λ_t be the marginal propensity of consumption in period *t*, such that $c_t^* = \lambda_t x_t$. In this case, we have:

$$x_{12}=[x_0 - c_0^*]\{[1 + r_m]^{12} \prod_{k=1}^{11}[1 - \lambda_k]\}. \quad \text{(A9)}$$

The term between curly brackets converges quickly to zero it the marginal propensity of consumption is sufficiently high.

Therefore, the model predicts that LR_{13}^* is almost invariant to the true present bias β. Consequently, the effect of awareness on demand for commitment, such as commitment savings, is large relative to the effect of having self-control problems. This result contrasts with predictions of consumption models without a commitment device, in which the effect of awareness is small relative to the effect of being present biased (O'Donoghue and Rabin 2001b).